TRANSACTIONS

OF

THE SEVENTH SESSION

OF THE

AUSTRALASIAN MEDICAL CONGRESS.

ADELAIDE, SEPTEMBER, 1905.

AUSTRALASIAN MEDICAL CONGRESS

(Formerly the Intercolonial Medical Congress of Australasia).

TRANSACTIONS

OF

THE SEVENTH SESSION,

HELD AT

ADELAIDE, SEPTEMBER, 1905.

PUBLISHED UNDER THE DIRECTION OF THE EDITORIAL COMMITTEE.

South Australia:
C. E. BRISTOW, GOVERNMENT PRINTER, NORTH TERRACE, ADELAIDE.

1907.

35 159 564

PREFACE.

In presenting to members of the Australasian Medical Congress the Transactions of the Adelaide Session of 1905, the Editorial Committee regret the long delay that has attended its publication.

For this delay unusual pressure of work in the Government Printing Office and the prior claims of official work are chiefly responsible; but it must be confessed that the Committee have also found themselves hindered by the neglect of some authors to forward their papers at a sufficiently early date.

The Transactions embodied in this volume form a continuance of the series issued in previous years as the "Transactions of the Intercolonial Medical Congress of Australasia," and it is hoped that members will find it, like its predecessors, a faithful record of their principal proceedings.

The Committee desire to express their appreciation of the liberality of the Government of South Australia in defraying the cost of publication of the volume, and of the care taken in its production by the official printer (Mr. Bristow) and his staff.

E. C. STIRLING, *President.*

Adelaide, November, 1906.

CONTENTS.

Australasian Medical Congress.

Patrons :

His Excellency the Right Honorable the Lord Northcote, G.C.M.G., G.C.I.E., C.B., Governor-General of the Australian Commonwealth.

His Excellency the Right Honorable the Lord Tennyson, G.C.M.G., late Governor-General.

His Excellency Vice-Admiral Sir Harry Rawson, R.N., K.C.B., Governor of New South Wales

His Excellency Major-General the Honorable Sir Reginald Talbot, K.C.B., Governor of Victoria.

His Excellency Major-General Sir Herbert Chermside, R.E., G.C.M.G., C.B.. Governor of Queensland.

His Excellency Sir George R. Le Hunte, K.C.M.G., Governor of South Australia.

His Excellency Admiral Sir Frederick G. D. Bedford, R.N., G.C.B., Governor of Western Australia.

His Excellency Sir Gerald Strickland, K.C.M.G., Governor of Tasmania.

His Excellency the Right Honorable the Lord Plunket, K.C.V.O., Governor of New Zealand.

His Excellency Captain Barton, C.M.G., Administrator of British New Guinea.

His Honor the Right Honorable Sir S. J. Way, Bart., P.C., D.C.L., LL.D., Chief Justice and Lieutenant-Governor of South Australia.

President :

Professor E. C. Stirling, C.M.G., M.A., M.D., Cantab., F.R.C.S., Eng., F.R.S.

Retiring President :

Hon. G. H. Butler, M.R.C.S., L.R.C.P., Hobart.

Treasurer :

W. T. Hayward, M.R.C.S., &c.

General Secretary :

B. Poulton, M.D., Melb., M.R.C.S.

Associate Secretary :

J. B. Gunson, M.B., Ad., M.R.C.S.

Executive Committee:

The President
The Treasurer
The General Secretary
The Associate Secretary
T. Borthwick, M.D.
W. R. Cavenagh-Mainwaring, M.B., F.R.C.S.
W. L. Cleland, M.B.
T. W. Corbin, M.R.C.S.
W. Anstey Giles, M.B., Ch.M.
A. A. Hamilton, B.A., M.B.
J. A. G. Hamilton, B.A., M.B.
T. K. Hamilton, M.D.
Melville Jay, M.R.C.S., &c.
A. A. Lendon, M.D., Lond., M.R.C.S.
R. H. Marten, M.D., Cantab.
H. Swift, M.D., Cantab.
M. J. Symons, M.D., Edin.
C. E. Todd, M.D., M.R.C.S.
J. C. Verco, M.D., Lond., F.R.C.S.
Professor A. Watson, M.D., F.R.C.S., Etc.

Vice-Presidents of Congress:

J. C. Verco, M.D., Lond., F.R.C.S., President Adelaide Congress, 1887.

Sir T. N. Fitzgerald, C.B., F.R.C.S.I., President Melbourne Congress, 1889.

Sir P. Sydney Jones, M.D., Lond., F.R.C.S., President Sydney Congress, 1892.

F. C. Batchelor, M.D., Durh., M.R.C.S., Lond., L.R.C.P., Edin., President Dunedin Congress, 1896.

John Thomson, M.B., Ch.B., Edin., President Brisbane Congress, 1899.

Hon. G. H. Butler, M.R.C.S., L.R.C.P., President Hobart Congress, 1902.

Surgeon-General W. D. C. Williams, C.B., M.R.C.S., L.R.C.P., Director-General of the Army Medical Services of the Commonwealth, Melbourne.

Fleet Surgeon James O'B. Williams, M.D., Ch.M., Royal Univ. I., L.R.C.S.I., Senior Naval Medical Officer on the Australian Station, Sydney.

Officers of all Sessions of the Intercolonial Medical Congress of Australasia.

Adelaide, 1887.	Melbourne, 1889.	Sydney, 1892.	Dunedin, 1896.	Brisbane, 1899.	Hobart, 1902.
PRESIDENTS OF CONGRESS.					
J. C. Verco, M.D. (Lond.), F.R.C.S. (Eng.)	Sir T. N. Fitzgerald, C.B., F.R.C.S. (Irel.)	Sir P. Sydney Jones, M.D. (Lond.), F.R.C.S. (Eng.)	F. C. Batchelor, M D. (Durh.)	John Thomson, M.B., C.M. (Edin.)	The Hon. G. H. Butler, M.R.C.S. (Eng.), L.R.C.P. (Lond), M.L.C.
TREASURERS OF CONGRESS.					
W. T. Hayward H. E. Astles	G. Graham	T. Chambers	J. O. Closs	W. F. Taylor	J. E. Wolfhagen
SECRETARIES OF CONGRESS.					
B. Poulton	H. B. Allen J. W. Barrett G. A. Syme	T. P. Anderson Stuart S. T. Knaggs	J. H. Scott L. E. Barnett	Wilton Love	Gregory Sprott

PRESIDENTS OF SECTIONS.

Sections.	Adelaide, 1887.	Melbourne, 1889.	Sydney, 1892.	Dunedin, 1896.	Brisbane, 1899.	Hobart, 1902.
Medicine	John Williams	W. F. Taylor	James Robertson	A. A. Lendon	R. Scott Skirving	J. Jamieson
Surgery	T. N. Fitzgerald	E. C. Stirling	W. Gardner	H. M. O'Hara	A. McCormick	L. E. Barnett
Gynæcology	J. Foreman	F. C. Batchelor	W. Balls-Headley	E. W. Way	M. U. O'Sullivan	R. Worrall
State Medicine	H. T. Whittell	H. N. MacLaurin	H. T. Whittell	J. W. Springthorpe	J. Ashburton Thompson	T. Cherry
Anatomy	—	T. P. Anderson Stuart	H. B. Allen	A. Watson	R. H. Marten	J. H. Scott
Pathology	—	W. Camac Wilkinson	do.	do.	—	do.
Ophthalmology, &c.	—	M. J. Symons	—	H. M. O'Hara	H. Lindo Fergusson	T. K. Hamilton
Psychology	—	F. Norton Manning	—	—	—	—
Pharmacology	—	Baron von Mueller	H. B. Allen	A. Watson	—	—
Diseases of Skin	—	J. P. Ryan	—	A. A. Lendon	—	—
Diseases of Children	—	W. Snowball	—	—	—	—
Public Health	—	—	—	—	—	—

ROLL OF MEMBERS.

Abramowski, O. L. M., M.D., Mildura, Victoria.
Adam, Rothwell, M.D., C.M., Melbourne, Victoria.
Ahearne, Joseph, L.R.C.S., L.R.C.P., Townsville, Queensland.
Aitken, W. Blair, M.B., M.R.C.S., Jamestown, South Australia.
Allen, H. B., M.D., Melbourne, Victoria.
Allwork, F., L.S.A., New Norfolk, Tasmania.
Altmann, C. A., M.B., F.R.C.S. Edin., Melbourne, Victoria.
Ambrose, Ethel, M.B., B.S., Adelaide, South Australia.
Ambrose, T., M.B., B.S., Mount Magnet, Western Australia.
Anderson, A. V. M., M.D., B.S., St. Kilda, Victoria.
Anderson, G. M., M.B., C.M. Aberdeen, Tasmania.
Angove, W. T., M.R.C.S., Teatree Gully, South Australia.
Archer, E. L., M.B., Moonta, South Australia.
Armstrong, W. G., B.A., M.B., Ch.B. Edin., D.P.H. Cantab., Sydney, New South Wales.
Arthur, Richard, M.D. Edin., Sydney, New South Wales.
Asher, M., L.R.C.S., L.R.C.P., Lithgow, New South Wales.
Astles, H. E., M.D., F.R.C.P. Edin., Perth, Western Australia.

Bage, Charles, M.D., South Yarra, Victoria.
Barrett, J. W., M.D., F.R.C.S., Eng. Melbourne, Victoria.
Barrington, Fourness, M.B., M.S. Edin., F.R.C.S. Eng., Sydney, New South Wales.
Batchelor, F. L., M.D., M.R.C.S., Dunedin, New Zealand.
Beeston, J. L., L. et L. Mid., K.Q.C.P. and S. et L. Mid., R.C.S. Irel., L.M. Newcastle, New South Wales.
Benham, F. Lucas, M.D., Exeter, South Australia.
Bennett, A. H., M.B., Adelaide, South Australia.
Benson, A. V., M.R.C.S., Alberton, South Australia.
Bickle, L., F.R.C.S. Edin., Adelaide, South Australia.
Bird, F. D., M.B., M.S., M.R.C.S., Melbourne, Victoria.
Bird, W. J., M.B., Ch.M., Boort, Victoria.
Black, E., L.R.C.P., L.R.C.S. Edin., Perth, Western Australia.
Blackney, S., M.B., Port Adelaide, South Australia.
Bollen, C., M.B., Woodville, South Australia.
Bollen, Percival, M.B. Toronto, Semaphore, South Australia.
Bonnin, J. A., M.B., F.R.C.S., Adelaide, South Australia.
Borthwick, E. L., M.B., Ch.M. Edin., Payneham, South Australia.
Borthwick, T., M.D., Adelaide, South Australia.
Bourne, Eleanor, M.B., Ch.M., Brisbane, Queensland.
Bowker, R. Steer, M.R.C.S., L.R.C.P., Sydney, New South Wales.
Brady, A. J., L. et L. Mid., K.Q.C.P. Irel., L.R.C.S., Sydney, New South Wales.
Branson, G. A., M.R.C.S., L.R.C.P., Richmond, Victoria.
Brett, J. T., M.R.C.S., L.R.C.P., Melbourne, Victoria.

Brown, T. W., M.R.C.S., L.R.C.P., Hummocks Hill, South Australia.
Brummitt, R., M.R.C.S., Adelaide, South Australia.
Bryant, H. W., L.R.C.P. et S. Edin., Melbourne, Victoria.
Burgess, T. W. W., M.R.C.S., L.R.C.P., L.S.A., Wagga, New South Wales.
Burnard, Renfrey G., M.B., Adelaide, South Australia.
Burnett, L. G., M.B., Edin., F.R.C.S., Dunedin, New Zealand.
Butler, G. H., M.R.C.S. Eng., Hobart, Tasmania.
Byrne, Wm. S., M.D., B.S. Dub., M.R.C.P., Lond., Brisbane, Queensland.

Cameron, Donald, L.R.C.P. Ed., Mount Morgan, Queensland.
Cameron, J. A., M.B., B.S. Cantab., Brisbane, Queensland.
Carr, Hampden, L.R.C.P. et S. Irel., Port Pirie, South Australia.
Carvosso, A. B., M.B., C.M. Edin., Brisbane, Queensland.
Cawley, T., M.D., Adelaide, South Australia.
Champion, E., M.B , B.S. Melb., Ballarat East, Victoria.
Chapple, Fred, M.B., Ch.B., Adelaide, South Australia.
Chapple, Phœbe, M.B., B.S., Adelaide, South Australia.
Chenery, A., M.R.C.S., L.R.C.P., Port Augusta, South Australia.
Chisholm, W., B.A. Syd., M.D. Lond., M.R.C.S. Eng., Sydney, New South Wales.
Clarke, A. H., M.R.C.S. Eng., Hobart, Tasmania.
Cleland, W. L., M.B., C.M. Edin., Adelaide, South Australia.
Clendinnen, F. J., L.R.C.P. Lond., M.D. Brux., Hawksburn, Victoria.
Clenhall, W. T., M.D., B.S. Melb., F.R.C.S.E., Sydney, New South Wales.
Closs, J., M.D., Ch.M. Edin., Dunedin, New Zealand.
Clubbe, Charles, M.R.C.S., L.R.C.P., Sydney, New South Wales.
Collins, W. S., M.B. Lond., M.R.C.S., Wellington, New Zealand.
Colquhoun, D., M.D. Lond., M.R.C.P. Lond., M.R.C.S., Dunedin, New Zealand.
Connell, E. J., M.B., B.S. Melb., Warrnambool, Victoria.
Connolly, F. Glynn, M.R.C.S., L.R.C.P., Brisbane, Queensland.
Cooper, Constance M., M.B., B.S., Adelaide, South Australia.
Cooper, C. T., M.B., B.S., Adelaide, South Australia.
Corbin, John, M.R.C.S., Adelaide, South Australia.
Corbin, T. W., M.R.C.S., Adelaide, South Australia.
Covernton, Hugh, M.R.C.S., L.R.C.P., Gawler, South Australia.
Cowen, H. O., M.B., C.M., Glasgow, Kew, Victoria.
Crago, W. H., M.R.C.S., Eng., L.R.C.P. Lond., Sydney, New South Wales.
Craig, Gordon, M.B., Ch.M. Syd., Newtown, New South Wales.
Crouch, E. J., M.R.C.S., Hobart, Tasmania.
Cudmore, A. M., M.B., F.R.C.S., Adelaide, South Australia.
Culpin, Millice, L.R.C.P. et S. E., L.S.A. Lond., Brisbane, Queensland.
Cuscaden, George, L.R.C.P. et S. Edin., Port Melbourne, Victoria.

Darbyshire, D. E., M.B., Ch.B. Vic., M.R.C.S., L.R.C.P., Perth, Western Australia.
Davenport, A. F., M.B. Lond., M.R.C.S., St. Kilda, Victoria.
Davy, T. G., M.A., M.D., Ch.B. Oxon., Perth, Western Australia.
Dawes, R. St. M., M.R.C.S., &c., Gawler, South Australia.
Dawkins, S. L., M.B., Ch.B., Hamley Bridge, South Australia.
Donaldson, J. B., M.D., Linton, Victoria.
Douglas, F. J., M.B., &c., Port Victor, South Australia.
Downey, M. H., M.B., Ch.B. Melb., L.R.C.P. et S. Edin., Parkside, South Australia.
Downie, Taylor, M.B. C.M. Glas., Clifton Hill, Victoria.
Drummond, J. H. G., M.D., Strathalbyn, South Australia.

Eames, W. L'Estrange, B.A., M.B. et Ch.B. Dub., Newcastle, New South Wales.
Elkington, J. S. C., M.D., D.P.H., L.R.C.P. Ed., Tasmania.
Evans, H. M., M.B., Ch.B. Melb., Willunga, South Australia.
Evans, J. H., M.B., B.S., Brompton, South Australia.
Ewbank, W. W., M.R.C.S., Perth, Western Australia.
Ewing, S. A., L.R.C.P. et S. Ed., Melbourne, Victoria.

Fearnley, W. J., M.B., B.S. Melb., Charters Towers, Queensland.
Fell, Walter, M.D. Oxon., M.R.C.S. Wellington, New Zealand.
Ferguson, H. Lindo, M.D. Dub., Dunedin, New Zealand.
Fetherston, R. H., M.D., C.M., Melb., Prahran, Victoria.
Fiaschi, Thomas, M.D., Ch.D. Pisa and Florence, Sydney, New South Wales.
Fiege, A., M.D., Endunda, South Australia.
Finniss, J. H., M.B., Ch.M., Glenelg, South Australia.
Fischer, G. A., M.B., B.S., Adelaide, South Australia.
Fitzgerald, Sir T. N., C.B., F.R.C.S., Irel., Melbourne, Victoria.
Flecker, O. S., M.B., B.S., Ad., Subiaco, Western Australia.
Fleetwood, J. T., M.D. Dub., F.R.C.S. Irel., Warrnambool, Victoria.
Foreman, J., M.R.C.S. Eng., L.S.A. Lond., L. et L.M.R.C.P. Ed., Sydney, New South Wales.
Formby, H.H., M.B., F.R.C.S., Strathalbyn, South Australia.
Fox, W. R., L.R.C.P., L.R.C.S. Edin., Melbourne, Victoria.

Gault, A. H., M.D., Lond., M.R.C.S., L.R.C.P., Adelaide, South Australia.
Gault, E. L., M.B., B.S. Melb., Victoria.
Gething, W. J., M.R.C.S., L.R.C.P., Port Adelaide, South Australia.
Giblin, W. W,. M.R.C.S., L.R.C.P., Hobart, Tasmania.
Gibson, J. Lockhart, M.D., C.M., Edin., Brisbane, Queensland.
Gibson, T., M.B., C.M. Edin., Quorn, South Australia.
Giles, H. O'H., M.B., Ch.B. Ad., M.R.C.S., Glenelg, South Australia.
Giles, W. A., M.B., Ch.M. Edin., Adelaide, South Australia.
Gill, J. MacDonald, M.D. Lond., M.R.C.S., Sydney, New South Wales.
Gillies, Sinclair, M.D. Lond., M.R.C.S., Sydney, New South Wales.
Glynn, E. E., L.R.C.P. et S. Irel., Kapunda, South Australia.
Glynn, R. McM., L.R.C.P. et S. Irel., Riverton, South Australia.
Gocher, G., M.D., B.S. Durham, Bordertown, South Australia.
Good, J. E., M.B., Ch.M., Adelaide, South Australia.
Goode, A., M.B., B.S. Ad., Terowie, South Australia.
Grant, D., M.A., M.D. Edin., Melbourne, Victoria.
Gregerson, W. J., M.B., Ch.B. Melb., Adelaide, South Australia.
Gullett, Lucy, M.B., Ch.M. Syd., Bathurst, New South Wales.
Gunson, J. B., M.B., B.S., M.R.C.S., Adelaide, South Australia.

Halford, A. C. F., M.D., B.S. Melb., Brisbane, Queensland.
Ham, B. B., M.D. Brux., D.P.H. Cantab., Brisbane, Queensland.
Hamilton, A. A., M.B., Ch.B. Dub., Adelaide, South Australia.
Hamilton, C. W., M.D. Dub., Adelaide, South Australia.
Hamilton, J. A. G., B.A., M.B., L.R.C.S. Irel., Adelaide, South Australia.
Hamilton, R. H., L.R.C.S. Irel., Port Pirie, South Australia.
Hamilton, T. K., M.D. Dub., F.R.C.S. Irel., Adelaide, South Australia.
Harbison, D. T., M.D. Melb., F.R.C.S., Adelaide, South Australia.
Hardie, D., M.D., C.M. Aberd., Brisbane, Queensland,
Harris, L. Herschell, M.B., Ch M., Sydney, New South Wales.
Harrold, R. E., M.B., C.M. Edin., Adelaide, South Australia.

Hart, A. W., L.R.C.P. et S. Edin., Minlaton, South Australia.
Harwood, A. J., L.R.C.P. et R.C S. Edin., Adamstown, New South Wales.
Hawkes, C. S., M.R.C.S., L.R.C.P., Brisbane, Queensland.
Hayes, J. S., M.D., Q.U. Irel., Temuka, New Zealand.
Hayward, Lionel, M.B., B.S., Adel., Yorketown, South Australia.
Hayward, W. T., M.R.C.S., L.R.C.P. Irel., Adelaide, South Australia.
Heggaton, R. D., M.B., Syd., Murrumburrah, New South Wales.
Henderson, J., M.B., C.M., Crafers, South Australia.
Hill, A. W., M.D., Brux., M.R.C.S., L.R.C.P., Adelaide, South Australia.
Hinder, Critchley, M.B., Ch.M. Syd., Sydney, New South Wales.
Hogg, G. H., M.D. Edin., Launceston, Tasmania.
Hone, Frank, M.B., B.S. Ad., Semaphore, South Australia.
Hood, A. Jarvie, M.B., Ch.M. Glas., Sydney, New South Wales.
Hooper, J. W. D., L.R.C.P., L.R.C.S. Edin., Melbourne, Victoria.
Hornabrook, R. W., M.B., &c., Adelaide, South Australia.
Horrocks, H., B.Sc., M.D. Lond., Perth, Western Australia.
Howard, E. T., M.D., Ch.B. Melb., Melbourne, Victoria.
Hughes, W. Kent, M.B. Lond., M.R.C.S., &c., Melbourne, Victoria.
Humphrey, E. M., M.B., Ch.M., Sydney, New South Wales.
Hunter, W. A., M.B., B.S., Salisbury, South Australia.

Inglis, Tracy R., M.B., Ch.B. Melb., Auckland, New Zealand.
Isbister, J. L. T., M.B., Ch.B. Adel., North Sydney, New South Wales.

Jackson, E. S., M.B., Ch.M. Melb., Brisbane, Queensland.
Jackson, James, M.D. Lond., M.R.C.S., Collins Street, Melbourne, Victoria.
James, T., M.R.C.S., Moonta, South Australia.
Jamieson, J., M.D., C.M. Glas., Melbourne, Victoria.
Jay, Melville, M.R.C.S., L.R.C.P., Adelaide, South Australia.
Jermyn, F. D., M.B., Ch.B. Melb., Mount Gambier, South Australia.
Johnson, E. A., M.D. Göttingen, Adelaide, South Australia.
Johnson, John, M.B., F.R.C.S., Mount Gambier, South Australia.
Jones, Sir P. Sydney, M.D. Lond., F.R.C.S. Eng., Sydney, New South Wales.
Jones, Robert H., M.B., B.S., Sydney, New South Wales.
Jude, F. J., M.B., Ch.M. Edin., Broken Hill, New South Wales.
Jüttner, F. J. G., M.B., Ch.B. Melb., Tanunda, South Australia.

Kelsall, H. T., M.D. Lond., M.R.C.S., L.R.C.P., Perth, Western Australia.
Kennedy, J. C., M.B., Ch.B. Melb., Snowtown, South Australia.
Kenny, A. L., M.B., Ch.B. Melb., Melbourne, Victoria.
Kerr, D., M.B., C.M. Glas., Angaston, South Australia.
Kirkland, Hugh, M.B., Ch.M. Glas., Lithgow, New South Wales.
Kirkland, T. Spiers, M.D. Glas., Sydney, New South Wales.

Lamb, Wm., M.B., Ch.M. Edin., Mannum, South Australia.
Lander, C. D., M.B., Ch.M. Edin., Port Adelaide, South Australia.
Laver, C. W., L.R.C.P. et S. Edin., Perth, Western Australia.
Lawrence, A., M.B., L.R.C.S. Edin., Adelaide, South Australia.
Lawrence, H. F., M.R.C.P., L.R.C.S. Edin., Melbourne, Victoria.
Le'Estrange, Guy, L.R.C.S. Irel., L.R.C.P. Irel., Brisbane, Queensland.
Lendon, A. A., M.D. Lond., M.R.C.S., &c., Adelaide, South Australia.
Lewis, Thos. Hope, M.R.C.S., Auckland, New Zealand.
Lines, D. H. E., M.B., Ch.B. Melb., Hobart, Tasmania.
Lipscomb, T. W., M.B., Ch.M. Syd., Leichardt, Sydney, New South Wales.
Litchfield, W. F., M.B., Ch.B. Syd., Glebe, Sydney, New South Wales.

Long, W. J., M.B., Ch.B. Melb., Bendigo, Victoria.
Loughrey, B., M.B., Ch.B. Melb., Hawthorn, Victoria.
Love, W. Wilton, M.B., Ch.M., Edin., Brisbane, Queensland.
Lovegrove, J. F., M.R.C.S., L.S.A., Gunning, New South Wales.
Lovegrove, T. H., M.R.C.S., Perth, Western Australia.
Ludlow, Victor E., L.R.C.S. Irel., L. et L. Mid., K.Q.C.P. Irel., Waverley, Sydney, New South Wales.
Luker, Donald, M.B., Ch.M. Syd., Sydney, New South Wales.
Lukowitz, M. von, M.D., Staats Exam., Adelaide, South Australia.

MacCormick, A., M.D. Edin., M.R.C.S., Sydney, New South Wales.
MacDonald, D., M.B., M.S. Glas., Adelaide, South Australia.
Macdonald, T. F., M.B., C.M., &c., Geraldton, Queensland.
Macfarlane, W., M.B., Melb. New Norfolk, Tasmania.
Mackay, E. Alan, M.B., Ch.B. Melb., South Yarra, Victoria.
Magarey, C., M.B., Ch.B. Adel., Norwood, South Australia.
Magarey, Frank, M.D. Syd., Adelaide, South Australia.
Magarey Rupert, M.B., Ch.B. Melb., North Adelaide, South Australia.
Magee, C. Crozier, L.R.C.P., M.R.C.S., Penola, South Australia.
Magnus, F. D., M.D. Nat. Univ. Wash., U.S.A., Sydney, New South Wales.
Maher, W. Odillo, M.D., M.S., M.R.C.S., Sydney, New South Wales.
Mainwaring, W. R. Cavenagh-, M.B., Ch.B. Adel., F.R.C.S., Adelaide, South Australia.
Maitland, H. L., M.B., Ch.M. Syd., Sydney, New South Wales.
Marks, A. H., M.B., B.S. Dub., Brisbane, Queensland.
Marks, C. F., M.D., Q.U. Irel., M.R.C.S., Brisbane, Queensland.
Marks, Herbert W. J., M.D., B.S. Cantab., M.R.C.S., Sydney, New South Wales.
Marsden, W. C., L.R.C.P. et S. Edin., Port Broughton, South Australia.
Marshall, H. Hamilton, M.B., M.S. Edin., Ch.B. Melb., L.R.C.P. et S. Edin., Sydney, New South Wales.
Marten, R. H., M.D. Cantab., M.R.C.S., Adelaide, South Australia.
Maudsley, H. C., M.D. Lond., F.R.C.P. Lond., M.R.C.S., Melbourne, Victoria.
McCallum, G., M.B., Ch.M. Glas., Geelong, Victoria.
McDouall, H. C., M.R.C.S., L.R.C.P., D.P.H. Cantab., Gladesville, Sydney, New South Wales.
McGowan, A. G., M.B., Ch.B. Melb., Ballarat, Victoria.
McInerny, J., M.D. New York, North Carlton, Victoria.
McIntyre, Sinclair, M.D. Glas., D.P.H. Cantab., Queen Victoria Sanatorium, New South Wales.
Mead, Gertrude, M.B., Ch.B. Melb., Perth, Western Australia.
Meikle, A. Jamieson, M.D., Ch.M. Edin., Yankalilla, South Australia.
Meinholt, Maximilian, Staats Exam., M.D., Bonn, Footscray, Victoria.
Meyer, Felix, M.D. Melb., Melbourne, Victoria.
Millard, R. J., M.B., Ch.M. Syd., D.P.H. Cantab., Sydney, New South Wales.
Mills, Arthur E., M.B., Ch.M. Syd., Strathfield, Sydney, New South Wales.
Mitchell, J. T., M.D. Aber., M.R.C.S., Ballarat, Victoria.
Morgan, A. M., M.B., Ch.B. Adel., Adelaide, South Australia.
Morgan, E. H., M.R.C.S., Hamilton, Tasmania.
Moore, T. C., M.D. Dub., Napier, New Zealand.
Moorhouse, B. M., M.B., M.S. Edin., M.R.C.S., Christchurch, New Zealand.
Morris, Bedlington H., M.B., B.S. Durham, Parkside, South Australia.
Morris, E. W., M.R.C.S., &c., Port Adelaide, South Australia.
Morton, D. Murray, M.D., Ch.B. Melb., Richmond, Victoria.
Morton, F. W. W., L.R.C.P. et S. Edin., Fitzroy, Victoria.

Muir, R. S., L.R.C.P. et S. Edin., Mount Gambier, South Australia.
Murray, H. L., L.R.C.P. et F.R.C.S. Edin., Melbourne, Victoria.
Moule, E. E., M.B., Ch.B. Adel., Nairne, South Australia.

Naphthine, G. J., L.R.C.P. et S. Edin., Stawell, Victoria.
Napier, Leith A. D., M.D. Aber., M.R.C.P., Adelaide, South Australia.
Nash, J. B., M.D. Edin., M.R.C.S., North Sydney, New South Wales.
Naylor, A. G. E., L.R.C.P. et S. Edin., Foster, Victoria.
Neill, J. H., M.B., Ch.B., N.Z., Auckland, New Zealand.
Newell, J. A., M.B., M.S., N.Z., Lytteltou, New Zealand.
Newland, H. S., M.B., M.S. Adel., F.R.C.S., Adelaide, South Australia.
Newland, Clive, M.B., Ch,B. Adel., M.R.C.S., Morphett Vale, South Austarlia.
Newman, F. J., M.B., Ch.B. Melb., Geelong, Victoria.
Newton, R. E., M.B. Glas., F.R.C.S., Perth, Western Australia.
Nicholls, G. Gray, M.B. Melb., Maitland, South Australia.
Niesche, F. W., M.D. Edin., Adelaide, South Australia.
Norman, Walter, M.R.C.S., L.R.C.P., Goolwa, South Australia.
Nyulasy, F. A., M.B., Ch.B. Melb., Toorak, Victoria.

Officer, E. A., M.B., Ch.B. Melb., Perth, Western Australia.
Ogston, Frank, M.D., M.R.C.S., Dunedin, New Zealand.
O'Hara, Henry, F.R.C.S. Irel., Melbourne, Victoria.
O'Leary, A. P. Evelyn, M.R.C.S., L.R.C.P., Glenelg, South Australia.
Oram, A. Murray, M.D., C.M. Ed., Sydney.
O'Reilly, Susan H., M.B., Ch.B. Syd., Adelaide, South Australia.
O'Reilly, Walter W. J., M.D., Ch.M. Queen's Uni., Irel., M.R.C.S., Sydney, New South Wales.
Orr, Andrew W., M.D., B.S. Dub., L K.Q.C.P.I., Brisbane, New Zealand.

Palmer, Arthur, M.B., C.M. Syd., M.R.C.S. Eng., L.R.C.P. Lond., Sydney, New South Wales.
Pike, C. J., M.B., B.S. Lond., M.R.C.S., Launceston, Tasmania.
Plummer, Violet, M.B., Ch.B. Melb., Adelaide, South Australia.
Pockley, F. Antill, M.B., Ch.M. Edin., M.R.C.S. Eng., Sydney, New South Wales.
Pooler, E. L., M.D., Ch.M. Dub., Stirling East, South Australia.
Poulton, B., M.D., M.R.C.S., Adelaide, South Australia.
Powell, H. Arthur, M.B., Ch.B. Adel., Kadina, South Australia.
Proctor, J. S., L.K.Q.C.P. Irel., L.R.C.S., Mount Pleasant, South Australia.
Purser, Cecil, B.A., M.B., Ch.M. Syd., Lewisham, New South Wales.

Ramsay, J. E., M.B. Lond., Perth, Western Australia.
Ramsay, J., M.B., Ch.B. Melb., Launceston, Tasmania.
Read, Clarence, M.R.C.S. Eng., L.R.C.P. Lond., Chatswood, Sydney, New South Wales.
Read, G. F., L.R C.P. et S. Edin., Tasmania.
Reid, R. G., M.D., Nagambi, Victoria.
Reissmann, C., M.D. Cantab., M.R.C.P., Adelaide, South Australia.
Rennie, G. E., B.A. Syd., M.D. Lond., M.R.C.S., M.R.C.P., Sydney. New South Wales.
Richards, S. J,. M.B., Ch.M. Syd., Mount Morgan, Queensland.
Roberts, E. J., M.B., B.S., N.Z., Hobart, Tasmania.
Roberts, Lewellin W., M.B., Ch.B. Melb., M.R.C.S., L.R.C.P. Lond., Cowra, New South Wales.
Roberts, W. S. W., M.R.C.S., Dunedin, New Zealand.

Robertson, W., M.B., Ch.B. Melb., Adelaide, South Australia.
Robertson, W. V., M.B., Ch.M. Edin., Brisbane, Queensland.
Rogers, R. S., M.A., M.D., Adelaide, South Australia.
Rosenfeld, R. L., M.B., Ch.B. Melb., Port Melbourne, Victoria.
Ross, Chisholm, M.B., Ch.M. Edin., M.D. Syd., Sydney, New South Wales.
Russell, H. H., M.D. Adel., Ch.B., M.R.C.P. et S. Edin., Unley, Adelaide, South Australia.
Russell, R. H., F.R.C.S. Eng., L.R.C.P., Melbourne, Victoria.
Russell, R. U., L.R.C.S. Irel., L.R.C.P. Edin. Newcastle, New South Wales.
Ryan, C., M.B., Ch.M. Edin., Melbourne, Victoria.
Ryan, J. P., L.K.Q.C.P.I., Melbourne, Victoria.
Ryan, T. F., M.B., Ch.B. Melb., Nhill, Victoria.

Salter, A. E., M.B., B.S. Melb., Sydney, New South Wales.
Sangster, J. I., sen., M.R.C.S. Eng., L.R.C.P. Edin., Burra, South Australia.
Sangster, J. I., jun., M.B., Ch.B. Adel., M.R.C.S., Burra, South Australia.
Saunders, John Harry, M.B., B.S. Melb., M.R.C.S. Eng., Sydney, New South Wales.
Savage, T. C., M.B., Lond. F.R.C.S., Auckland, New Zealand.
Saw, A. J. H., M.D., B.S. Cantab., Perth, Western Australia.
Scott, E. Kerr, M.B., Ch.M. Edin., Brisbane, Queensland
Scott, F. Steele, M.B., Ch.B., Irel., Mitcham, Adelaide, South Australia.
Scott, J. D. K., M.B., Ch.B. Melb., Queenscliffe, Victoria.
Scott, J. H., M.D., Edin., F.R.S., Dunedin, New Zealand.
Scott, J. M., M.B., B.C., Kalgoorlie, Western Australia.
Scott, Malcolm L., M.B., Ch.B. Adel., Adelaide, South Australia.
Scott, Robert, M.B., Ch.M. Glas., Ballarat, Victoria.
Scott, R. G., M.B., C.M. Edin., Hobart, Tasmania.
Scott, Thos., M.R.C.S., L.R.C.P., Warrnambool, Victoria.
Seabrook, L. L., M.B., Ch.B. Adel., Broken Hill, New South Wales.
Setzke, E. F., M.D. Griefswald, Broken Hill, New South Wales.
Shanasy, T., L.R.C.P. et S. Edin., Nhill, Victoria.
Shepherd, A. E., L.R.C.P., L.R.C.S. Edin., Norwood, South Australia.
Shields, A., M.D. Edin., Melbourne, Victoria.
Sinclair, J. W., M.B., Ch.M. Syd., Sydney, New South Wales.
Smeaton, Bronte, M.B., M.R.C.S., Adelaide, South Australia.
Smith, A. A., M.R.C.S., L.R.C.P., Clare, South Australia.
Smith, Charles, M.R.C.S., L.R.C.P., M.D. Lond., South Yarra, Victoria.
Smith, Otto W., M.D., Ch.M. Edin., Clare, South Australia.
Smith, Ventry A. J., L.R.C.S. Irel., L.K.Q.C.P.I., Grafton, New South Wales.
Smith, W. B., L.R.C.P. et S. Edin., F.R.C.S. Edin., Melbourne, Victoria.
Smith, William Ramsay, D.Sc., M.B., &c., Adelaide, South Australia.
Souter, C. H., M.B., Ch.M., Adelaide, South Australia.
Springthrope, J. W., M.D. Melb., Ch.M., M.R.C.P. Lond., Nhill, Victoria.
Sprott, Gregory, M.D., C.M., D.P.H. Glas., Hobart, Tasmania.
Steell, J., M.B., Ch.M. Edin., Ballarat, Victoria,
Steven, G. M., M.B., Ch.M. Edin., Booleroo, South Australia.
Stewart, R. W., M.R.C.S., L.R.C.P. Edin., Port Pirie, South Australia.
Stirling, E. C., C.M.G., M.A., M.D. Cantab., F.R.C.S. Eng., F.R.S., &c., Adelaide, South Australia.
Stoker, Henry, L.R.Q.C.P.I., F.R.C.S.I., Wagga, New South Wales.
Stokes, A. F., M.B., Ch.B. Adel., Adelaide, South Australia.
Stokes, Edward S., M.B., Ch.M. Syd., D.P.H., Sydney, New South Wales.
Stoney, R. B., M.B., Ch.B. Dub., Echuca, Victoria.
Strangman, T. H., M.R.C.S. Irel., L.K.Q.C.P.I., Seymour, Victoria.

Sweetapple, H. A., M.D., B.S. Durham, Adelaide, South Australia.
Syme, G. A., M.B., Ch.M., Melb., F.R.C.S. Eng., Melbourne, Victoria.
Symons. M. J., M.D., Ch.M. Edin., Adelaide, South Australia.

Taylor, W. F., M.D., M.R.C.S., D.P.H., R.C.S. Eng., Brisbane, Queensland.
Thane, P. T., M.R.C.S. Eng., L.R.C.P. Lond., Sydney, New South Wales.
Thomas, Walter, M.B., M.S. Glas., Christchurch, New Zealand.
Thompson, Ashburton, M.D. Brux., D.P.H. Camb., M.R.C.S. Eng., and L. M., L.R.C.P. Lond., L.S.A. Lond., Sydney, New South Wales.
Thomson, John, M.B., C.M. Edin., Brisbane, Queensland.
Tidswell, Frank. M.B., Ch.M. Syd., Sydney, New South Wales.
Todd, C. E., M.D. Brux., M.R.C.S., Adelaide, South Australia.
Traill, M. W., M.R.C.S. Eng., L.R.C.P. Lond., Burwood, New South Wales.
Tratman, F., M.D. Lond., D.P.H., M.R.C.S., Perth, Western Australia.
Trethowan, W., M.B., M.B. Aber., Perth, Western Australia.
Turner, A. J., M.D. Lond., M.R.C.S., Brisbane, Queensland.
Tyrie, C. Baxter, M.B., C.M. Edin., Brisbane, Queensland.

Verco, C. A., M.B., B.S. Syd., Adelaide, South Australia.
Verco, J. C., M.D. Lond., F.R.C.S. Eng., Adelaide, South Australia.
Verco, W. A., M.B., Ch.B. Adel., Adelaide, South Australia.
Voss, F. H. Vivian, F.R.C.S. Eng., L.S.A., Rockhampton, Queensland.

Watson, Archibald M.D., &c., Paris and Göttingen, F.R.C.S., Adelaide, South Australia.
Webb, J. R., M.B., Ch.B. Melb., F.R.C.S. Eng., Footscray, Victoria.
Webster, P. S., M.R.C.S., L.R.C.P., M.D. Durham, Melbourne, Victoria.
Weld, Eleanor E., M.B., Ch.B. Melb., Adelaide, South Australia.
Welsh, D. A., M.D. Edin., M.S., University, Sydney, New South Wales.
Wheeler, J. A., M.B. Lond., M.R.C.S., Brisbane, Queensland.
Whishaw, R. R., M.B. Camb., F.R.C.S. Eng., Brisbane, Queensland.
Wield, David, M.D., C.M. Edin., Brisbane, Queensland
Wigg, A. E., M.D. Brux., M.R.C.S., L.R.C.P., Adelaide, South Australia.
Wigg, H. H., M.D. Brux., M.R.C.S., L.R.C.P., Adelaide, South Australia.
Wilkinson, W. Camac, B.A. Syd., M.D. Lond., F.R.C.P. Eng., Sydney, New South Wales.
Williams, J. O'B., Fleet-Surgeon R N. M.D., &c., H.M.S. *Euryalus*, Sydney, New South Wales.
Williams, Surgeon-General W. D. C., C.B., M.R.C.S., L.R.C.P., Melbourne, Victoria.
Willis, F. R. H., M.D., Ch.B. Melb., Malvern, Victoria.
Wilson, C. E. C., M.B., Ch.B. Melb., Wallaroo, South Australia.
Wilson, F., L.R.C.S., L.R.C.P. Edin., D.P.H., R.C.S. Edin., Brisbane, Queensland.
Wilson, J. T., M.B., Ch.M. Edin., University of Sydney, New South Wales.
Wilson, T. A., M.B. Melb., Creswick, Victoria.
Wilson, T. G., M.D, Ch.B. Syd., Adelaide, South Australia.
Wohlmann, A. G., M.D., Lond. M.R.C.S., Rotorua, New Zealand.
Wolfhagen, J. G., M.B., C.M. Edin., Hobart, Tasmania.
Wood, A. Jeffrys, M.D., Ch.B. Melb., Melbourne, Victoria.
Wood, W. A., M.D., Ch.B. Melb., D.P.H., R.C.S., South Yarra, Victoria.
Woods, G., L.K.Q.C.P., L.R.C.S., Clarendon, South Australia.
Woodward, G. P. M., M.D. Brux., F.R.C.S.I., Sydney, New South Wales.
Worrall, Ralph, M.D. et Ch.M., Q.U. Irel., Sydney, New South Wales.

HONORARY MEMBERS.

J. R. Baker, B.A., LL.B. Cantab., Chairman of Local Board of Health, Adelaide.

W. Barlow, LL.D., Vice Chancellor of the University of Adelaide.

C. A. Bayer, Esq., Hydraulic Engineer, South Australia.

The Hon. G. Brookman, M.L.C., South Australia.

Theodore Bruce, Esq., Mayor of Adelaide.

Richard Caton, M.D., &c., Liverpool, England.

T. G. Ellery, Esq., Town Clerk, Adelaide.

A. G. Fryett, Esq., Melbourne, Victoria.

C. R. Hodge, Esq., Registrar, University of Adelaide.

T. Roberts, Esq., Chief Mechanical Engineer to the Railway Department of South Australia.

Australasian Medical Congress.

SEVENTH SESSION.

PROGRAMME OF CONGRESS WORK.

MONDAY, SEPTEMBER 4TH.

10 a.m.	Registration of Members at University.
11.30 a.m.	General Business Meeting of Congress at University.
8.30 p.m.	Inaugural Meeting and President's Address at the Elder Hall, University.

TUESDAY, SEPTEMBER 5TH.

10 a.m.	Address in Medicine by Dr. Daniel Colquhoun, M.D. (Lond.), M.R.C.P., M.R.C.S., of Dunedin, President of the Section, in the Prince of Wales' Theatre, University.
11 a.m. to 1 p.m.	Primary Meetings of all Sections.
12 noon.	Section of Pathology: Address on Cancer, by Professor Welsh, M.A., M.D., B.Sc. (Edin.), of Sydney, President of the Section.

WEDNESDAY, SEPTEMBER 6TH.

10 a.m.	Address in Surgery by Mr. Fred. Bird, M.B., M.S. (Melb.), M.R.C.S, of Melbourne, President of the Section.
11 a.m. to 1 p.m.	Sectional Work.
12 noon.	Section of State Medicine: Address by Mr. Syme, M.B., M.S. (Melb.), F.R.C.S., President of the Section.

THURSDAY, SEPTEMBER 7TH.

10 a.m. to 1 p.m.	Sectional Work.

FRIDAY, SEPTEMBER 8TH

10 a.m. to 1 p.m.	Sectional Work.
8 p.m.	Popular Health Lecture by Dr. W. G. Armstrong, B.A., M.B., Ch.M. (Syd.), D.P.H. (Cantab.), President of the Section of Public Health, in the Town Hall, under the auspices of the Right Worshipful the Mayor of Adelaide.

SATURDAY, SEPTEMBER 9TH.

9.30 a.m.	Sectional Meetings.
10 a.m.	Final General Meeting of Congress at University.

Section I.

MEDICINE.

President .. .. Daniel Colquhoun, M.D., Lond., M.R.C.P., M.R.C.S. ; Dunedin.

Vice-Presidents .. G. E. Rennie, M.D., Lond., M.R.C.P., M.R.C.S. ; Sydney.
David Grant, M.A., M.D.. Edin. ; Melbourne.
A. J. Turner, M.D., Lond., M.R.C.S. ; Brisbane.
H. Astles, M.D., St. And., F.R.C.P. Edin. ; Perth.
R. R. Whishaw, M.B., B.C., Cantab. ; Hobart.
W. Fell, M.D., Oxon., M.R.C.S., L.R.C.P. ; Wellington, N.Z.

Secretary .. .. H. Swift, M.D., Cantab., M.R.C.S. ; Adelaide.

LIST OF PAPERS.

The Address in Medicine—Dr. Colquhoun.

1. Diet in Relation to Kidney Disease.
Dr. Rennie.

2. Epilepsy : Is it incurable ?
Dr. Rennie.

3. The Mineral Waters of New Zealand.
Dr. Wohlmann.

4. Is there such a Disease as Croup ?
Dr. H. E. Astles.

6. Preliminary Communication on the No-Food Treatment of Typhoid Fever.
Dr. Abramowski.

7. Pulmonary Tuberculosis.
Dr. McIntyre Sinclair.

8. Sanatorium Treatment of Phthisis.
Dr. Stoney.

9. Relation of Bovine to Human Tuberculosis.
Dr. W. C. Wilkinson.

10. The Results of Sanatorium Treatment of Pulmonary Tuberculosis.
Dr. W. C. Wilkinson.

11. Sanatorium Treatment of Phthisis.
Dr. Gault.

12. Anchylostomiasis.
Dr. MacDonald.

13. The Nasal Treatment of Asthma.
Dr. W. V. Robertson

14. Anaemias of Children and Infancy.
Dr. C. Baxter Tyrie.

15. The Blood Count of Appendicitis.
Dr. Wilton Love.

16. The Medical Treatment of Appendicitis.
Dr. F. L. Benham.

17. A Fatal Case of Acute Pancreatitis.
Dr. F. L. Benham.

18. Radium and Electro Therapeutics in the Treatment of Skin Diseases.
Dr. H. Lawrence.

Exhibits—" An X Ray Bath."
Dr. H. Lawrence.

19. Present Position of the Rontgen Rays in Medicine and Surgery.
Dr. L. H. Harris.

20. A Case of Spleno-Medullary Leucocythaemia treated with X Rays.
Dr. F. Clendinnen.

21. A Case of Lymphadenoma (internal) treated with X Rays.
Dr. F. Clendinnen.

22. A Case of Bazin's Malady Treated with Ultra-Violet Light.
Dr. F. Clindennen.

23. The Principles of Chemistry and the Study of Medicine.
Dr. Reissmann.

24. On Leukaemia and allied conditions in Children.
Dr. J. Macdonald Gill.

25. Two Cases of Lead-Poisoning.
Dr. J. Macdonald Gill.

26. The External Application of Guaiacol.
Dr. T. C. Moore.

27. The Complications of Pneumonia other than Pulmonary.
Dr. Scott Skirving.

28. Use and Abuse of Mental Therapeutics.
Dr. Springthorpe.

29. Transposition of the Organs verified by *Post-mortem* Examination.
Dr. Jarvie Hood.

CLINICAL EXHIBITS: 9 A.M., WEDNESDAY, SEPTEMBER 6TH.

Clinical Session—
Exhibits: Cases of Skin Disease, &c.
Dr. H. Swift.

Section II.

SURGERY.

President .. .. F. D. Bird, M.B., M.S., Melb., M.R.C.S.; Melbourne.

Vice-Presidents .. H. V. C. Hinder, M.B., Ch.M., Syd.; Sydney.
R. Hamilton Russell, F.R.C.S.; Melbourne.
E. S. Jackson, M.B., Ch.B., Melb.; Brisbane.
F. Tratman, M.D., Lond., M.R.C.S., L.R.C.P., D.P.H., Lond.; Perth.
R. G. Scott, M.B., Ch.M., Edin.; Hobart.
W. E. Collins, M.B., Lond., M.R.C.S.; Wellington, N.Z.

Secretaries .. .. W. Anstey Giles, M.B., Ch.M., Edin.; Adelaide.
C. E. Todd, M.D., Brux., M.R.C.S.; Adelaide.

LIST OF PAPERS.

1. Address in Surgery.
Mr. F. Bird.

2. Prostatectomy.
Dr. A. MacCormick.

3. Prostatectomy.
Dr. H. O'Hara.

4. Enlarged Prostate.
Dr. H. L. Maitland.
5. Prostatectomy.
Dr. Poulton.
6. Urinary Surgery.
Dr. C. Hinder.
7. Papilloma of Kidney.
Dr. A. A. Lendon.
8. Cerebral Hydatids.
Dr. McCormick.
9. Congenital Hypertrophic Stenosis of Pylorus.
Dr. A. McKay.
10. Intestinal Anastomosis.
Dr. MacCormick.
11. Stereoscope in Surgery.
Dr. C. H. Souter.
12. Radical Cure of Hernia.
Dr. H. Russell.
13. Hernia.
Mr. F. Bird.
14. Appendicitis.
Dr. C. Hinder.
15. Appendicitis.
Dr. Long.
16. Appendicitis.
Dr. McCallum.
17. Appendicitis.
Dr. Kelvington,
18. Appendicitis.
Dr. Scott.
19. Appendicitis,
Dr. Newman.
20. Appendicitis.
Dr. Steer Bowker.
21. Observations on Appendicitis.
Dr. Fiaschi.
22. Treatment of Movable Kidney.
Dr. Collins.
23. Some Subject in Paediatrics.
Dr. Turner.
24. Nerve Crossing and Suturing.
Dr. Kelvington.
25. Some Strange Coincidences in Surgical Cases.
Dr. Bickle.
26. Notes on Modern Cerebral Surgery.
Dr. Marten.
27. Brophy's Method of Treating Cleft Palate.
Dr. M. Morton.
28. Transplantation of Ureters into Rectum.
Dr. Newland.

Clinical Exhibits: 9 a.m., Thursday, September 7th.

Section III.

GYNÆCOLOGY AND OBSTETRICS.

President .. .. W. S. Byrne, M.D., Ch.M., Dub., M.R.C.P. ; Brisbane.

Vice-Presidents .. F. T. Thring, F.R.C.S., L.R.C.P. ; Sydney.
G. Rothwell Adam, M.D., Ch.M., Edin., M.D., Melb. ; Melbourne
F. G. Connolly, M.R.C.S., M.R.C.P. ; Brisbane.
H. Horrocks, M.D., Lond., M.R.C.S., L.R.C.P. ; Perth.
C. J. Pike, M.B., Ch.B., Lond., M.R.C.S. ; Launceston.
B. Moorehouse, M.B., Ch.M., Edin., M.R.C.S. ; Christchurch, N.Z.

Secretary .. .. J. A. G. Hamilton, B.A., M.B., Dub., L.R.C.S., Edin. ; Adelaide.

LIST OF PAPERS.

The Opening Address by the President.

1. Some Causes of Pelvic Suppuration.
Dr. Rothwell Adam.

2. Treatment of Pelvic Suppuration.
Dr. R. Worrall.

3. Some Practical Points in Relation to frequently performed Operations in Gynæcology.
Dr. F. W. Thring.

4. The Evolution of the Abdominal Operation and Incision.
Dr. Foreman.

5. Chorion Epithelioma.
Dr. Batchelor.

6. Differential Diagnosis between Appendicitis and Pelvic Disease in Young Women.
Dr. D. Hooper.

7. Puerperal Sepsis.
Dr. Cowen.

8. The Toxæmias of Pregnancy.
Dr. Batchelor.

9. Paper.
Dr. Felix Meyer.

10. Thrombosis of Jugular Vein after Parturition.
Dr. F. Clendinnen.

11. Retroflexion of Uterus.
Dr. T. G. Wilson.

12. Acute Dilatation of Stomach following Abdominal Section.
Dr. J. A. G. Hamilton.

13. Modern Treatment of Puerperal Sepsis following Labor and Abortion.
Dr. Connolly.

14. A Rare Form of Pelvic Tumor.
Dr. Nyulasy.

15. Ovarian Dermoid.
Dr. Setzke.

Section IV.

EYE, EAR, AND THROAT.

Presidents .. .. A. J. Brady, L.R.C.S.I., L.K.Q.C.P.I. ; Sydney.
W. Odillo Maher, M.D., Ch.M., Dub., M.D., Syd.. M.R.C.S. ; Sydney

Vice-Presidents .. G. T. Hankins, M.R.C.S. ; Sydney.
P. S. Webster, M.D., Durh., M.R.C.S., L.R.C.P. ; Melbourne.
J. Lockhart Gibson, M.D., Edin., M.R.C.S. ; Brisbane.
H. T. Kelsall, M.D., B.S., Lond., M.R.C.S., L.R.C.P. ; Perth.
W. W. Giblin, M.R.C.S., L.R.C.P. ; Hobart.
H. Lindo Ferguson, M.A., M.D., Dub., F.R.C.S.I. ; Dunedin, N.Z

Secretaries .. .. Charles W. Hamilton, B.A., M.D., Dub. ; Adelaide.
M. J. Symons, M.D., Ch.M., Edin. ; Adelaide.

(A) EYE DIVISION.—PAPERS, &c.

President .. .. Dr. W. Odillo Maher.

Secretary .. .. Dr. M. J. Symons.

Tuesday, 11 a.m.—Opening Address by the President.
Discussion Paper on Trachoma by Dr. Antill Pockley.

1. Keratitis during and after Dengue.
2. Pseudo-Myopia and its Treatment.
   Dr. Lockhart Gibson.
3. Cases Illustrating the Influence of Increased Intracranial Pressure as a Cause of Optic Neuritis.
4. Exophthalmos in relation to Disease in the adjacent Bony Cavities.
   Dr. Wallace Mackenzie.

Wednesday, 11 a.m.—Discussion Paper on Plumbic Neuritis—Dr. Lockhart Gibson.

1. Treatment of Trachoma with Cuprol.
2. Cases of Extraction of Foreign Bodies by Magnet.
3. The Difficulties in arriving at a Decision as to when an Injured Eye must be removed.
   Dr. G. M. Scott.
4. Accommodation in Aphakic Eyes.
   Dr. R. H. Jones.
5. On the suturing of gaping wounds of the Sclerotic, and of those made by the Surgeon in Cataract Operations.
   Dr. J. P. Ryan.
6. Paper undescribed.
   Dr. Andrew Orr.
7. Paper undescribed.
   Dr. Andrew Orr.
8. Lantern Slide Illustrations.
   Dr. Andrew Orr.
9. Melanotic Sarcoma of the Conjunctiva.
   Dr. Pockley.
10. Myasthenia Gravis in special relation to Eye and Throat Conditions.
    Dr. G. H. Hogg.
11. Exhibit.—A Cast of Paraphimosis Conjunctivae.
    Dr. L. Herschell Harris.

(B) NOSE, EAR, AND THROAT DIVISION.—PAPERS, &c.

President .. .. Dr. A. J. Brady.

Secretary .. .. Dr. C. W. Hamilton.

The following subjects have been selected for special discussion :—

A. The Indications for the Radical Mastoid Operation, the Methods of its performance, and the value or otherwise of Epithelial Grafting.

B. The Diagnosis and Treatment of Suppuration of the Accessory Sinuses of the Nose.

Discussion *A* will be introduced by Dr. Webster (Melbourne), Dr. Lockhart Gibson (Brisbane), and Dr. Arthur (Sydney).

Discussion *B* will be introduced by Dr. Kirkland (Sydney), and Dr. Hogg (Launceston). The following Papers will be read :—

1. Nasal and Nasopharyngeal Factors in Chronic Catarrhal Disease of the Middle Ear.
   Dr. Ewing.

2. The Therapeutics of the Commoner Affections of the Nose.
   Dr. Kirkland.

3. Certain Considerations regarding Operations of the Mastoid.
   Dr. Arthur.

Section V.

PATHOLOGY, ANATOMY, PHYSIOLOGY, & THERAPEUTICS.

President .. .. Professor D. A. Welsh, M.A., M.D., B.Sc., Edin. ; Sydney.

Vice-Presidents .. Professor J. T. Wilson, M.B., Ch.M., Edin. ; Sydney.
C. H. Mollison, M.B., Ch.B., Melb., M.R.C.S. ; Melbourne.
J. A. Wheeler, M.B., Lond. ; Brisbane.
W. Trethowan, M.B., Ch.M., Aberd. ; Perth.
A. H. Clarke, M.R.C., L.R.C.P. ; Hobart.
W. S. Roberts, M.R.C.S. ; Dunedin, N.Z.

Secretary .. .. W. R. Cavenagh-Mainwaring, M.B., Ad., F.R.C.S. ; Adelaide.

LIST OF PAPERS.

1. Address by the President.
   Professor Welsh.

2. Paper on Deciduoma Malignum.
   Professor Welsh.

3. Leucocythaemia.
   Dr. Reissmann.

4. Human Parasites.
   Dr. Angas Johnson.

5. Note on a New Human Parasite.
   Dr. MacCormick and Professor Welsh.

6. Note on a Larval Tapeworm from the Human Subject.
   Dr. MacCormick and Dr. J. P. Hill.

7. Note on the frequency of Streptothrix Infections in Man.
   Dr. MacCormick, Dr. Barling, and Professor Welsh.

8. Exhibits with Lantern.
   Dr. J. Thomson.

Section VI.

PUBLIC HEALTH, &c.

President .. .. W. G. Armstrong, B.A., M.B., Ch.M., Syd., D.P.H., Cantab.; Sydney.

Vice-Presidents .. Frank Tidswell, M.D., Syd., D.P.H., Cantab.; Sydney.
W. Beattie Smith, F.R.C.S. *et* L.R.C.P., Edin.; Melbourne.
B. B. Ham, M.B., Brux., M.R.C.S., D.P.H., Cantab.; Brisbane.
Ernest Black, L.R.C.P. *et* L.R.C.S., Edin.; Perth.
George F. Read, L.R.C.P. *et* L.R.C.S., Edin.; New Norfolk, Tasmania.
James Mason, M.D., Brux., L.R.C.P. *et* L.R.C.S., Edin., D.P.H., Cantab.; Wellington, N.Z.

Secretary .. .. T. Borthwick, M.D., Ch.M., Edin.; Adelaide.

LIST OF PAPERS.

1. Presidential Address.—Some Lessons from the Statistics of Infantile Mortality in Sydney.
2. Sewage Treatment—A Review, by Dr. E. S. Stokes, Sydney.
3. Typhoid Fever in New South Wales, 1898-1904, by Dr. R. J. Millard, Sydney.
4. Summer Diarrhœa in Infants, from a public health point of view, by Dr. W. F. Litchfield, Sydney.
5. A Short Record of Twenty Years' Sanitary Progress in Melbourne, by Dr. J. Jamieson, Melbourne.
6. Sanitary Administration in Adelaide, by Mr. T. Geo. Ellery, Secretary Local Board of Health, Adelaide.
7. A Review of the Public Health Legislation of Australasia, by J. R. Baker, LL.B. (Cantab.), Chairman Health Committee, Adelaide.
8. School Hygiene, by Dr. J. S. C. Elkington, Hobart.
9. Attitude of the State towards Consumption, by Dr. J. M. Mason, Wellington.
10. Notification of Phthisis, by Dr. T. Borthwick, Adelaide.
11. Public Lecture.—Some Aspects of Municipal Sanitary Administration, by Dr. W. G. Armstrong, Sydney.
12. Exhibits, by Dr. F. Tidswell, Sydney:
    1. Collection of formalinised cultures of typical bacteria, yeasts, and moulds.
    2. Collection of photomicrographs of various pathogenic bacteria.
    3. A series of enlargements from photomicrographs illustrating the histopathology of Leprosy.
    4. Photographs of Board of Health Laboratories, Sydney.
13. Photomicrographs of Bacteria, &c. Dr. Andrew Orr, Brisbane.

RESOLUTIONS SUGGESTED.

1. "That in order to secure efficiency in the Sanitary Administration of Australian Cities, is it necessary to have Public Abattoirs, Refuse Destructors, and a Steam Disinfector, as part of their equipment."

2. "That the combination of City and Suburban Local Boards is essential to the effective sanitary administration of Metropolitan Areas, and should be made compulsory in future legislation."

3. "That the compulsory notification of Phthisis is advisable in the interests of Public Health."

4. "That it is desirable to exclude emigrants suffering from Phthisis who are likely to become a burden on the State in the near future."

Section VII.

STATE MEDICINE AND MEDICAL ETHICS.

President .. .. G. A. Syme, M.B., M.S., Melb., F.R.C.S.; Melbourne.

Vice-Presidents .. Sir P. Sydney Jones, M.D., Lond., F.R.C.S.; Sydney.
Andrew Shields, M.D., Edin.; Melbourne.
Hon. C. F. Marks, M.D., Dub., M.R.C.S.; Brisbane.
T. G. Davy, M.A., M.D., Ch.B., Oxon.; Perth.
F. Ogston, M.D., Ch.M., Aberd.; Dunedin, N.Z.

Secretary .. .. A. A. Hamilton, B.A., M.D., Ch.M., Dub.: Adelaide.

LIST OF PAPERS.

1. Opening Address by the President.
Mr. G. A. Syme.
2. Paper—On Hospital Abuse.
Dr. Bryant.
3. Paying Patients in Public Hospitals.
Dr. Moore.
4. Diminished Responsibility in relation to Criminal Law.
Dr. A. Shields.
5. The Role of Provident Medical Associations.
Dr. George Rennie.
6. The Ethical Relations between Town and Country Practitioners.
Dr. F. Allwork.

SERVICE AT THE ANGLICAN CATHEDRAL.

SUNDAY, SEPTEMBER 3RD.

A special service in connection with the Australasian Medical Congress was held at St. Peter's Cathedral on Sunday, September 3rd, in the afternoon. There was a very large attendance, His Excellency the Governor and Lady Le Hunte, the Chief Justice and Lady Way, and the Mayor of Adelaide being amongst those present. The choir was augmented and the organ reinforced by an orchestra to render the music specially arranged for the occasion by the organist, Mr. J. M. Dunn, who conducted. Prayers were read by the Precentor, the Rev. Dr. Milne, and the Dean of Adelaide read the lessons. An effective sermon was preached by Canon Hopcraft from Revelations xix., 6, "The Lord God Omnipotent reigneth."

PROGRAMME OF ENTERTAINMENTS.

Monday, September 4th.

MAYORAL RECEPTION.

The Mayor of Adelaide (Mr. Theodore Bruce) welcomed the Congress at an "At Home" in the Town Hall, on Monday afternoon. Several hundred guests, including members of the medical profession, their wives and daughters, as well as many other prominent citizens of Adelaide, were received by Mr. and Mrs. Bruce and the Misses Bruce. Shortly after the arrival of Lady Way (the wife of the Lieutenant-Governor) the National Anthem, played on the large organ, announced the arrival of the vice-regal party, which consisted of His Excellency and Lady Le Hunte, the Hon. Victor Hood (Private Secretary), and Miss Swete. Afternoon tea was served in the banqueting hall, and musical selections were given at intervals by Mr. Pybus (City Organist) and a large orchestra.

Tuesday, September 5th.

GOVERNMENT HOUSE GARDEN PARTY.

The perfect weather of the previous Sunday, which changed for the worse on Monday, was still more unsettled when the large number of invited guests were bidden to Government House on Tuesday afternoon. Fortunately the rain held off until the close of the entertainment, and intervals of sunshine gave favorable opportunities for exploring the vice-regal gardens and listening to the music of an excellent band. Refreshments were served upon the lawn, in large marquees.

Tuesday evening was set apart by the Reception Committee for private dinners and house parties. About half of the visitors and the honorary members of Congress were entertained at dinner at the Adelaide Club, on the invitation of its medical members, the club dining-room not being large enough to accommodate the whole number at one time. His Excellency the Governor, a patron of the Congress, was present. The remaining visitors were entertained at private houses.

Wednesday, September 6th.

VISIT TO THE CONSUMPTIVE SANATORIA IN THE HILLS AND TO THE NATIONAL PARK.

Motor cars belonging to local medical men and four five-horse drags conveyed visitors from the University to Belair, where, within a third of a mile from one another, near the National Park, are erected two sanatoria for consumptive patients. One, Nunyara, is a private hospital; the other, Kalyra, a public institution, built by the "James Brown Trust." The visit was so arranged that all could inspect both establishments. Dr. Gault showed visitors over the first-named institution, while Dr. Reissmann (medical superintendent) and some of the trustees conducted the party over the latter. Refreshments were provided at each place, and all were invited to afternoon tea at Dr. T. K. Hamilton's residence, at Belair. A drive through the National Park provided occupation for the ladies and others not interested in sanatorium treatment.

EXCURSION TO THE OUTER HARBOR.

For visitors not interested in sanatoria, an excursion on the Port River was provided. A special train conveyed a couple of hundred guests through

Port Adelaide and, by the new branch line, to the Outer Harbor works now in progress at Light's Passage. After an inspection of this large undertaking the visitors were invited by Dr. and Mrs. Morris, of Port Adelaide, to return by steamer up the Port River. The Quarantine Station was briefly inspected *en route*, and throughout the afternoon refreshments and music were provided on the launch, which reached the wharf about dusk. Hence a special train conveyed the visitors to Adelaide.

THE PRESIDENT'S RECEPTION.

Nearly 600 guests accepted the invitation of the President and Mrs. Stirling to a reception and ball, at the Elder Hall of the University, on Wednesday evening. This was a brilliant success, the decorations, carried out in excellent taste in yellow and orange artificial flowers and natural foliage, by the Misses Stirling and lady friends, transforming the fine hall into a strikingly pretty ballroom. His Excellency the Governor and Lady Le Hunte, the Chief Justice and Lady Way, and some members of the Government were present. Songs and instrumental music were rendered during the reception, and dancing followed, supper being served in two large classrooms in the basement.

THURSDAY, SEPTEMBER 7TH.

VISIT TO THE SEWAGE FARM AND RAILWAY WORKSHOPS.

A special train conveyed about sixty members and their friends to Islington on Thursday afternoon. Under the guidance of the Hydraulic Engineer (Mr. C. A. Bayer) about twenty visitors alighted at the Sewage Farm, and were shown over the establishment, while the remainder proceeded by train about a mile farther to the Islington Workshops of the South Australian railways. There they were met by Mr. T. Roberts (Chief Mechanical Engineer), and conducted through the buildings by experts from the staff, who explained the various engineering processes. Mr. Roberts entertained the party, joined by those who had visited the Sewage Farm, at afternoon tea, served in the workmen's dining hall, and he was heartily thanked in a brief speech, as was also Mr. Bayer and the Railways Departments.

MRS. SYMONS'S "AT HOME."

Mrs. Symons gave an "At Home" at Pennington Terrace, North Adelaide, to all the lady visitors of the Congress. There was a crowded attendance during the afternoon.

GOVERNMENT HOUSE DINNER.

In the evening His Excellency the Governor entertained at dinner the medical representatives of the other States and the officers of Congress.

DINNER AT THE ADELAIDE CLUB.

Those visitors whom it had not been possible to invite on the previous Tuesday were entertained at dinner in the Adelaide Club, on the invitation of its medical members.

MRS. C. E. TODD'S BALL.

At the North Adelaide Institute, on Thursday evening, Dr. and Mrs. Todd gave a ball in honor of the members of the Congress. The hall was gracefully decorated, and a large number of guests were present.

FRIDAY, SEPTEMBER 8TH.

VISIT TO THE CREMATORIUM.

Vehicles were provided at an early hour to convey those interested to the Crematorium, at the West Terrace Cemetery, to witness the incineration of a sheep. Mr. Owen Smyth (Superintendent of Public Buildings), who had

invited the visitors, met them at the entrance and showed them over the building. Everything having been previously prepared, the sheep's carcass was introduced into the furnace and soon reduced to ashes, the remains, contained in a small urn, being exhibited at the University on the following day.

MRS. SIMPSON NEWLAND'S GARDEN PARTY.

On Friday afternoon the majority of the visitors attended the garden party given in their honor by Mr. and Mrs. Simpson Newland, at their home, "Undelcarra," at Burnside. There was a large attendance.

SEPTIC TANKS AT GLENELG.

On the same afternoon about fifty visitors went by train to Glenelg. Vehicles were waiting at the station, which conveyed them about a mile and a half out of the town to the septic tanks recently constructed in connection with the new drainage system of Glenelg. Here the Hydraulic Engineer (Mr. C. A. Bayer) met the party and showed them over the establishment. On the return journey the visitors were driven along the esplanade through the seaside town to the residence of Mr. and Mrs. Richard Smith, who had asked a large party to meet them.

PUBLIC HEALTH LECTURE.

At the Town Hall, on Friday evening, a public health lecture was given by Dr. W. G. Armstrong, of Sydney, to a large and appreciative audience, which included many local municipal officers and officials. The subject was "Some aspects of municipal sanitary administration."

THE CHANCELLOR'S CONVERSAZIONE.

On Friday night a conversazione was given by the Chancellor of the University (Sir Samuel Way) to more than 600 guests. After the host and hostess had received their guests in the Elder Hall a musical programme was carried out, while at the same time lecturettes were given in an adjoining lecture theatre by Dr. John Thomson, of Brisbane, and Mr. T. G. Ellery (Town Clerk of Adelaide). On each occasion His Excellency the Governor and Lady Le Hunte were amongst the audience.

SATURDAY, SEPTEMBER 9TH.

EXCURSION TO HAPPY VALLEY.

At noon on Saturday about 150 guests, in ten large drags, journeyed to the Hon. George Brookman's farm, "Glenthorne," near Happy Valley. The drive of an hour and a half's duration was very pleasant, and on arrival at the farm Mr. and Mrs. Brookman had lunch awaiting in a large marquee. Afterwards many availed themselves of the opportunity to inspect the valuable stud on the estate. Rain then, unfortunately, came on, but this did not prevent about sixty guests from going about two miles farther to inspect the Happy Valley reservoir and waterworks.

EXCURSION TO MOUNT LOFTY.

Several drags conveyed the remainder of the visitors to Mount Lofty on Saturday afternoon. Afternoon tea was kindly provided at Mr. J. W. Bakewell's residence, near the summit, by his daughter, Mrs. O'Leary, of Glenelg. Unfortunately it became very wet during the afternoon.

DAILY LUNCHEON AT THE UNIVERSITY.

With the view of economising time, lunch was provided, daily, at the University for members of the Congress; the hosts on these occasions being Dr. Symons and some South Australian country members.

Australasian Medical Congress.

SEVENTH SESSION,

HELD AT THE BUILDINGS OF THE UNIVERSITY OF ADELAIDE,

SEPTEMBER 4th to 9th, 1905.

FIRST BUSINESS MEETING, SEPTEMBER 4TH, AT NOON.

THE PRESIDENT (Professor E. C. Stirling) occupied the chair, and was supported by the Hon. G. H. Butler, M.L.C., Drs. John Thomson, F. C. Batchelor, J. C. Verco, the General Secretary (Dr. B. Poulton), the Treasurer (Dr. W. T. Hayward), and the Associate Secretary (Dr. J. B. Gunson).

THE PRESIDENT: Members of the Australasian Medical Congress—My first duty should be to thank you very sincerely for the distinguished honor you have conferred on me by electing me to this Presidential Chair; but, if you will permit me, I will postpone my thanks until this evening, when I hope that I may be able to make a more suitable acknowledgment than I do at the present time. Therefore, gentlemen, I propose that we should pass to the business of the day, and commence the work of the Congress. (Cheers.) My first duty is to declare this Congress duly constituted, and, in the second place, I will ask the Secretary, Dr. Poulton, to read the report of the Executive Committee. (Cheers.)

THE SECRETARY read the report of the Executive Committee, as follows:—

REPORT OF EXECUTIVE COMMITTEE TO CONGRESS.

At the last meeting of the Intercolonial Medical Congress of Australasia, held in Hobart during February, 1902, under the presidency of the Hon. G. H. Butler, M.L.C., it was decided—"That the name of Congress be altered from 'The Intercolonial Medical Congress of Australasia' to 'The Australasian Medical Congress,' and that the succeeding session be held in Adelaide in 1905."

On the motion of PROFESSOR ALLEN, M.D., Professor E. C. Stirling, C.M.G., F.R.S., M.D., Cantab., F.R.C.S., was unanimously elected President.

At a representative meeting of the South Australian Medical Profession held on May 29th, 1902, the following gentlemen were elected by ballot as the Executive Committee of the Australasian Medical Congress to be held in Adelaide in 1905:—

Dr. W. T. Corbin	Dr. M. J. Symons
" W. Anstey Giles	" C. E. Todd
" J. B. Gunson	" J. C. Verco
" J. A. G. Hamilton	Professor Watson
" R. H. Marten	

Dr. W. T. Hayward was elected Honorary Treasurer, and Dr. B. Poulton General Secretary.

The Executive Committee, so elected, added to their number Drs. T. Borthwick, R. Brummitt, Cavenagh-Mainwaring, W. L. Cleland, A. H. Gault,

A. A. Hamilton, T. K. Hamilton, R. E. Harrold, Melville Jay, A. A. Lendon, A. M. Morgan, B. Smeaton, A. E. Wigg, W. B. Aitken, John Johnson, O. W. Smith, J. T. Sangster, H. A. Powell, C. H. Souter, M. P. O'Leary, T. James, E. L. Archer, R. W. Stewart, J. H. G. Drummond, J. H. Evans, R. St. Mark Dawes, E. W. Morris, F. S. Hone, and F. Goldsmith, and constituted themselves "The General Congress Committee."

This committee determined the division of Congress into seven sections, and appointed a secretary for each section. It also took preliminary steps for securing Presidents and Vice-Presidents of Sections, and Secretaries of Congress, in the several States, and appointed Dr. J. B. Gunson Associate Secretary; and subsequently, in August, 1902, nominated a standing Executive Committee for the conduct of all ordinary business of Congress consisting of:—

EXECUTIVE COMMITTEE.

The President	J. A. G. Hamilton
The Treasurer	T. K. Hamilton
The General Secretary	Melville Jay
The Associate Secretary	A. A. Lendon
T. Borthwick	R. H. Marten
W. R. Cavenagh-Mainwaring	H. Swift
W. L. Cleland	M. J. Symons
T. W. Corbin	C. E. Todd
W. Anstey Giles	J. C. Verco
A. A. Hamilton	Professor A. Watson

The Executive Committee has been cordially supported in its labors by many leaders of the profession in the various States, and has, in accordance with precedent, elected Presidents and Vice-Presidents of Sections from all the States except South Australia. Their Excellencies the Governor-General of Australasia, the late Governor-General, and the Governors of the several States of the Commonwealth, and of New Zealand have, with the Lieutenant-Governor of South Australia, graciously consented to become Patrons of the Congress which will be inaugurated by His Excellency Sir George R. Le Hunte, Governor of South Australia.

The Government of this State has very generously consented to print the transactions, and offers every facility to members for visiting Government institutions and establishments. Government has also, at the instance of the Executive Committee, secured the appointment by the other States of Government representatives in the persons of Dr. Ashburton Thompson (New South Wales), Dr. John Thomson (Queensland), Dr. T. H. Lovegrove (Western Australia), Dr. Elkington (Tasmania), and Dr. Mason (New Zealand). Victoria did not appoint a representative.

Dr. James Jamieson has been specially appointed to attend as representing the Melbourne City Council, and Dr. W. G. Armstrong by the city of Sydney.

Matters closely concerning Military Medicine and the Health of Railway and other Officials were brought under the notice of the Hobart Congress; and facilities will be offered during the present sitting for special meetings of Army Medical Officers and members of the profession connected with the Railway Departments.

The Committee have been most ably assisted by the advice and support of the State Secretaries, who were chosen so long ago as December, 1902, and who, since then, have continually worked to promote the success of this meeting. They are as follows:—Drs. A. A. Palmer, G. A. Syme, Wilton W. Love, A. J. H. Saw, Gregory Sprott, and J. O. Closs.

The editors of the *Australasian Medical Gazette* and *The Australian Medical Journal* have constantly aided Congress in communicating with the profession. The committee wish gratefully to acknowledge the eoncessions given to members by the various State Governments with reference to special railway fares, and by the South Australian Railway Department in allowing them to travel in this State at single fares for double journeys. They desire also to record their appreciation of the generous allowances made by the shipping companies; and especially their sense of indebtedness to the Council of the University of Adelaide for the use of the greater part of these buildings, and the cordial sympathy extended by them to the work of Congress.

E. C. STIRLING, President.

September 4th, 1905. BEN. POULTON, General Seeretary.

DR. POULTON: I move the adoption of this report.

DR. J. C. VERCO: I seeond that.

The motion was carried.

THE BALANCE-SHEET.

THE PRESIDENT: I ask for the Treasurer's report.

THE TREASURER: It is the duty of the Treasurer to report on the balance-sheet with which he has had nothing to do. I am really giving the report of the last Treasurer, Dr. Wolfhagen.

THE INTERCOLONIAL MEDICAL CONGRESS.—HOBART SESSION, FEBRUARY, 1902.

In Account with the Hon. Treasurer.

CR.	£	s.	d.
By Subscriptions—			
318 members at £1 1s. ..	333	18	0
" Remittance from Treasurer Brisbane Congress	109	19	4
" Proceeds Sale of Typewriter (Brisbane)	2	14	0
" Exhibitors................	17	5	0
" Savings Bank Interest......	5	8	8
	£469	5	0

DR.	£	s	d.
To Expenses Local Branches—			
New South Wales £8 7 1			
Victoria 7 8 6			
South Australia.. 3 7 0			
New Zealand.... 2 9 0			
Queensland 2 1 0			
	23	12	7
" Expenses, Hobart..	306	16	0
" Remittance to Treasurer Adelaide Congress	100	0	0
" Balance Remitted to Treasurer Adelaide Congress ..	38	16	5
	£469	5	0

Audited and found correct. J. E. WOLFHAGEN, Hon. Treasurer.

F. B. RATTLE, Auditor.

In moving the adoption of the balance-sheet, I congratulate the Hobart Congress on the great success they had. They not only sent to us the amount of money they reeeived from Brisbane, but exceeded it by about £30. (Cheers.)

DR. BEESTON (Neweastle): I second the adoption of the balanee-sheet.

The motion was earried.

DR. ODILLO MAHER.

THE PRESIDENT: Before we pass on to the next business I should like to make reference to the regret we experience at the absenee of Dr. Odillo Maher. He, unfortunately, has been prevented from being present by a family affliction, and we regret his absence from our meetings. (Hear, hear.)

Congratulations.

The President: I should also like to communicate to the Congress that there has been received from the late Mayor of Hobart (Mr. G. Kerr), a telegram—which reads as follows:—

> "Mayor of Hobart congratulates President, and wishes Medical Congress now assembled successful deliberations."

Those who were at the Hobart Congress no doubt will remember that they were greatly indebted to the mayor for the interest which he took in its proceedings, and particularly for the hospitality which he, and those connected with him, extended to all the members. (Cheers.) If any member of the Congress has any motion of urgency, now is the time to bring it forward.

Sectional Resolutions.

Dr. W. T. Hayward (Adelaide): I suppose at this Congress, like previous ones, we shall have resolutions brought up from the different sections. I have been struck with the apparent incongruity of Congress passing resolutions on the last day, when everybody is hurrying away to an interesting picnic or something of that kind, and those resolutions going forth with the imprimatur of Congress, without consideration from the Congress as a whole. It would be much better if the sections which pass these resolutions took the responsibility of them. If they did that, the resolutions would, I believe, stand a very much better chance of being carried out; because they would be dealt with by the energetic secretaries of the sections, instead of being left to the Executive Committee of the next Congress, which would probably not be appointed for some two or three years afterwards. I was rather amused on looking over the Congress report the other day, in order to find what was done in the other States, to see that I had been appointed a member of a committee to consider certain subjects. That was the first intimation I had that I had been appointed. In these circumstances I propose:—

> "That all resolutions adopted by sections shall be reported to the Congress, but that discussions shall take place only on such as have been specially recommended by the section for adoption by the Congress."

I admit that there are certain resolutions that require the whole weight of Congress behind them. These, I think, should be adopted by Congress, but a great many of them are technical, particularly the Eye Section and Public Health, which I think would be better left to be dealt with by the section themselves.

Dr. Gregory Sprott (Tasmania): I second that.

Dr. Anstey Giles (Adelaide): Is it in order that this motion should be put without notice?

The President: Perhaps Dr. Giles is right, but there is some reason for it, and I think it is in the power of the chair to admit it. If we discuss it now, we shall have clear instructions for next Saturday's meeting, when the real motions will come up. I therefore rule that it is in order, and can be discussed now. I invite discussion on the point. It appears to me to be perfectly reasonable.

Dr. Kent Hughes (Melbourne): Would it not be better to refer the matter to the Executive Committee, rather than to the sections? A section might think a subject of more importance than would the Executive Committee.

The President: Dr. Hayward's motion intends that some things shall go on to the records of the Congress, as the opinions of the sections, but there

may be other things that should be adopted as congressional resolutions. It leaves the matter perfectly within the power of the section to recommend it to the Congress.

The motion was carried.

THE PRESIDENT: It is now my duty to adjourn this meeting until 10 a.m. on Saturday. Will you please allow me to say that it is impossible, in a gathering of this kind, that there should not be some errors of omission and commission. Those who have been in the position of the General Executive of these Congresses will have experienced this difficulty when dealing with a large number of visitors. Disappointments are bound to take place in the arrival of so many visitors, and it is easy for the committee to make very regrettable errors in the distribution of invitations. Therefore, on behalf of my colleagues and myself, I beg of you, if there have been such omissions—and we recognise that there have been—that you will be good enough to communicate the same to either Dr. Poulton or Dr. Gunson, and we shall have every regret that the mistake has been made, and make every effort to remedy it as soon as possible.

The meeting was adjourned until Saturday morning.

THE INAUGURAL MEETING.

OPENED BY HIS EXCELLENCY THE GOVERNOR.

The inaugural meeting of Congress was held in the Elder Hall, University Buildings, on Monday evening. There was a large and distinguished audience in the body of the hall, and among those on the platform, in addition to the President (Professor Stirling), were His Excellency the Governor (Sir George Le Hunte), the Chief Justice (Sir Samuel Way, Bart., Chancellor of the University), the Premier (Hon. T. Price, M.P.), the President of the Legislative Council (Sir Lancelot Stirling), the Speaker of the House of Assembly (Sir Jenkin Coles), the Mayor of Adelaide (Mr. Theodore Bruce), the Vice-Chancellor of the University (Dr. Barlow), the Leader of the Opposition (Hon. R. Butler), and members of the Executive of the Congress, and of the Council and Senate of the University.

THE PRESIDENT: Ladies and Gentlemen—In the name and on behalf of the Australasian Medical Congress, I ask His Excellency the Governor to declare open, this, its seventh session. (Applause.)

HIS EXCELLENCY: Mr. President, your Honor, Mr. Premier, your Worship, Ladies and Gentlemen—Before I formally declare this session open I wish, on behalf of His Majesty the King and the State of South Australia, to extend to all those visitors who have come to the Congress a most hearty welcome. (Applause.) You had a welcome tendered to you yesterday in that most magnificent address which you heard from the pulpit in the Cathedral by Canon Hopcraft, and it is my great pleasure and privilege to repeat his welcome to you. About six years ago I had the honor of being present at your Congress in Brisbane, when Lord Lamington was Governor of Queensland. He began his address to you by apologising for the weather. I am very sorry that I have almost to follow in His Lordship's footsteps. However, that is a matter which is beyond our control, and even beyond the control of that excellent Executive Committee who have worked out so well and so fully the details of the Congress for you. I am sorry for one thing—that the time of the Congress was obliged to be fixed just at this particular period of the year, because had you come here a little later, we would have been able to show you a wealth of color of flowers in our gardens which just now are at their very worst. Adelaide is at all times worth visiting and seeing, but

when it has its summer garb, or as our commercial friends would say, "when we open up our summer lines," there is no city in the Southern Hemisphere which, at any rate, could surpass it. But though the warmth is not on the surface, it is in our hearts towards you. (Applause.) It is a very pleasing moment for South Australia, that after eighteen years the cycle of time should have brought around this Congress to where it was first born. Those who took part in that Congress, and were instrumental in giving it its first start, must feel extremely gratified at the results of their efforts and their foresight. I will not keep you any longer now, because others have to speak, and we are looking forward to the address of our able President, Professor Stirling. I now have the honor to formally declare this seventh session of the Australasian Medical Congress duly open. (Applause.)

THE PREMIER : Your Excellency, Mr. President, Ladies and Gentlemen—It affords me great pleasure to perform my portion of the proceedings this evening, and welcome the Congress to South Australia. It falls to my lot because at the present time I happen to be in the position of Premier of the State. When I got that position I never thought I should have to appear on a platform like this, before such an audience. I am sure I could get plenty of men who could perform this part of the programme better than myself, but I want to assure you that, although they may be able to do it better, none could do it more heartily than I do it now. (Cheers.) The people of South Australia welcome you here because we believe that you are engaged in a work that is to the advancement of humanity generally, and because it is not a close movement—this great movement of medicine. It is a movement on behalf of mankind generally. We are all indebted to you for the amount of work you put into it, and for the sacrifices you make. I want to say to-night that we have much to thank the medical profession for—in performing their duties in attending to the wants of society—especially among the poorer classes of the community. I know it from my own experience that the doctors in the poorer parts of the cities are men who are nobler than missionaries, and greater even than those, in my opinion, who occupy our pulpits, because they put practical work into their lives. I am sure the same feelings animate the whole of the medical profession throughout the world. I have known within my own knowledge of a medical man who paid the most minute attention to cases where he never expected to get a farthing out of them. That comes close to my home. I remember my mother being afflicted with a certain trouble, and a doctor in Liverpool took great interest in her. For five or six years he attended to her, and at last had the satisfaction of seeing a perfect cure. I remember when she was told that she was not to go any more. And I remember also the scene at home. and the thought also of how much we would have to pay, and how we were going to pay it. I remember the answer that came back—"Do you think two guineas is enough ?" And there was rejoicing in that home. As a rule the honest poor are very anxious to pay their liabilities. When there is generosity to them, as in the case I have mentioned, there is rejoicing. The medical profession are blamed sometimes for charging too much. and thank God, some of them know where to charge and where not to charge. I hope that you will be able to discover not only new means of treating disease. but means of making the people pay you when you have done your work. (Laughter, and cheers.)

THE MAYOR : Your Excellency, Mr. President, your Honor, Ladies and Gentlemen—In the position I hold to-day it affords me great pleasure to welcome the members of the Congress to South Australia, and particularly to the city of Adelaide. The citizens of Adelaide are particularly proud of and pleased with the town in which they live. We look upon it as being

de facto one of the most healthy towns in Australasia, and the credit of that to a great extent is due to the number of very able medical men we have in our midst. (Cheers.) The doctors in South Australia are, I believe, such a very excellent lot, and so worthy of imitation, that I have not the slightest doubt that those gentlemen who have arrived from the other States will carry away with them a great deal of information that they otherwise would not be able to obtain. (Cheers and laughter.) I am extremely pleased to know that the first Medical Congress was held in the city of Adelaide, and was presided over by Dr. J. C. Verco. whom I am glad to see on the platform to-night—(Cheers)—and of whom it might be said in the words of the immortal bard—

Time cannot wither nor custom stale his infinite variety.

I remember going to Victoria, and consulting a medical man there for a complaint, and he told me that it was unnecessary for me to go there, because we had medical men over here who could combat equally with those in the sister State any disease known in the cyclopœdia. I hope during your stay in Adelaide you will have a good time. I believe a good many fetes and entertainments have been provided for you, and I hope the weather will hold up. so that you may have a really enjoyable time. I am perfectly sure, knowing as I do know so many members of your profession, that your deliberations will be fraught with and provocative of doing a vast amount of good to the people whom you desire to benefit. There is no profession among the learned professions that I admire so much, or in which I take so much interest, as the medical profession. It is *the* one, if I may say it, except the Church, that has for its object the raising of humanity, the amelioration of suffering, and the assistance of mankind generally. I hope and trust you will have a good time, and that your deliberations will be provocative of a great amount of good to the honorable and learned profession of which you are all such distinguished members. (Cheers.)

PRESIDENT'S INAUGURAL ADDRESS.

The President of the Congress (Professor E. C. Stirling, C.M.G., M.D., F.R.C.S., F.R.S.) then delivered the inaugural address on "Medical Science and Social Problems":—

Your Excellency, Members of the Australasian Medical Congress, Ladies and Gentlemen.—The first and very pleasant duty that falls to my lot as President of this Congress is to welcome, in the name of the medical profession of South Australia, our brethren from beyond our borders. If these, to some extent, still divide us politically, yet they form no barriers to the confraternisation of the members of the great army of medical science whose common object is the relief of suffering and abolition of disease. On our visitors do we, in this numerically small State, very greatly rely for the success of this meeting which they have honored by their presence. Though South Australia cannot, indeed, offer the scenic attractions and advantages of some of the other States, yet we, its citizens, like to think that we are not inferior to our neighbors in the warmth of the personal and social welcome we endeavor to extend to those who favor us with their too infrequent visits. On such an occasion as the present we may be relied upon to do our best to uphold what reputation for hospitality we may be thought to possess. May I also be the mouthpiece of the Congress in welcoming to this meeting all those who, though not belonging to the medical profession, yet have shown their interest in our proceedings and in the cause we represent by being present at a gathering which can scarcely have appealed to them as a recreationary attraction.

For my second duty the more difficult task presents itself of acknowledging, in an adequate manner, the compliment which has been conferred

upon me by my election to a presidential chair that has been occupied by some of the most distinguished members of the profession in Australasia. I take this earliest opportunity of expressing my deep appreciation of the highest honor which lies in the power of my medical colleagues to bestow on one of their number. May I be found as worthy of their confidence as were those who have preceded me in the dignities of the position, in its duties and, will not all my predecessors agree with me if I add, in its great anxieties and responsibilities. In this particular instance such an acknowledgment almost requires the inclusion of an apology, seeing that I, standing for some years outside the active practice of our profession, feel myself in some respects unqualified to address my experienced colleagues on many matters which lie nearest to their thoughts and work. Instruction or inspiration is not in my power to offer, but I console myself with the thought that there may, perhaps, be some propriety, or even advantage, in an arrangement whereby, every now and then, the opportunity of speaking *ex cathedra* should be sometimes given to those whose daily work consists in the consideration of the healing art rather from the point of view of its underlying principles than from that of their adaptation to the requirements of actual practice.

This, your Excellency, ladies and gentlemen, is not an ordinary anniversary; I am glad to think by the faces I see before me that there are many here this evening who need no reminding that the Institution of Intercolonial Medical Congresses, to give them their original name, was founded in South Australia, and that in Adelaide, in 1887, was held the first of the series of meetings that have continued held uninterruptedly ever since to the great benefit of ourselves and, I would fain think, of our calling. Now, after these eventful years it returns to us for the second time. It is pleasant to see here, older, by those eighteen years that have passed since he held office, but still in the prime of intellectual life, the first President of the first Congress—Dr. Verco. So also do we welcome those other past Presidents—Dr. Batchelor, President of the New Zealand Congress in 1896; Dr. John Thomson, President of the Queensland Congress in 1899; and Dr. Butler, President of the Tasmanian, and last meeting, held in 1902, who, in their respective States, have so worthily upheld the dignity of their country and their profession as to make their example difficult of emulation for their successors.

Still with us, also, is the real originator of the system of Congresses, Dr. Poulton, the Secretary of the first meeting, as he is again of this. Neither has he lost his superabundant energies of eighteen years ago, which, to our great advantage, he now, once more, places at our disposal. And other distinguished participators of the first Congress I see, now become the Nestors of their profession, whom we gladly welcome to Adelaide for the second time.

How, then, shall I use this, an opportunity that comes once only in a man's lifetime, when he may address not only his assembled colleagues, but those now numerous members of the public to whom the widespread dissemination of scientific information has brought some knowledge of the romance of medicine and surgery.

On occasions such as these, which serve as milestones in our lives and in our profession, it is a time-honored custom for him who stands in the position which I have the honor to occupy to-night to review the progress of that particular branch of the profession with which his life's work has been identified. For one whose special interests were, for more than a quarter of a century, centred in the practice and principles of surgery, it is a tempting theme to exalt the horn of his calling by dwelling on its phenomenal advance during a period of which his own student days saw the beginning. But that has been an oft-told tale; it has been told by some of those who themselves

were in the forefront of that triumphant march which has brought surgery to a pitch of success undreampt of even only half a century ago, and I need not repeat it.

The thaumaturgic proceedings of the operating-room have, indeed, always appealed to the popular imagination, and their results have become perfectly familiar to the intelligent public, either as the outcome of their own personal experience or of the general spread of information which is one of the characteristics of this age of publicity. Indeed we know how each remarkable discovery is served up to us by the daily press on the morning following its announcement in the country of its origin.

The brilliancy of the results of surgery not infrequently gives rise to disparaging comparisons between its progress and that of its less spectacular sister, the science of medicine. But this disparagement is not wholly justified, for medicine, if it has been later than surgery to respond to the vivifying influences of the ancillary sciences, has now at last entered upon an era of remarkable discovery pregnant with infinite possibilities for the future.

If surgery, as a craft, has, in the period in which we live, reached its golden age, it is possible to predict with great assurance that the present century will witness an equal if not greater advance on the part of medicine and its collateral branches. Indeed, as we can plainly see, that advance has now begun. Already the new therapeutics and the new science of preventive medicine has accomplished unexpected results, while offering boundless promise of achievements in the future.

About thirty years ago a distinguished English surgeon, a leader of the profession, gave it as his opinion that operative surgery had nearly, if not quite, reached its ultimate limits, and that certain regions of the body must perforce remain sacred from interference. But a great deal of water has flowed under the bridge since that time. Surgery has extended its scope far beyond what then appeared possible. Almost every organ of the body, including such important structures as the spleen, kidneys, lungs, liver and its appendages, the brain and spinal cord, are now successfully invaded by the surgeon's knife ; some of them are even entirely removed. The heart itself, the centre of the circulation, the slightest wound of which was, not so long ago, thought to be a fatal injury is not exempt from interference, for there have been cases in which wounds of this organ have been successfully sewn up ; the feasibility has even been seriously considered of removing the obstruction to the circulation caused by the disease of its valves, which as experimental physiology has shown, need not be regarded as impracticable. Long lengths, to be measured in feet, are, with impunity, cut out of the intestines and the continuity of the remainder restored. The stomach itself, that long-suffering organ than which, as has been well said by a surgical cynic, no ass is more heavy-laden, has, by its successful removal, been proved to be one of those useless superfluities of which not a few encumber our far from structurally perfect bodies. But it must be clear that in a finite body there must be some limit to our interference with organs on the adequate integrity of which the maintenance of life depends, and, if we have not yet quite reached those limitations, we can hardly doubt that, so far as actual removal of parts are concerned, there cannot in this respect be many more worlds to conquer for the surgical Alexander. But, even if this be the case, it is quite possible that surgery may be considerably extended on the constructive side, so that we may be able to engraft, wholly or in part, complex organs as we do now in our transplantations of skin, bone, teeth, and nerve ; and, of course, there is still abundant room for improvement in our methods of operation and means of diagnosis.

May I venture in this connection to emphasise the importance of the too often neglected *minutiæ* of human anatomy. Now that surgical interference is pushed to the utmost limits of corporeal endurance, and almost every doctor considers himself capable of undertaking the most intricate operations, a refined knowledge of the smallest structural details of the human body is more than ever imperative. A due regard to those methods of operation which will ensure the least possible disturbance of parts and the least interference of normal functions may make all the difference between success and failure. But it is astonishing what the human body will endure.

> "Oh! the little more, and how much it is;
> And the little less, and what worlds away!"

To whom shall this appeal with greater and more painful force than to those whose "little less" of knowledge or of skill, that might have been the "little more," has cost a human life.

But after all, though I speak as a surgeon proud of my calling, our craft is, we must admit, in many of its proceedings, a rough and ready sort of business, seeking to cut the Gordian knot of disease rather than to untie it. Though the surgeon's knife will probably remain the *ultima ratio* of treatment of many of those injuries, deformities, and accidents to which our body is subject, partly on account of its own inherent imperfections, yet the discoveries of recent years render it permissible, and by no means visionary, to conceive of more rational and less violent therapeutics than the cutting off, or cutting out, of diseased organs or tissues. Indeed we are even now sometimes able to achieve this object by appropriate medication. Better still, the counsel of perfection would be to substitute prevention for our cures, for indeed there is no reason why the principles of the new science of the preventive medicine should not equally be applicable to the surgical manifestations of disease.

THE OLD AND THE NEW MEDICINE.

After long epochs of slow and gradual growth, interrupted by dark ages of stagnation and superstition, both medicine and surgery have, in our own times, witnessed a remarkable and, relatively to past epochs, sudden expansion, not only in their methods, but in their underlying principles. Between the old and the new medicine there is a difference not merely of degree, but, in a great measure, of kind. What, then, we may ask, is the nature of the new influences which, to so conspicuous a degree, and with a potency exceeding that of past forces, have served both as a stimulus to and as the basis of this sudden bound of progress. It may not be without some interest, even to those outside the profession, if we see what answer can be given to this question.

In ancient Greece, some time during the three centuries that preceded the Christian era, there arose, amongst the physicians of the day, a sect who, because they based their practice on experience alone, to the exclusion of generalisation, reasoning, and analogy, termed themselves the Empirics, this word being the Anglicised form of the Greek word, meaning "one who is guided by experience." Another sect, which flourished at the same time, held experience to be valueless, and founded its doctrine exclusively upon reasoning and theory. These styled themselves Methodists, because they based their practice on a simple method which we might call a "rule of thumb." To this sect Galen, one of the greatest names in the early history of medicine, belonged. A third sect availed themselves of both experience and reasoning, and were called the Dogmatists: thus, even in these early days, there were schisms in the profession, and it is not surprising to learn that between them there was much and bitter controversy. Truly our medical forefathers were very human.

The Empirics were controversially defeated ; the sect died out, and the name Empiric, no doubt from its association with what was considered to be a defunct medical heresy, became a term of opprobrium which connoted quackery, and, as applied to medicine, it still retains much of its sinister meaning.

But if the Empirics as a distinct medical sect died out, their basic doctrine of practice has survived. For long ages empiricism—using the term now in its literal and proper sense—has been one of the principal foundations of our medical practice : to the wise empiricism of our medical forefathers it has owed much, and we must admit that on a method of sound empiricism successful clinical treatment greatly rests, and must continue to rest, as indeed do most of the judgments of our daily lives.

And here let me observe that when we are apt to decry the manner of practice of our forefathers in the exaltation of our own methods, we should remember that it is questionable whether we of the present day are in all respects the equals, in powers of assiduous and attentive observation, of those great masters of the profession who so long ago preceded us. Without the multitudinous mechanical and other aids to diagnosis that we now possess, and that to some extent are substitutes for natural wits, yet these men, by their unaided powers of observation, were often able to form an extraordinarily correct conception of the conditions and course of a disease. Their methods of treatment, indeed, purely empirical though they were, had, in many cases, a success that we have not been able greatly to improve upon. So, then, let us ever remember when we boast of our modern progress that our predecessors of old were not all ignorant, nor always wrong in their methods and judgments, and that they have left us a rich legacy of recorded observations and clinical insight into disease that stand as an example of how much may be achieved by the unaided senses and by natural intelligence wisely applied. In many ways we might well ask for "the old paths, where is the good way, and walk therein."

But so long as our practice continued to be based only upon empirical methods, to the exclusion of reasoned processes which could enable us to recognise the underlying principles of which the observed facts were but scattered expressions, so long could medicine lay no legitimate claim to come within the category of the sciences. To the dignity of that position both medicine and surgery may now, with some justice, lay claim.

No longer does medical practice rest upon a chaos—an incoherent and unrelated aggregate of observed facts ; no longer are we satisfied to know that such and such things are, but we have begun to ask the questions why and how they are, and when we are able to give even a partial answer to these questions our knowledge ceases to be empirical, and becomes rational or scientific, and it is this just in so far as our answers are complete. In other words, medicine no longer contents itself with the knowledge, however accurately determined, or however often recorded, that a disease has such and such a set of symptoms, and that, for some unexplained reason, such and such a treatment is beneficial or detrimental, but it seeks to know what are the intimate causes of the perturbed action, why does it arise, why does it run the definite and constant course which has been observed to characterise it, or which distinguishes it from other kinds of perturbed action, and why should a particular kind of treatment be beneficial or the reverse.

When we are able to give answers to questions of this kind in a sufficient number of corelated cases, we are able to establish certain laws of disease that embody principles which guide us in our practice, and give to medicine not only rational methods but extend infinitely its scope of action. A sound principle, or even a wrong hypothesis that has been rightly used, is worth any number of unrelated facts. *Felix qui potuit rerum cognoscere causas* is just as true for

the physician as for the philosopher; nay, twice happy is the former who can by his knowledge project his own felicity into the bodies and minds of those unto whom he ministers.

It is part of the aims of this address to bring before the notice of those to whom the facts may be unfamiliar some conspicuous illustrations of the wide influence that scientific principles have exerted upon medical science; to show that the phenomenal progress which the last half-century has witnessed in various departments of our profession has been due to the application of these principles, and that what has been achieved already in this direction is only an earnest of further and perhaps greater triumphs in the future. Here, then, is the difference between the old and the new medicine. The old based its methods, often we must admit with great practical success, upon empiricism and irrational hypotheses. The new, while retaining what is good of the old methods, seeks for the deeper and the truer insight into the How and the Wherefore.

THE GERM THEORY.

In these days, when everyone is familiar with the extensive role played by the microscopic organisms, which collectively we call germs, in the causation and manifestations of disease, it almost requires an apology for alluding to so well-known a story. But, as no more striking example can be mentioned of the far-reaching influence that may follow the application of a scientific conception, may I be pardoned if I make some brief allusion to the subject.

Now-a-days everybody, even that much talked of individual the "man in the street," is familiar with the fact that the great successes of surgery have been due to the co-operation of two factors—the use of anæsthetics and the adoption of aseptic and antiseptic procedures. Everybody knows that the object of these methods is, in the former case, to exclude from entrance into open wounds and susceptible surfaces those noxious vegetable organisms, generally found in the neighborhood of man, which, when they do enter, are the causes of the local putrefactions and dangerous infections of the system commonly called blood poisonings. Antisepsis has the less favorable design of destroying the germs if unfortunately they should have already gained entrance. Not only have these aseptic methods permitted a vastly extended scope of surgical procedures, especially in the abdominal region, but they have also led to the practical abolition of those former scourges of surgical practice, hospital gangrene, septicæmia, erysipelas, and other forms of blood poisoning, secondary hæmorrhage, and such like surgical calamities. And if puerperal and its allies still exist more frequently than they should do, it is principally because of the ignorance and want of surgical cleanliness of those obstetric anachronisms—the amateur Mrs. Gamps, who so confidently and so gleefully preside over the occasions which are liable to give rise to this class of complaints.

Now, for the purposes of my argument, the point is that all these great results resulting from the use of aseptic methods may, with much truth, be said to have sprung from a single bio-chemical discovery which, when it was made, had no connection whatever with medicine or surgery, indeed it had reference to the very prosaic and apparently remote question of the making of beer. Nevertheless it was undoubtedly Pasteur's classical investigations into the causes and conditions of fermentation which formed the immediate starting point, and, indeed, the foundation of the aseptic methods of treatment with which the name of Lister will ever be honorably associated. For, applying the principles of Pasteur's discovery to surgery, Lister showed that just as fermentation is caused by the presence and action of certain definite, living, vegetable organisms, so putrefaction in wounds, which is only a special

variety of fermentation, is similarly caused by certain other minute organisms, and he proved also that the same kind of methods which would prevent the one process would prevent the other. Thus the practical question, in respect to surgery, resolved itself, as we have said, virtually into methods for the exclusion from wounds of these organisms, or for their destruction should they have gained entrance. No better example could be given of the world of difference that lies between the knowledge of a fact and the knowledge of its cause. Our forefathers were perfectly cognisant, indeed they had disastrous experience, of the broad fact that putrefactive processes were apt to be set up in wounds, but, being ignorant of the cause, they were powerless to avert the fatal results.

The principles involved in Pasteur's discoveries in the field of fermentation were soon destined to receive a still wider application, for they led to the discovery that many of the diseases we term infectious were due to the entrance into, and operation within, the body of other organisms of a nature allied to those which cause the infection of wounds. And if still there are some infectious complaints in which the specific germ has not yet, with certainty, been detected, we may be absolutely sure that for every such disease there is an organic generator, and, further, there is even much to justify the view that other diseases than those now generally classed with the infectious group, such, for instance, as rheumatism, may be due to similar causation. And need I say that we are ever on the alert to discover a possible germ of cancer.

Nor is the knowledge gained from this generalisation limited in its application to man and his diseases; it has been extended to those pestilences which at times, in certain places and with such fatality, infest the domestic animals; it has been successfully applied to the improvement of economic and manufacturing processes, and, in the light of the same knowledge, we have learned the nature of those chemical changes taking place in the soil by which it becomes better fitted for the nutrition of vegetable crops. The formation of nitrates which are so valuable as fertilisers we now know to be due to the action of vegetable organisms similar to these which produce disease, and when these are absent, or deficient, we can supply them by direct inoculation.

How wide, then, and far-reaching has been the influence of this one bio-chemical discovery of the organic causes of fermentation, and I have but merely touched upon the fringe of an immense subject. Truly the value of a new principle, however limited its application may at first appear to be, is incalculable, and the direction which its influence may take unlimited.

It is, however, clear that these discoveries, great as they are, form but the introduction of fresh problems. It is a great step to have traced the causation of disease to specific organisms which are tangible entities that may be seen, isolated and used as we will for the experimental confirmation of our theories. But our canon requires that causation should be pushed still further back, for there arises the next question—how and why do these germs produce their effects? Into this domain of inquiry we have now begun to penetrate. But as these considerations involve questions of somewhat greater complexity, I shall most conveniently bring them under your notice in a concrete form. And, in doing so, may I seek once more the indulgence of my medical brethren for dealing with matters which to them are so familiar.

TOXINS, ANTITOXINS, AND IMMUNITY.

Diphtheria, one of the oldest known of infectious disorders, is caused by specific living organisms which, as a rule, gain entrance into the body through the nose or throat. Settling on some part of these regions, they

rapidly multiply and, at the same time, secrete a poisonous substance or toxin. This entering the circulation is distributed throughout the body, and poisons the tissues by interfering with the normal processes by which they maintain their vitality. In short, a *contagium animatum* from without produces a *virus inanimum*—a lifeless poison which does the mischief within. Now it is found that if some of this diptheria toxin which, up to a certain degree of purity, can be obtained separately by growing the diphtheria germ in suitable culture mediums outside the body be repeatedly, and in successively increasing doses, injected into the blood of a horse, this animal becomes so affected that it acquires, at length, the capacity of withstanding, without symptoms, a dose of the toxin that would, without preliminary treatment, inevitably have killed it. In other words, using the terminology of the day, we say that the horse has been made immune to this special diphtheria poison. In this condition the serum, or fluid part of the horse's blood, is found to contain a substance which, because it is antidotal to or capable of neutralising, diphtheria toxin, is called antitoxin. And when a suitable dose of this serum is injected into the blood of a human being it may act either by preventing an attack of diphtheria—that is, it confers immunity in this respect—or, if the disease has not advanced too far, it acts as a curative agent. From this method of treatment arises the term so much in evidence at the present time—serum therapy; and speaking of this one disease—diphtheria—it may safely be said that by its timely application thousands of lives have been saved.

Now it must be understood that this conclusion as to the antagonism of toxin and antitoxin does not merely rest on the results of trials on the living bodies of man or other animals, but its main facts are based securely on experiments in the laboratory, where it can be shown that if the proper proportions of the two substances are added together in a test tube, the mixture is harmless when injected ; the poisonous toxin has, in fact, been neutralised by the antitoxin.

The case of diphtheria presents us with one of the most satisfactory examples of the remedial application of the antidotal action of antitoxin on a toxin produced by bacterial action. In some other diseases, of which the now comparatively rare disease, tetanus or lock-jaw, may be taken as an example, though laboratory experiments justify similar conclusions as to the manner of formation and interaction of these antagonistic substances, yet the application of this kind of treatment has not so far yielded such satisfactory results as in the case of diphtheria. For this relative failure we can recognise reasons which in no way invalidate the general conclusions, but these it is not necessary to discuss. We find, also, that the toxins arising from bacterial action are not alone in their power of generating antitoxins, for each kind of snake poison will similarly produce its own antitoxin, which, if it can be applied in time, acts as an efficient antidote, and so will ricin, the poisonous element of the castor oil bean, and abrin, the active principle of those bright red and black seeds called crabs eyes or jequirity, both these last-named poisons being far more powerful than strychnine. In all these cases the reaction may be demonstrated in the most certain manner in a laboratory experiment.

Considered merely in the light of a system applicable to the treatment of particular diseases, these facts concerning toxins and antitoxins are of great importance ; but of still greater value are certain underlying principles which have emerged from some recent investigations on this subject, with which the name of Ehrlich will ever honorably be associated, although, as in most other great discoveries, the whole result is not the work of a single mind. The great merit, in this respect, of this distinguished pathologist is that he has reduced

the interactions of toxins and antitoxins, both *intra vitam* and *in vitro*, to principles of molecular chemistry and of chemical physics. He has shown that the interactions in question are subject, both qualitatively and quantitatively, to the known laws that govern chemical action. We are not able, it is true, with our present knowledge, to symbolise the reactions involved in the form of an equation, for we do not know the exact chemical nature of the substances concerned, and, indeed, both toxins and antitoxins have not yet been isolated in a pure condition, but we have evidence that the former, at least, appears to be related to substances closely resembling products formed in the course of ordinary digestion. Consequently it is not improbable that, notwithstanding their poisonous character, they are substances not far removed from these which form the normal food of the body tissues. Antitoxins are believed to belong to a closely allied group.

Ehrlich's theory further refers the chemical actions involved to those cells of the body on which toxins exert their influence ; he shows us how, on grounds quite in harmony with known physiological laws, the formation of antitoxins depends on the power, possessed by the molecules of certain susceptible body cells, to seize hold of, and anchor to themselves, the circulating toxins, and in so doing they are stimulated to an increased activity which causes them to liberate into the blood-stream certain substances which are, in fact, the antitoxins, and these, meeting the toxins, combine with them and neutralise their effects in much the same way as an alkali can unite with and neutralise an acid. Thus in the laboratory of the body are produced the same effects as when two chemical antagonists are mixed together in a test tube.

Great as is the importance of this theory from a therapeutic point of view, it has a still wider significance, for it throws light upon one of the central problems of physiology, viz., the nature of the chemical mechanisms of those normal physiological processes by which the living cells of the body regenerate their substance by incorporating the nutritive materials which form their food, and then again by their activities break down into waste material. And the conception has still wider applications, which will be mentioned directly.

We have seen that in diphtheria and tetanus the deleterious influence of their organic generators depends upon the production of toxins which, indeed, may produce their effects quite independently of the actual presence of the germs themselves ; but we also know that there is another class of diseases in which the effects are due to the actual bodily presence of the bacteria, and not to toxins which may be obtained from them. Of such a class are typhoid fever, cholera, plague, pneumonia, septicæmia, and other forms of blood poisoning, in which diseases serum therapy is still on its trial, but so far with less certain and successful results than in the case of diphtheria. In immunising an animal against this class of diseases the object is to develop in it, not a serum capable of neutralising the toxic produce, but one capable of destroying the actual living bacterial organisms. This is done by using for immunisation cultures containing the bodies of the bacteria themselves, not merely their separated toxins. To the reactions which take place when these bacterial sera, as they are called, are used, Ehrlich applies a chemico-physiological explanation similar to, but more complicated than, that which he gives for the relatively simpler affair of toxins and antitoxins ; but this, again, it is not necessary to discuss.

By pursuing this line of research it has been found possible to produce in animal serums a variety of these antibodies, as they may be called collectively, each of which has some specifically destructive action not only on such organic cells as are represented by bacteria, but upon other kinds of cellular units of complex fluids or organs, such as the red corpuscles of the blood, the cells of the liver, nervous system, and so on ; and here can be seen the ground

for the hope so often expressed, but as yet unfulfilled, that by an application of this principle we may be able to develop a serum which shall have the property of destroying those cells of which a cancer or other malignant growth is composed, while leaving untouched the healthy cells, or, if cancer should be proved to be due to a germ, as some think, we would seek for a serum which will destroy either it or its poison. Koch's tuberculin treatment of consumption, the tragedy of the failure of which we so well remember, was one of the many attempts that have been made to treat this disease on the principles we have been discussing. Nevertheless, now that we know so well that in this and many other complaints we have to deal not with a nameless and unknown something, but with definite and specific organisms, with whose conditions of existence we are perfectly familiar, the hope remains that the defeat in these cases may not be final. Indeed we have good grounds for believing that the organic generator of every disease, once it is known, may be made to produce its own antidote. This surely is a magnificent conception.

Nor even do the results of these investigations stop here, for out of them have arisen new methods, more precise than any others available, for the identification of various bacterial species; means, more delicate than any other test, for detecting the presence of toxins; a means of diagnosis for typhoid fever; a method for the determination, with extraordinary accuracy, of the resemblances and differences between the blood of various species of animals, which is capable of being applied in courts of law for the identification of blood stains. The same methods can be applied also for the purpose of determining the relationships of species and groups of animals by demonstrating the fact that "there has persisted a common property in the blood of various groups through the countless ages that have elapsed since their evolution from a common ancestor, and this in spite of differences of food and habits of life." Lastly, from an exhaustive study of the phenomena of old age, to which an increasing amount of attention has been paid of recent years, the distinguished physiologist, Metchnikoff, whose name is a guarantee that he does not speak as a visionary enthusiast, and who himself has borne a conspicuous part in the investigations that we have just been discussing, has suggested that it may be possible to apply the principles of serum therapy to the arrest of those senile changes which we have been accustomed to believe form the inevitable and distressing concomitants of old age, to the end that we may prolong the usual span of life. Be this as it may, the increasing study devoted to the conditions of old age makes it abundantly clear that many of its painful incidents are of our own making, and, above all others, the deleterious effects of alcoholic excess in bringing about premature senility and decay stand out in malignant relief. In these days, when the questions of serum therapy and immunity loom so large in the foreground of medical science, we may, in the light of recent discoveries, recall the epoch-making discovery of

VACCINATION.

A little more than a hundred years ago, the Englishman, Jenner, recognised what seems at that time to have been common knowledge on the dairy farms of Gloucestershire, that persons who had contracted cow-pox—a mild disease—from their charges did not, as a rule, afterwards contract the virulent disease small-pox, and vaccination is the methodised application of these observations. It rests on the basis that by the introduction of a weakened poison, such as that of cow-pox, which is in effect small-pox modified and mitigated by its passage through the cow, it is possible to establish in the body of man a certain insusceptibility to, or tolerance of, the poison of small-pox, so that the latter disease is either not contracted at all, or, if contracted, it assumes a milder form. Thus vaccination is but a special application of what we know to be a general principle.

I need not dwell on the fact that by the general adoption of vaccination small-pox, which, from the thirteenth century, ranked as the most destructive of pestilences, has been, if not completely eradicated, at least greatly mitigated in its prevalence and effects wherever communities have been willing to avail themselves of the protection afforded by the practice. On the other hand, we have had frequently manifested the power of mischief that may arise from its neglect, and from the policy of indulgence towards that type of person who calls himself a conscientious objector.

You will see why I have recalled this episode of medical history; it is, of course, because the basic principle of Jenner's discovery has received such ample confirmation by the recent investigations in serum therapy and immunity. But, we may ask, why did vaccination remain more than a century as the single example of an immunisation conferred by this method? The broad idea of establishing artificially an immunity for poisons is itself very old, for, as we learnt in our school days, Mithridates, King of Pontus, in the fear of attempts upon his life by poisons, produced in himself a tolerance of their effects by the habitual and continued taking of them in small doses. The term Mithridatism, still applied to the tolerance of poisons thus produced, perpetuates the name of an early practical exponent of artificial immunisation

We may, I think, safely say that the reason why vaccination remained for so long as the sole example of protective inoculation was because it was an emprical discovery in the sense that no underlying principle was discerned which would admit of its explanation or its extension to other cases. The fact of its protective influence was known, and, indeed, was so obvious that the practice of vaccination was adopted by most civilised communities, though, let us regretfully admit, with greater thoroughness by others than by the countrymen of the great Englishman who devised it; but no rational explanation of the cause of the fact was forthcoming, and this is just what Ehrlich's theory attempts to supply. Even if, as is very probable, it will ultimately be found out not to be correct in all its details, it may safely be said that its fundamental principles will never die, and nothing can minimise the stimulating influence of this very remarkable conception that has radiated in so many diverse directions. It is, then, not a little remarkable that, in these days when the principle of Jenner's discovery has been so amply vindicated and extended, and when some of its undoubted original dangers have been removed by better methods of application, that we witness a marked tendency to neglect the protection which the world's experience has shown it so conspicuously affords.

With this wide, and ever widening, scope of the principles involved in the recent theories of immunity, it is no wonder that they occupy so prominent a place in modern medicine. Nor, as we have seen, is their application limited to the processes of disordered function with which medical science has to deal, but it extends towards a better understanding of the normal physiological mechanisms of all living tissues. They illustrate the conception that is being so surely borne in upon us that medical science, like biology, of which indeed it is but a branch, requires for its elucidation the convergence upon it and application to it of fundamental physical and chemical conceptions. No better example could be given of the value to science of a well-ordered imagination and of a good working hypothesis, or of how investigations in one field may open up unexpected discoveries in many and unexpected directions. The dreams of to-day become the realities of to-morrow, and in these theories we have a germinating idea which is destined to grow into a tree of knowledge whose manifold branches will bear abundant fruit.

THE ANIMAL KINGDOM IN RELATION TO DISEASE.

So large is the role played in disease by those minute vegetable organisms that we collectively call bacterial germs, that we are apt to forget that the animal kingdom also provides its contingent of deadly enemies to the human body. Putting aside those grosser forms of parasitism such as hydatids, tapeworms, and the like, which are not as a rule fatal, it is now well known that in another class of diseases—infectious but not contagious—such as the various forms of malaria, yellow fever, and others, the exciting cause is the presence in the body of minute organisms which belong to the lowest class of animals; and, further, that the transmitters to man of these low organisms are to be sought amongst other and higher orders of animals. In the diseases particularly mentioned it is now well established that mosquitos of certain sorts are both the agents in the transference as well as the necessary hosts in which a part of the parasite's life's cycle of development takes place. Thus the problem of the abolition of these diseases resolves itself into a question of the destruction of the breeding-places of mosquitos, which task, apparently at first sight so impracticable, has met with a gratifying success wherever it has been intelligently and persistently undertaken. That terrible complaint, sleeping sickness, which devastates a large belt of tropical Africa, is similarly caused by a minute animal parasite transmitted to man by a Tsetze fly; sleeping sickness is, in fact, the human form of that deadly complaint known as Nagana or Tsetze fly disease, which renders large parts of South Africa uninhabitable for the introduced domestic animals. Other forms of the same fatal animal disease are those known in India as Surra, and in North Africa as Dourine. The agency of animals in respect to the causation of infectious disease is also shown in plague, where there is good reason to believe that rats, and perhaps mice and fleas, act as carriers to man. We strongly suspect house-flies to be agents whereby typhoid fever, cholera, and forms of blood poisoning may be distributed; anthrax and glanders come to us from the horse, sheep, and ox; our common tapeworms through the pig and bovine cattle; and the familiar friend of man—the dog—is, as we have long known, practically the sole source of hydrophobia and hydatids. From the knowledge of these and many other similar facts it has followed, as part of our recent views, that medical science refuses more and more to be bound by the rigid limits formerly prescribed for it, viz., that the human body and its diseases are to be regarded as isolated factors to be considered apart from their organic environment. It has been compelled to recognise, what the zoologist has long recognised, that man is only a unit in the world of living things, inter-related not only in structure but in disease with his animal and vegetable surroundings. While I speak of this subject, perhaps it is not out of place if I utter a word of serious warning in respect to the danger that now confronts Australia from the ease with which some of these fatal diseases of stock, from which we have hitherto been free, may be introduced into this country. Occurring all around us, in South America, Africa, Mauritius, India, Manila, and New Guinea they are, with the frequent and increasing means of communication, knocking, so to speak, at our very doors. And if ever they, or any of them, should, through carelessness or misadventure gain a footing in this land, you may depend upon it that the result will be a destruction of our flocks and herds that is inconceivable to those unfamiliar with their deadly effects in other countries. Ask of Africa what it has lost from nagana, dourine, rinderpest and horse-sickness; India, Mauritius, and the Philippines from surra; South America from mal de caderas; and from the answers let Australia take warning before it is too late. A few hundreds a year spent now, with intelligent and prudent foresight, in securing the services of some competent scientific expert, who has had large and practical

experience of the highly complex and difficult problems which attend the investigation and management of this class of pestilence, might be the means of saving millions of money and untold trouble in the future.

All these discoveries, then, which I have mentioned, with their extensive ramifications, constitute great and important advances not only in the wealth of added facts but also, in what is still more important, the widening of our ideas concerning the nature of living activities and those departures from the normal we call disease. Already they have profoundly influenced our medical and surgical theory and practice, while they are full of hope for the future. Together they have led to the establishment of what is practically a new science—that of preventive medicine, which in its turn has led to the virtual suppression of some diseases and the mitigation of the effects of others. Were it not for ignorance, apathy, and that self-complacency which is so essentially a characteristic of our own British race, the results would be still more marked. Indeed success in this direction has led a cynic to observe that medicine has made prodigious progress in all things except in the treatment of disease, and there is some truth in the sarcasm; but if we are not much better able to cure diseases in the full sense of the term than were our forefathers, we are certainly much better able to prevent them, and we may regard this as the higher and better function.

RATIONAL THERAPEUTICS.

We have, however, also learnt that there are other and wiser methods of treatment than by a multitudinous, and often haphazard, polypharmacy for we have a much deeper knowledge, to which the recent investigations into the conditions of immunity have largely contributed, of how great, how manifold, and how effective are the natural recuperative powers of the healthy body which were long ago recognised in the Hippocratic aphorism *vis medicatrix naturæ.* We have discovered, or at all events we better appreciate, the facts that fresh air, abundant sunlight, change of scene and climate, and more suitable dietaries are in themselves potent therapeutic agents, and not the least important discovery is the realisation that the doctor's best function often is to be simply the *homo minister naturæ.* We have successfully applied the actinic rays of light to obstinate and disfiguring complaints of the skin, and we have invoked, with as yet uncertain success, as therapeutic agents, some of those mysterious and potent electrical and radioactive emanations placed at our disposal by the extraordinary developments of physical science, which bid fair to exhaust the Greek alphabet in their provisional nomenclature. By the researches of the synthetic chemist we are able to use medicaments of absolute purity and known chemical composition, instead of crude drugs of varying and uncertain contents; while a more extended knowledge of molecular structure has directed attention to a possible connection between chemical constitution and physiological action which must eventually lead to a more accurate and scientific therapeusis.

That comparatively recent development of physico-chemical science known as the ionic dissociation theory, which gives us a rational explanation, up to a certain point, of such intricate living actions as the contraction of ordinary muscles and of the rhythmic and ceaseless beat of the heart, the transudations of physiological fluids, and has even been applied to the explanation of that mystery of mysteries the fertilisation of the egg cell, is similarly applicable to the chemical mechanisms involved in the action of drugs, germicides, and disinfectants, as well as to many of the normal activities of the living body. Again we come back to the fact that the more we learn of living actions the more do we find that they may be referred to the same laws of physics or chemistry which govern the relations of inert matter.

MEDICAL SCIENCE IN RELATION TO THE COMMUNITY.

But while we may contemplate with much satisfaction these great advances in medical science, let us not forget that the ledger of national life has its other side, which is not so flattering to our pride of progress. The gaunt spectre cancer, with its allied malignant maladies—the *opprobrium medicinæ* —the dread of which is an added terror to the frailties of old age, still stalks the land claiming in thousands its victims of unutterable pain and misery. In spite of all our boasted progress, we have not even learned their cause; we do not know the remedy, save in so far that all experience tells us, more and more, that early and complete removal—and the earlier the better—offers a good hope of recovery. To this let all take heed, and they may take the comfort they may so sorely need. On this problem of malignant disease is now being concentrated, in an organised system, some of the best minds of the medical profession throughout the civilised world, and it may well be hoped, nay, it may be expected, that its solution will not long be delayed, to the glory of medicine and the infinite relief of human suffering.

The great white plague, consumption, in spite of our knowledge of its cause and of the conditions which will bring about its prevention and cure, still slays its myriads, far exceeding those of battle and all other pestilences combined, in the most interesting, joyous, and useful epochs of their lives; but it may also be said that if tuberculosis is still propagated it is not because of the failure of science, but because of ignorance, apathy, wilful neglect, or, it may be, genuine poverty on the part of those who still continue habits and practices which are responsible for its prevalence and spread.

We go into hysterics at the thought of the advent amongst us of some rare visitant disease such as bubonic plague or small-pox or leprosy, but we complacently tolerate the continuance in our midst of typhoid fever and diphtheria, which are perfectly preventable, and whose tale of victims is far greater than that of the former. We fail to have our children vaccinated, or to be ourselves revaccinated, by which means we might render the community secure against a severe small-pox epidemic, while our governments, with all their love of compulsory measures, tolerate the neglect, preferring rather, or at least finding it sometimes necessary, to spend thousands in the effort to eradicate what might be prevented.

We see unmistakeable evidence of the physical deterioration of the race due, no doubt, to many co-operative factors into which I may not enter except to say that one contributory cause is the tendency for people to flock to the towns, where the mothers of the future pass the most important period of their lives—that of early adolescence—under the unfavorable or even injurious conditions of crowded and ill-ventilated working rooms. The cry "back to the land" may have a wider and a deeper significance than the mere desire to fill up the blank spaces on the map with profitable industries. It is rare to see even a child with a sound set of teeth, a defect most frequently due to their neglect, from which arises many an ailment, both then and afterwards, and there is much truth in the observation that toothpowder may serve as a better national protection than gunpowder.

It cannot also be doubted that the progress of medical science has been a contributory factor in this degeneracy, by bringing about the saving and the prolongation of the lives of those who are physically or mentally unfitted to be the parents of a healthy race. This brings us within sight of the eternal disharmony between the tendency of the work of our social, political, and ethical systems, of our hospitals, asylums, and the like, which are directed towards a similar end, and the efforts of unaided nature "red in tooth and claw," which, by the elimination of the unfit, strives with unceasing purpose only for the improvement of the race and the survival and perpetuation of

its best. And if time permitted it would be an interesting study to see what an important part has been played by disease in the evolution of the human species, and how civilised man, by his migrations, his diseases, and his meddling, has disturbed existing harmonies, and altered, often for the worse, the organic face of the earth. And that the degeneration of which I have spoken is not merely physical, but extends also to the mental organisation, is shown by the striking and progressive increase both in the number of the actually insane and in the increase of those nervous complaints of which neurasthenia and the like may be taken as the type; this phenomenon being apparently characteristic of all highly civilised communities, and for which the mental strains and stresses of modern life and intemperance are held to be chiefly responsible.

Co-relative with the question of physical deterioration is the ugly fact, for some time suspected and recently confirmed by the report of a Royal Commission in a neighboring State, that the decline in the birth-rate, which we have been accustomed to think of as a feature only of those older countries which have had to face the problems of over-population, has extended to these sparsely peopled States, and that all classes are affected, even those who have not the burden of want to offer as an excuse. Thus one of the conspicuous signs of national decadence is beginning to affect us while we are still a nation in the making. And the causes at work, clear and distinct, and for the most part disreputable, bring with them the uncomfortable idea of a concomitant general loosening of moral fibre which will inevitably radiate its malign influence in other directions. Here, at least, is a field in which that democratic panacea, compulsory enactment, is powerless.

Closely connected, in respect to cause, with the question of physical degeneration is the excessive mortality of infant life, for which, as in the former case, inadequate and improper feeding are more largely responsible than any other single cause, and this, when not due to genuine poverty, is attributable to the lamentable ignorance on the part of those who are mothers, which our vaunted system of education does little to remove. If we are powerless to counteract those evil and other influences which underly the reduction of the birth-rate, it ought, at least, not to be an insuperable problem, and indeed it is our obvious duty to find means for preserving those we have.

The hydra-headed monster, alcoholic excess, still dwells with us, bringing in his train poverty, crime, endless social and domestic misery, and physical and mental suffering, predisposing to disease, and second only to another cause, also begotten of human frailty, in bringing about those degenerative changes which lead to senile decay and premature old age. The solution of the alcohol question would be the solution of many an ill to which weak flesh has made itself the heir.

We still show little wisdom in our dietaries, either in quantity or quality; we still continue to "dig our graves with our teeth," and to lay the foundations of disease, amongst which may be mentioned the prevalent complaint appendicitis. In this respect we in Australia should pay more heed to the evils of our excessively carnivorous diet, to which many of our ills may be traced.

Reviewing these and other physical disabilities which it is the lot of man to suffer and endure, it must in fairness be admitted that the responsibility for the existence and continuance of most of them cannot be laid at the door of medical science. Where this has been able to intervene it has done so with some success, as witness the results of preventive medicine. If we except from the category cancer and some other diseases in which we must regretfully confess the present impotence of our profession, the majority of these physical ills have now, for the most part, largely become social questions,

and their solution when the doctor, speaking in the name of science, has pointed out the cause and cure, lies in the individual and collective efforts of the people.

If, owing to ignorance, apathy, or self-indulgence, they still continue to exist, it is the community, not the doctor, who is responsible. And it would be better if society, instead of blaming science, as it frequently does, that it cannot find ways of curing disease, set its house in order, and, by abstaining from hurtful ways, automatically abolished a vast amount of suffering.

Sometimes the advice and precepts of science are wilfully disregarded, as in the case of vaccination, where the effective remedy is deliberately thrust aside, with the approval, or even the instigation, of politicians, who care more for the votes than for the bodies of their constituents.

Then think of the enormous sum of human improvement, physical, mental, and moral, that would accrue to a great mass of the people if only that one factor in the predisposition to and causation of disease, intemperance, were eliminated from their lives by deliberate voluntary restraint, for I have no belief whatever in the permanent efficacy or benefit of those prohibitive measures which are so often advocated as the only possible panacea. It is stated that two-thirds of the immense national drink bill is incurred by the working classes; how surely, then, would the diversion into healthier channels of money saved in this direction bring an automatic relief to those who are most concerned—a relief from an untold amount of grinding poverty, sickness, and mental and moral distress. And to a degree much greater than is realised, how very much longer and healthier lives we should lead if we refrained from eating so much, so often, and so unwisely. What might be done towards the relief, or even the abolition, of that scourge of home life, consumption, if only people would cease from doing those things which favor its propagation and continuance, and would do those which, they can be told in all honesty, will, with certainty, prevent its spread and bring about its cure. But how shall we blame the general public for their errors of commission and omission when those who cannot claim the excuse of ignorance, in defiance of risks that are not so imaginary as they seem to think, persist in continuing the reprehensible and insanitary practice of the promiscuous transference from mouth to mouth of an uncleansed communion cup. So, too, any betterment in respect to the unsatisfactory and unwholesome conditions which prevail in connection with physical deterioration, decline of birth rate, infant mortality, and the like is only to be expected from a rude awakening of the public conscience, and the adoption of a sane and rational system of training in those hygienic and physiologic matters which, though of such great importance for the sound mind and the sound body, yet are so inadequately treated, or even entirely neglected, in our systems of education.

The suppression of malaria and yellow fever has become, as we have seen, a question of the destruction of the right sorts of mosquitos; plague, largely, of the extermination of rats and of the filth which usually invites their presence; cholera and typhoid, of pure water and uncontaminated food; hydrophobia was speedily eradicated in Great Britain by the much-abused order for muzzling dogs; and in a hundred different ways the fact is obvious that what were once considered medical questions have now, to a large extent, resolved themselves into social problems.

REVIVAL OF QUACKERY AND OCCULTISM: INFLUENCE OF THE MIND ON THE BODY.

Whether it is to be considered as a social failing or only an amiable foible, it is nevertheless true that the boundless credulity of mankind upon subjects connected with medicine has, from earliest times, been a conspicuous

feature in his life, and it is not a little remarkable that an age which has witnessed so great a growth of scientific precision in our medical methods and conceptions should be also characterised by a revival of superstition, a belief in occultism and quackery of all descriptions, that would be worthy of mediæval times. It is particularly remarkable that these beliefs and the habits and practices begotten of them are not confined to the ignorant, but prevail notoriously amongst those who belong to the educated classes. We see the columns of the press filled with the announcements of clairvoyants, professors of second sight, palmists, herbalists of various nationalities, and the more outlandish the more sought after; hypnotists, crystal gazers, faith healers, and a whole tribe of a similar sort, many of the designations being merely cloaks for practices much worse than is indicated by their names. We are told that imperfect noses can be corrected by absolutely painless methods; that double chins, drooping cheeks, and wrinkles can be removed; and that ruptures can be cured without operation. There are infallible remedies for cancer, consumption, obesity, baldness, alcoholism, narcomania, and numerous other diseases. Columns are filled with the marvellous and unfailing virtues of somebody's pills, potions, syrups, and other nostrums—the same article professing to cure a whole group of the most diverse complaints; while flamboyant posters portray the features of the eminent specialists who deal in them. There is an abiding faith, chiefly among the more educated, in various routine "systems" of treatment, as they are called, which pass under the names of their exploiters. And have we not seen in recent times worthy and otherwise blameless citizens pinning their faith to the efficacy of an iron ring as a cure for rheumatism?

One of the most recent developments of this revival of superstition is the pseudo-religious movement which passes under the name of Christian Science, with the Rev. Mary Baker Eddy as the chief exponent and apostle. As an example of the fatuous twaddle which passes for Mrs. Eddy's gospel, let me give you the following extract, which is part of an invocation for the cure of cancer of the stomach, one of the most real, most objective, and most distressing of complaints.

"Lord, help us to believe that all Evil is Utterly Unreal; that it is silly to be sick, absurd to be ailing, wicked to be wailing, atheism and denial of God to say I'm sick. Help us to stoutly affirm with our hand in your Hand, with our Eyes fixed upon Thee, that we have no Dyspepsia, that we never had Dyspepsia, that we will never have Dyspepsia, that there is no such thing, that there never was any such thing, there never will be any such thing.—Amen."

Surely no mumbo-jumbo of Haitian Obi-man could be more ridiculously inept, and yet this is the sort of thing that has gained the concurrence and approval of many educated people. Nevertheless, however repellant we may find the methods and practices of these charlatans, and however silly we may think them, in an age which professes to be one of reason and enlightenment, it is very evident that they must find their trade profitable, for they continually increase in number, in the voluminousness of their announcements, and, we may add, in the impudence of their pretensions.

True of all times and of all sorts and conditions of men and women, and never more true than in this connection, the Hudibrastic distich—

> "Doubtless the pleasure is as great
> Of being cheated as to cheat,"

embodies a profound knowledge of human nature, and condenses a practical wisdom which explains the facile success of many a rogue and charlatan with their confiding dupes.

To our sorrow and discomfiture it must, however, be admitted that we doctors have all of us, at some time or another, met with cases in which so-and-so's patent pills have cured our patient when *precisely* the same ingredients (for the composition of all of these remedies is no secret) have failed, and do we not know, all of us at least who have served our time in a large hospital, the marvellous efficacy of the local brew of *mistura diabolica* concocted with quite other objects in view than that of scientific medication. There can be no doubt of the reason of many of these successes, which is, in fact, the principal foundation on which all kinds of quackery is based. It is that the confident assertions and brazen effrontery of the purveyor of the patent medicine has produced an expectation of, and a profound belief in, its efficacy, which contributory factors often fail to accompany the orthodox prescription. This statement brings us within sight of the great question of the influence of the mind on the body, a factor curiously enough more recognised by the laity than by the medical profesion, not only as manifested in one and the same person, but also as between two different individuals. The potency of this influence everybody freely admits, but few of us actually take it into serious consideration in our practice. We were never taught anything about it in our medical course, nor do we teach it now, and but little attempt has been made to organise and systematise our scattered but yet considerable mass of knowledge of the subject. Yet there is no doubt but that the power to utilise, consciously or unconsciously, this mental factor in medicine is the real secret of the success of many a physician who, except in this respect, has no better medical equipment than his less successful compeers. Though we ridicule the quality, yet really we all envy the possession of a good bedside manner, which is only one mode of expression for a pleasant and reassuring comportment, and it is just faculties belonging to the same category which account for much of the success of the advertising fraternity. "Two mighty powers," says the writer of a recent book, "work for good in every physician worthy of the name: what he knows and what he is; but, alas, as a rule, we only value the former." This well suggests the too often neglected powers that may lie behind the personality of the physician.

Gradually we are learning that the abundant faith which inspires to noble deeds and to the enduring of great suffering is a form of mental energy which may be diverted into the swelling therapeutic stream; but it is a pity that the chief exploitation of this potent force should have so chiefly fallen into the hands of unscrupulous impostors and charlatans ignorant of all things except the illimitable credulity of human nature.

Here, then, is a wide field of investigation for the physician of the future from which a rich harvest may be expected.

CONCLUSION.

It has been my endeavor in this address, in which you may well say that I have wandered from the Dan to Beersheba of medical and social science, to present to those who do not belong to our profession some account of its aims and methods; of its triumphs over physical ills, of its hopes and aspirations for the future, and of the vivifying influence of some principles of science which have so greatly contributed to its advance. I have endeavored to show that man must be considered only as a unit in the world of living things closely inter-related by his diseases with his animal and vegetable surroundings, and that medical knowledge, consequently, stands not apart from but part of the great body of organised science; indeed, more and more is it borne in upon us that not only every science, but also every system of truth, is intimately connected with every other. From these facts it arises that the investigations of an ever-increasing number of diseases have virtually

resolved themselves into problems of botany, zoology, or of physical chemistry. We see how wide has become the scope of preventive medicine, and by a slight stretch of the imagination we may almost foresee a hygienic millenium in which all diseases shall be abolished, leaving only accidents and the inevitable incidents of old age. These latter we are, on the one hand, assured by one of the most eminent of living pathologists might, by appropriate treatment and a better living, be postponed to a period considerably beyond that at which we now accept them with what resignation we can command; while, on the other hand, to those of us who have passed the meridian of days, it comes, as an uncomfortable suggestion, from a man of science of equal eminence in another branch of medicine, that an anæsthetic euthanasia is the most fitting treatment for the sexagenarian. For my part I prefer to pin my faith to the optimistic view rather than to the homicidal treatment, and I fancy there are many similarly conditioned, in point of years, who will have the same preference.

Then it has been indicated, very perfunctorily, as I am too well aware, that many of the medical questions relative to disease have passed out of the domain of medicine and entered that of social and political science, in which, again, they are dependent upon individual, collective, and organised efforts scientifically directed and persistently carried out. Had I not outraged your patient indulgence by the length of my remarks it would have been interesting and possibly profitable to examine the extent to which these efforts have been successful, or, alas, too often inefficient or even unattempted.

Science has, I think, shown by its achievements and by its energies that it may be trusted to do its share in the world's work; but how shall we, in an age distinguished by its shams, shoddy, and superficialities, inculcate and foster that spirit of lofty patriotism, self-reliance, self-restraint, supreme devotion to duty, and unceasing striving for excellence in all things undertaken which are the real foundations on which a nation's greatness rests?

It is well that we should realise, before it is too late, that a nation's pre-eminence depends not alone on the extent to which its collective intelligence is organised and co-ordinated along the many diverse paths that lead to success, but also on the high sense of duty and self-sacrifice of its citizens. And the doctor may perhaps be forgiven if, in his capacity of a social unit of the community, he ventures to say that it is impossible to view without anxiety the prevailing tendency of the day towards the pursuit of pleasure and the decline of high enthusiasms and beliefs in ideals. In this we may see the sacrifice of many things that have made our country great, and the presence of many that have contributed to the downfall of great nations in the past. It is said of us with much truth that we are apt to be in earnest about trifles and to trifle about earnest things, and this in days when, with unexpected suddenness, our national patrimony—nay our very existence as a great nation—may become the stake for which we shall contend with others less self-complacent than ourselves.

Many lessons have been taught to a wondering and startled world by the great Manchurian drama, but they may all be summed up in the one singularly happy and comprehensive appreciation, which we who deal with the lives of men may well take to heart—"Nothing but the best will do." This after all is but the most modern variant of that venerable exhortation of the Preacher applicable to all men and for all time—

"Whatsoever thy hand findeth to do, do it with thy might."

Dr. John Thomson (Brisbane): The task imposed upon me can best be discharged by brevity. We came to-night to listen to the inaugural address by our distinguished President. We came expecting an intellectual

treat, and I can assure you, speaking for my own part, and speaking for all of you, that our expectations have been realised. (Loud and prolonged cheering.) In this feast he has put before us many dishes of the very highest quality, to provide food for our mental digestion and assimilation. The address has been worthy of the man—the man the medical profession throughout Australasia unanimously selected to preside over us, the man who can put after his name the mystic letters F.R.S., which but few other Australian men can do—a distinction which England bestows upon only her most scientific sons. (Cheers.) The address was worthy of the profession to which it was delivered, a profession which the brilliant preacher yesterday declared to be one of the learned professions, and associated it with law and divinity. He clearly hinted that law and divinity had been left behind, and that medicine had made marvellous strides. If, therefore, it was learned in the past, it is still more learned now. The address was equally worthy of the men in this State of yours, including Dr. Poulton, who, in 1887, to commemorate the Jubilee of Queen Victoria, inaugurated this Congress. It went through all the colonies, and came to Brisbane ; and it was at Brisbane that His Majesty's representative here to-night was initiated into the mysteries of the Congress. It has come back here, and it gives us great pleasure to be here amongst you. May I ask your Excellency to put a vote of thanks to our President for his address this evening ?

His Excellency : As Dr. Thomson has said, it hardly needs words from anyone else to ask you to give a unanimous vote of thanks to our distinguished President. I think I may, in the name of you all, not only those who are present, but of all who will have the pleasure of reading it and hearing of it, present to him a unanimous and hearty vote of thanks.

The vote of thanks was carried with the audience standing.

The President bowed his acknowledgments.

The Hon. G. H. Butler (Hobart) : It is my pleasing duty to-night to propose a vote of thanks to His Excellency the Governor, who has shown a keen interest in our deliberations and discussions. The fact of one in his high position and attainments showing such an interest in our Congress must be an incentive to all of us. There is no doubt that one advantage of this Congress is that we learn so much from each other when we meet. I also, on behalf of the visitors, thank His Excellency for the cordial welcome he has extended to us on our visit to Adelaide.

The motion was carried with acclamation.

The proceedings closed with the National Anthem on the Conservatorium organ by Professor Ennis, Mus. Doc., Director of the Elder Conservatorium.

FINAL GENERAL MEETING.

The final general meeting of Congress was held at the Prince of Wales Theatre, University Buildings, on the morning of Saturday, September 9th The President (Professor Stirling) occupied the chair.

The President : In the first place I have to read you a resolution from the Synod of the Church of England. It is as follows :—

"Church Office, Adelaide, September 6th, 1905.

"The President of the Medical Congress, Adelaide.

"Dear Sir—I beg to forward hereunder a copy of a resolution passed unanimously by the Synod of the Church of England at its annual meeting yesterday.

"I am, dear Sir, yours faithfully,

"Charles Marryat, President of Synod.

"THE RESOLUTION.

"This Synod, recognising the dignity and greatness of the medical profession, its unique opportunities, its immense responsibilities, together with its countless deeds of unknown and unacknowledged kindness to the sick and needy, tenders its respectful and hearty greeting to the Australasian Medical Congress, assembled for the second time in the city of Adelaide. The Synod trusts that, by the blessing of God, the deliberations of the Congress will minister to the progress of advancing science and consequent amelioration of some of the world's distresses. It also hopes that the visit to Adelaide of members of the Congress from other States will be one of pleasure as well as profit to themselves."

We shall take means a little later on in this meeting to send a suitable reply to this message. The next business is dealing with the resolutions from the various sections of Congress. Before I deal with them specifically I may be allowed to make the following remarks. It is no doubt a difficulty that we experience at these meetings that we have no standing orders under which to work, and no rules of debate. That is a difficulty which has occurred to us, and a little later will follow a resolution which will seek to remove it. Seeing that we are obliged to conduct this meeting without guidance or rules of debate, perhaps you will allow me to lay down the following rules, which I hope you will not consider too draconian. We are all anxious to give due consideration to the business of the resolutions, and it will be understood that as the various resolutions have been considered by the sections in which they have arisen, we may reasonably anticipate that full discussion has taken place on them. I therefore say that no speaker shall speak for more than five minutes and that at any time, on its being proposed and seconded—"That the motion be now put," the motion shall be put without discussion. (Loud cheers.) That will enable us to see whether we want to decide one way or another. May I take it by those cheers that you agree ? (Cries of "Aye.") The first resolutions are from the Section of Medicine.

SECTION OF MEDICINE.

DR. JARVIE HOOD (Sydney) : I have much pleasure in moving—"That this section is of opinion, judging from the complex and sometimes dangerous symptoms produced by X Rays, Radium, and Finsen light, that the use of these agents should be in the hands only of members of the medical profession. That this resolution should be forwarded to the general meeting of Congress for its consideration." We discussed it thoroughly in the Section of Medicine, and there is no need to discuss it further now.

DR. MURRAY (Melbourne) : I second that.

DR. LOVE (Brisbane) : I move an amendment to add—"Or under their direct supervision." Since that resolution was adopted by the Section of Medicine a conversation has taken place, and we believe that the amendment will overcome a difficulty that presents itself in the original resolution.

DR. MURRAY (Melbourne) : I hardly think it is an amendment. Dr. Hood and I are agreeable to accept that the words be inserted.

The addition was agreed to, and the motion as amended carried.

SECTION OF PUBLIC HEALTH.

THE PRESIDENT : The next is a series of resolutions from the Public Health Section, but I notice that they do not come to us with a recommendation to be forwarded to the General Congress.

RESOLUTIONS.

1. "That in view of the profound influence of school life and school training upon the physical future of the race, the hygienic construction and management of schools, the training of teachers in school hygiene, and the systematic practical instruction of school children in elementary personal, domestic, and civic hygiene are subjects which demand the earnest attention of the Governments of the various States of the Commonwealth."
2. "That for each State Education Department there should be available a whole-time medical officer, possessing special knowledge of applied hygiene in relation to schools, to whom should be referred all questions relating to the hygienic construction and management of State schools, to the protection of teachers and pupils from preventable disease, and to the methods and scope of hygienic instruction and training."
3. "That applied school hygiene should form a compulsory subject in the training of State teachers, and that a system of medical inspection of schools should be introduced at an early date, particularly in the larger centres of population."
4. "That the compulsory notification of phthisis is advisable in the interests of public health."
5. "That in view of the large number of deaths from phthisis in Australasia, this Congress urges upon the Governments of the various States the necessity of establishing and maintaining sanatoria for the treatment of cases among the poor."
6. "That it is necessary to provide a fund for assisting families among the poorer classes where the breadwinner is suffering from phthisis, and has entered a sanatorium for treatment, and this Congress urges upon the Governments of the various States the necessity of taking steps for providing such a fund."
7. "This Congress protests against any action which has the effect of interfering with a bi-daily delivery of milk in Australian cities, and considers any such action as seriously prejudicial to the public health."
8. "That it be a recommendation to the next Congress that special consideration be given to the attaining of uniformity in the compilation of vital statistics in the Australian capitals."

Dr. Springthorpe (Melbourne): I think the error mentioned by the President is one made by the printer. I know my resolutions were for recommendation to Congress. I move to add—"That these resolutions be forwarded to the general meeting of Congress for its consideration."

Dr. Elkington (Hobart): I second it.

Carried.

Dr. Armstrong (Sydney): I propose the adoption of the resolutions by Congress.

Dr. Worrall (Sydney): I second that.

Carried.

Section of State Medicine and Medical Ethics.

Dr. A. A. Hamilton (Adelaide): I beg to move to add to these resolutions—"That these resolutions be sent on for adoption by Congress."

Dr. Gibson (Brisbane): I second it.

Carried.

Dr. Rennie (Sydney) moved—" That a committee be appointed, consisting of representatives from each State, with power to add to their number, to consider the best means to be adopted to amalgamate the various medical organisations throughout Australasia and New Zealand; two representatives to be elected by the local branches of the British Medical Association in each State, by the Victorian Medical Society, and in Tasmania by the Medical Section of the Royal Society, and by each Medical Defence Association in Australasia. That the Secretary of the Section of State Medicine and Medical Ethics should communicate the above resolution to the various bodies, with a request to send him their nominations."

Dr. Marten (Adelaide): I second it.

Dr. Rennie (Sydney): I should like to point out that there was an addition to the resolution—that the committee report to the next meeting of Congress.

The President: Perhaps we can have that as a separate resolution.

The motion was carried.

Dr. Rennie (Sydney): I move—" That the committee appointed by the foregoing resolution bring up a report to the next meeting of Congress."

Dr. Sprott (Hobart): I second that.

Carried.

Dr. Springthorpe (Melbourne) moved—

1. " That in the opinion of this Congress the future provision for the care and treatment of the insane in Australasia should be based upon a recognition of the threefold requirements of (*a*) up-to-date mental hospitals, (*b*) farm and industrial colonies, and (*c*) benevolent asylums."
2. " That in the opinion of this Congress there is imperative need for the establishment in the different States of epileptic colonies, in which epileptic children should receive education, epileptic adults be suitably employed, and epileptics generally find a home—as well as the most successful treatment."

There is no machinery by which these resolutions shall reach the persons to whom they ought to go. There should be a general instruction to the General Secretary that they should be forwarded to the proper authorities. They should go to all the State Governments. I move to add—" That the General Secretary take the necessary steps to forward these resolutions to the proper authorities."

Dr. Howard (Melbourne): I second the resolutions with the addition.

Carried.

Dr. Rennie (Sydney): I beg to move—" That it is unethical for medical practitioners to grant interviews to representatives of the lay press on medical matters, in connection with which their names are allowed to appear. That a copy of this resolution be forwarded to the lay press."

Dr. Springthorpe (Melbourne): I second it.

Carried.

Dr. Springphorte (Melbourne): There is one by Dr. Syme, which is accidentally omitted.

Dr. Syme (Melbourne): There was no resolution afterwards passed that it should be presented to Congress.

Naval and Military Medical Officers.

Surgeon-General Williams: I move the adoption of the following resolution:—"That in the opinion of this meeting it is desirable to hold a Naval and Military Section at future Australasian Medical Congresses. That this resolution be forwarded to the general meeting of Congress for its consideration."

Dr. Giblin (Hobart): I second that.

Carried.

The Eye Section.

Dr. Lockhart Gibson (Brisbane): There was a resolution passed unanimously by the Eye Section, but there was no decision that it should be forwarded to the Congress to pass it. It was proposed by Dr. Kent Hughes and seconded by Dr. J. W. Barrett. It was as follows:—"That permanent blindness from lead-poisoning amongst young children in Queensland has been recognised; that it is highly probable that paint used on verandah railings, &c., especially when exposed to weather influences, is the source of the poison; and that this matter should be brought under the notice of the proper authorities." I beg to move that that resolution be reported to Congress.

Dr. Kent Hughes (Melbourne): I second that it be reported.

Carried.

Standing Orders.

Dr. W. T. Hayward (Adelaide): I have to move—"That it be a recommendation to the Executive Committee of the next Congress to draw up Standing Orders for the conduct of debates and other business of Congressional meetings, and that the same be reported to the first general meeting of the next Congress." I do not think I need say anything about it.

The President: We are only asking that something may be done to remove the difficulty I have mentioned.

Dr. Astles (Perth): I second it.

Carried.

Next Meeting of Congress.

Dr. Jamieson (Melbourne): I suppose, Mr. President, that it is because I am a member of that diminishing band whom you mentioned in your address as Nestors of the Congress that I have had this resolution put in my hands. I suppose we have to confess that it is greatly on account of the parsimony of the Commonwealth Parliament in refusing Western Australia its rights to a direct railway that we are unable to visit that State at present. I think it almost follows as a matter of course, then, that, since Melbourne succeeded Adelaide on the last occasion, it ought to succeed it on this. As those of us who were present at that first meeting of Congress carried away very pleasant recollections of that meeting, we tried when the Congress came to us in Melbourne to make the meeting a pleasant and successful one, and we did so. (Cheers.) We had nothing but pleasant recollections of that first meeting, and we shall go away again with grateful remembrances of the unparalleled hospitality that has been extended us on this present occasion. (Loud and prolonged cheering.) Melbourne is not now what it was in the days of the boom, but still if we cannot altogether imitate the almost regal hospitality we have received on this occasion, in our humble way we will try to do what we can I have to propose therefore—"That the next meeting of Congress be held in Melbourne—provisionally—in 1908." It will be left to the local authorities to fix the date. I have pleasure in inviting the Congress to Melbourne.

Dr. Syme (Melbourne) : I have very much pleasure in seconding the resolution. I can only repeat what Dr. Jamieson has said—that Melbourne feels so overwhelmed by the hospitality and kindness received in Adelaide on this occasion that we feel it is a very difficult matter to follow in the same regal way. We can only do our best, and we extend to you a very cordial invitation to come to Melbourne next time. (Cheers.)

Carried.

The New President.

Dr. Rennie (Sydney) : I have much pleasure in moving—"That Professor Allen, Professor of Pathology and Anatomy at the University of Melbourne, be the new President of Congress."

Dr. Lockhart Gibson (Brisbane) : I second it with much pleasure.

Carried.

Votes of Thanks.

The Hon. G. H. Butler (Hobart) : I have much pleasure in moving—" That a hearty vote of thanks be accorded to His Excellency the Governor for his patronage, the very keen interest he has taken in our proceedings, and also for his most royal hospitality since we have been here."

The President : No seconder is required for these resolutions.

Carried.

Dr. Wilton Love (Brisbane) : I move—" That a vote of thanks be passed to Sir Samuel Way, Lieutenant-Governor of the State, for his patronage in the first place, for his liberal support and the interest he has taken in the Congress, and lastly for his hospitality." Those who were at the conversazione last evenin will carry away pleasant recollections which will last a very long time.

Carried.

Dr. J. C. Verco (Adelaide) : I propose—" That a vote of thanks be passed to the Government of the State, the Premier, and the Acting Deputy Postmaster-General." The Government have promised to print our transactions—(cheers)—they have afforded very considerable facilities for visiting the State institutions, and also granted concessions in railway travelling. (Cheers.) We associate the name of the Premier, who gave us a very hearty welcome at the inaugural meeting, and made kindly reference to the medical profession, and expressed his very good wishes on behalf of the State. We thank him for that, and especially for his hope that we might discover some means whereby patients should be made to pay. (Laughter.) We also include the Acting Deputy Postmaster-General, for the conveniences he has afforded us in postal matters.

Carried.

Dr. Ross (Sydney : I move—" That a hearty vote of thanks be given to the Mayor of Adelaide for his warm welcome, and for the interest he has shown in our proceedings."

Carried.

Dr. Astles (Perth) : I have very much pleasure in proposing—" That a hearty vote of thanks be given to the Chancellor and Council of the University for the free and full use of their buildings and scientific apparatus, and also to the members of the staff for their active assistance, particularly the Registrar, Mr. Hodge, and Mr. Fuller and Mr. Addison." I am sure that without these buildings, and without the active courtesy and assistance of the staff, this Congress would not have been the success it was.

Carried.

Dr. Jeffrey Wood (Melbourne): It is my pleasing duty to propose—"That a very hearty vote of thanks should be accorded to the Synod of the Church of England for their very friendly message." I am sure also that I am only echoing the feelings of members of this Congress in also thanking the Dean and Chapter of the Cathedral, the choirmaster, and organist for the splendid service which the members of Congress had the privilege of attending on Sunday last, with special mention of Canon Hopcraft, for his sermon. (Cheers.)

Carried.

Dr. Thomson (Brisbane): This fair city of yours has been invaded by hordes from all the States, but you have captivated and captured us. Someone has said that the work of the Congress was getting beyond bounds, but don't you think the entertainments are getting beyond bounds? I look upon this place as a paradise for practitioners, and it seems to me that all your medical men are millionaires. They live in mansions instead of homes. They don't drive traps, but they drive luxurious equipages and motors—so, ostensibly, they are able to do so. (Laughter.) I move—"That a hearty vote of thanks be accorded to those ladies and gentlemen who have entertained us, with especial mention of the Chief Mechanical Engineer, Mr. T. Roberts, the Superintendent of Public Bildings, Mr. C. E. Owen Smyth, and the Hydraulic Engineer, Mr. C. A. Bayer." Every lady in the place has done her level best, and I wish to include in the motion all the fair sex who have been connected with the Congress.

Carried.

Dr. Jarvie Hood (Sydney): I have to move—"That a hearty vote of thanks be accorded to the Press for their full reports of meetings, and for their general information and support."

Carried.

Vote of Thanks to the President.

Dr. Batchelor (Dunedin): I feel that the motion entrusted to me is a very important one. It is—"That we accord a hearty vote of thanks to our President for his welcome."

(Members rose, and gave three rousing cheers.)

Any attempt to eulogise the work done by our President would be presumption, and I shall not attempt it. But we must congratulate ourselves on the way the work has been got through; and so long as we have leaders of the profession such as Dr. Stirling we may look hopefully for the maintenance of the status of the profession in Australasia. (Loud cheers.)

Carried.

The President: Gentlemen—I should be more than human if I did not greatly appreciate the compliment that you have been good enough to pay me this morning. It is quite true that as an individual I have done my best to make this Congress a success. (Dr. Thomson—"And you have succeeded.") Judging from your expressions of approval, we have not been altogether unsuccessful. (Cheers.) But let me say this—that although the executive head of a meeting of this kind may count for something, yet full well do I realise that any efforts would have been perfectly unavailing towards success but for certain contributory factors. I have been fortunate enough to have behind me the united medical profession of South Australia. (Cheers.) I have been extremely fortunate in having around me the most painstaking, devoted, and sagacious officials that possibly could help any President. You know their names—Dr. Hayward—(Cheers)—Dr. Poulton—(Cheers)—Dr. Gunson (Loud cheers)—and Dr. Symons, Chairman of the

Reception Committee. (Cheers.) When I had those men alongside me with their experience, with their personal friendliness, and their indomitable energy—which is indeed necessary on these occasions—I had the assistance of most valuable men. And I say, gentlemen, when you are good enough to thank me for what I may possibly have done, I wish that it should be recognised how much we owe to these gentlemen for their kindly assistance throughout the whole of this Congress. That, I think, is all the acknowledgment I am able to make; because after all the best acknowledgment has been your kindly tributes towards our efforts, and it is very gratifying to find that you, as visitors, are prepared to make the best of any virtues we may possess, and turn a very blind eye to the many failings that you must have discovered in ourselves. ("No.") We have had a very unanimous Congress, and I think it has been a pleasant one. (Loud cheers.) I thank you all for the unfailing support you have given us. That now concludes the Adelaide Congress. (Loud cheers.)

SECTION OF MEDICINE.

Tuesday, September 5th.

At 10 a.m. Dr. Colquhoun (Dunedin), President of the Section, read his address.

At 11.30 a.m. Dr. Rennie (Sydney) read his paper on "Diet in Relation to Kidney Disease."

Discussed by Drs. Verco and Jamieson.

Discussion adjourned until Wednesday, to permit members to hear Professor Welsh's paper on Cancer.

Wednesday, September 6th.

At 11.10 a.m. the President took the chair. (The Section did not meet earlier owing to members wishing to hear Mr. Bird's address on Surgery.)

Dr. Abramowski (Mildura) resumed the discussion on Dr. Rennie's paper.

Dr. Springthorpe (Melbourne) read his paper on "The Use and Abuse of Mental Therapeutics."

Discussed by Dr. Rennie and the President. Dr. Springthorpe replied.

Dr. Abramowski (Mildura) read his paper on "The No-food Treatment of Typhoid Fever."

An animated discussion followed, in which the following members took part, viz.:—Drs. Hornabrook, Hayward, Jarvie Hood, Verco, Springthorpe, and the President. Dr. Abramowski replied.

Dr. Rennie read his paper on "Epilepsy: Is it Incurable?"

Dr. Jamieson (Melbourne) read his paper on "Notes of a Case illustrating the Symptoms of Tumor of the Occipital Lobes of the Brain."

Discussion on these two papers was adjourned until Thursday.

Thursday, September 7th.

The President took the chair at 10 a.m.

An interesting discussion took place on Dr. Rennie's paper. Drs. Jamieson, J. Hood, Verco, and Sinclair taking part.

Dr. Sinclair moved and Dr. Jarvie Hood seconded—"That this Section supports the resolution submitted by Dr. Springthorpe, in the Section of

State Medicine, viz., ' That, in the opinion of this Congress, there is imperative need for the establishment in the different States of Epileptic Colonies, in which epileptic children should receive education, epileptic adults be suitably employed, and epileptics generally find a home as well as the most successful treatment.' "

Carried *nem. contrad.*

The resolution was forwarded to the Secretary of the Section of State Medicine.

Dr. McIntyre Sinclair (N.S.W.) read his paper " The Prognosis of Pulmonary Tuberculosis."

Dr. Stoney (Echuca) read his paper " Five Years' Records, Victorian Sanatoria for Consumptives."

Dr. Camac Wilkinson (Sydney) read his paper.

Dr. Gault (Adelaide) read his paper " Sanatorium Treatment of Pnthisis."

An interesting discussion was entered into by Drs. J. Hood, Gill, Lawrence, Abramowski, and the President. The readers of the papers replied.

Friday, September 8th.

At 9 a.m.—Clinical exhibits. Drs. Verco and Swift exhibited several cases of interest.

At 10 a.m. Dr. Herman Lawrence (Melbourne) read his paper " Radium and Electro Therapeutics in the Treatment of Skin Diseases."

Dr. Lawrence at the same time exhibited an " X-ray bath."

Dr. L. H. Harris (Sydney) read his paper on " The Present Position of the Röntgen Rays in Medicine and Surgery."

Dr. F. Clendennin (Melbourne) read his paper.

Discussion.—An animated discussion was taken part in by Drs. Springthorpe, Wilton Love, Closs, J. Hood, Reissmann, Benham, and the President.

Dr. Jarvie Hood moved and Dr. Murray seconded—" That this Section is of opinion, judging from the complex, and sometimes dangerous, symptoms produced by X-rays, radium, and Finsen light, that the use of these agents should be in the hands only of members of the medical profession, and that this resolution be forwarded to the General Meeting of Congress for its consideration."

Carried *nem. contrad.*

Resolution forwarded to the General Secretary.

Dr. Jarvie Hood read his paper on " The Transposition of the Organs verified by *Post-mortem* Examination."

Drs. Stoney, Astles, Reissmann, and the President remarked that they had each seen one case, and Dr. Verco said he remembered two cases.

Dr. Astles (Perth) read his paper " Is there such a Disease as Croup ? "

Drs. Stoney, Gill, and Benham discussed it. Dr. Astles replied.

Dr. J. McDonald Gill (Sydney) read his paper on " Leukœmia and Allied Conditions in Children."

Discussed by the President.

Dr. Gill read his paper on " Two Cases of Lead-poisoning in Children."

Drs. Stoney and Morris discussed it. Dr. Gill replied.

Dr. Benham (S.A.) read his paper on " A Fatal Case of Acute Pancreatitis."

Dr. Benham laid his paper on " The Medical Treatment of Appendicitis " on the table, as there was no time to read it.

Dr. Reissmann did the same with his paper on " The Principles of Chemistry and the Study of Medicine."

STATE MEDICINE, ETC.

WEDNESDAY, SEPTEMBER 6TH, 1905.

Presidential Address by DR. SYME. PROFESSOR STIRLING in the chair.

THURSDAY, SEPTEMBER 7TH, 1905.

At 9 a.m. DRS. VERCO and SWIFT had several clinical cases on view, but owing to the counter attraction of a visit to the crematorium, very few members attended.

DRS. VERCO and SWIFT exhibited a man suffering from idiopathic muscular paralysis.

DR. SWIFT showed a series of cases of icthyosis that he had treated with extract of thyroid gland and arsenic. He also brought forward three cases of psoriasis that had made very little progress under several different modes of treatment, but improved very rapidly when given thyroid extract and arsenic.

DR. SWIFT also exhibited two girls suffering from chronic progressive enlargement of joints, with enlarged glands (still), or so-called rheumatoid arthritis of children.

SEPTEMBER 7TH, 1905.

DR. SYME in the chair.

10 a.m. Paper on " Hospital Abuse " (DR. H. W. BRYANT).

10 a.m. Paper *re* " Taking of Payment from Hospital Patients " (DR. W. MOORE).

In the absence of the writers, the above papers were read by the secretary of the Section.

Moved by DR. HOWARD that DR. Rennie's paper be read before above were discussed.

" The Role of Provident Medical Associations," by DR. Geo. RENNIE.

Proposed by DR. RENNIE (Sydney), seconded by DR. BEESTON (Newcastle) " That a committee be appointed, consisting of representatives from each State, with power to add to their number, to consider the best means to be adopted to amalgamate the various medical organisations throughout Australasia and New Zealand, two representatives to be elected by the local branches of the British Medical Association in each State, by the Victorian Medical Society, and in Tasmania by the Medical Section of the Royal Society, and by each Medical Defence Association in Australasia ; that the secretary of the Section of State Medicine and Medical Ethics should communicate the above resolution to the various bodies, with a request to send him the names of their representatives ; that this committee bring up their report to next Congress."

Supported by DR. HOWARD ; carried unanimously.

Moved by DR. RENNIE—" That the above resolutions be sent on for adoption by Congress on Saturday morning."

Proposed by DR. SYME (as suggested by Medical Defence Association of Victoria), seconded by DR. SPROTT—" That it is unethical that autobiographical or biographical notices of medical practitioners should be published in the lay press during their lifetime."

Supported by DR. BEESTON ; carried unanimously.

Minutes taken as read, September 8th, 1905.

Confirmed—G. A. SYME.

SEPTEMBER 8TH, 1905.

Moved by DR. RENNIE, seconded by DR. SPRINGTHORPE—"That it is unethical for medical practitioners to grant interviews to representatives of the lay press on medical matters in connectian with which their names are allowed to appear."

That a copy of this resolution be forwarded to the lay press.

That this resolution be sent on for adoption by Congress.

Carried unanimously.

Moved by DR. SPRINGTHORPE, seconded by DR. HOWARD—"That, in the opinion of this Congress, the future provision for the care and treatment of the insane in Australasia should be based upon a recognition of the three-fold requirements of (*a*) up-to-date mental hospitals; (*b*) farm and industrial colonies; and (*c*) benevolent asylums."

2. Moved by DR. SPRINGTHORPE seconded by DR. RENNIE—"That, in the opinion of this Congress, there is imperative need for the establishment in different States of epileptic colonies, in which epileptic children should receive education, epileptic adults be suitably employed, and epilepties generally find a home as well as the most successful treatment."

As regards the foregoing resolutions, there was a consensus of expert opinion as to the methods that should be adopted in the future in the way of housing and treating the insane and of dealing with epileptics; and there was, in addition, an especial need of having this opinion confirmed by Congress, so as to bring it to bear upon different State Governments. In the absence of any such authoritative resolution, other than scientific reasons might dominate future action, leading to great waste of public money, and the perpetuation of unsuitable modes of treatment. The resolution suggested would do much to strengthen the hands of all those who were working for reform in our lunacy administration. He had much pleasure, therefore, in moving the adoption of both resolutions.

That above be forwarded to Congress.

Papers by DR. ALLWORK—"The Ethieal Relations between Town and Country Praetitioners," and DR. T. C. MOORE (Napier) —"The Friendly Soeieties in Napier, New Zealand."

Both taken as read.

ARMY MEDICAL CORPS.

MEETING OF NAVAL AND MILITARY MEDICAL OFFICERS.

A meeting of Naval and Military Medical Officers was held in the Prince of Wales Theatre, University Buildings, on Friday, September 8th. Major Ramsay Smith, P.M.O., South Australia, presided. Lieutenant Hornabrook, South Australia, acted as Seeretary.

SURGEON-GENERAL W. D. C. WILLIAMS, C.B., read a paper on "The Organisation of the Australian Army Medical Corps on a Peace Footing with its Expansion to War Requirements," which is printed in full elsewhere.

LIEUTENANT-COLONEL EAMES, C.B. (Newcastle): I move a vote of thanks to Surgeon-General Williams for his exeellent paper, and for putting the matter so plainly before us. I take it also that I may move—

> "That it is desirable that a Naval and Military Seetion be held at future Medical Congresses."

MAJOR BEESTON (Newcastle): Do you wish a discussion in general, as to the best method of obtaining offieers?

Surgeon-General Williams: Any point you wish; but I think we should settle first the matter of the section at the Congress.

Major Beeston (Newcastle): Then I second the motion.

The motion was carried.

Major Ramsay Smith (Adelaide): It is almost a pity to enter into any discussion on this paper. Any discussion which we may initiate or carry on will only tend to make the subject appear lopsided after we have finished with it. It is better, on the whole, to make up our minds to carry out the scheme as indicated. If we do that we shall have a first-class reserve of officers. That is very essential, and it is a thing which we should all welcome. Many of us know men who have done yeoman service in the war as consulting surgeons and physicians who do not meet us as officers. We all regret it, and I think the scheme Surgeon-General Williams has outlined to bring them in is one that deserves our support. The other point that is new to us is the extension of our service to supply a Red Cross institution. That also is an admirable suggestion, and ought to be carried out. Here in Adelaide I am glad to say that the ambulance associations are very closely connected with the Army Medical Corps. It has come about in a natural manner from single members of both associations, and I do not think it would be difficult to follow in the same course in the other States. You will forgive me for not initiating a discussion. The time will be better occupied in carrying resolutions which I hope members of the Congress will bring forward.

Colonel Thomson (Brisbane): My great idea is to get medical men to join the service. I do not know that it is a very alluring service. A man has to sacrifice a great deal of time to it, and to make it a success he must take it up as a hobby. Unless he does so, he is better out of it. Once he adopts it as a hobby he will find it very pleasant, very instructive, and, apart from the military aspect, he will find certain social surroundings that give it a certain charm. The reserves in some of the more populous States will be obtained more quickly than in others, and will find more ready acceptance. We have a very huge territory in Queensland, and the units in the various services and brigades are a long way apart. In connection with the 15th Light Horse there are 700 miles between the various squadrons, and it is almost impossible to get them together, and equally impossible to get medical officers to join. It should be less difficult to get the reserves, and there would be no difficulty to get the men enrolled. I do not think they would get much work out of them in peace or in camp; and it is in camp that the best kind of instruction is to be obtained. Many men are better surgeons and physicians, and are better qualified to look after patients, than those who have stars on their breasts and wear shoulder straps. But those men know nothing earthly about camp discipline, or about giving an order and seeing that order carried out. That is altogether beyond them. It is a difficult thing sometimes to get orders carried out, because some men with higher positions in the service resent carrying out orders. This association in camp, and learning the duties of camp, are of vital importance. I really think our nursing association should be extended. In Queensland we have fourteen nurses, and I see no reason why other nurses should not be on the reserve list. We can use them when opportunity offers. I do not see why some of those associations should not amalgamate, or why we should not recognise some of the civil ambulance corps. We have a very extensive transport corps in Brisbane, and they have something like eight or ten small wagons. The place is fitted with electric batteries, and a bell rings, and out they come, and go down the street like a fire brigade. That is a great advertisement. They are under discipline, and it would be a grand thing if we could get the whole of the civil medical men enrolled as a reserve, and the whole of the

nurses commandeered as well for the reserves. Every man ought clearly to understand the organisation in his own State—not only the organisation of the A.M.C., but the organisation of every unit in the service. You are responsible for the medical examination of the whole of the men, so that you are really brought in touch with every branch. If you do not really understand the whole organisation of a brigade, then necessarily you are at a disadvantage. It does not take much time to learn it. Having learned it, you can more readily understand the work of the G.O.C., who has made a homogenous whole out of the old disintegrated parts in the various States, each of whom was working under its own Act. He has done it in a marvellous way, and secured great harmony. I have paid great attention to the scheme of organisation that was drafted by General Hutton, and the last words he said to me were—" If we could get Williams and Bridges home in London, they would reorganise the whole British Army."

Surgeon-General Williams: I am extremely gratified at the unanimity of opinion that existed over the motion that we should at future Medical Congresses have a Naval and Military Section. That will give us in the next three years a chance to bring up matters of organisation, material, transport, camp sanitation, and other things; and we ought to occupy three or four days of the Congress if this movement is accepted by the Congress.

Major Ramsay Smith (Adelaide): I think we should give a vote of thanks to Surgeon-General Williams for his interesting paper, and bringing this matter before us.

Dr. Giblin (Hobart): I second that.

The motion was carried unanimously.

Surgeon-General Williams bowed his acknowledgments.

SECTION OF MEDICINE.

ON THE STANDARD OF ETHICS AND WORK IN MEDICINE.

By Daniel Colquhoun, M.D., M.R.C.P.,
President of Section.

Mr. President and Gentlemen—My first duty is to express my strong sense of the honor conferred on me in electing me President of the Section of Medicine in this Congress. While I recognise that the appointment is mainly a courteons recognition of your sister State of New Zealand and its Medical School, I none the less gratefully thank you, my colleagues of South Australia, both personally and on behalf of those I represent.

I confess that I have found great difficulty in deciding on what subjects to speak, and recognise how natural is the expression with which a well-known song begins—" That first of all we'd have you know how hard it is to write." What I say must be technical, because I am addressing an audience of experts : It must not be too technical, because I am addressing also an audience whose interest in our art is general and not particular. Nor can I forget that there is no part of the science of medicine that has not been fully dealt with from hundreds of platforms, and that I am following a company of distinguished men who have spoken on similar occasions from this chair, and who have initiated the discussions in the section of medicine with wisdom and eloquence which are hard to imitate. If therefore, in the selection of the theme and in the handling of it, I fall below the dignity of the occasion, I bespeak in advance the kindly consideration of the members of my own profession, and those other ladies and gentlemen who honor us with their presence.

In choosing to speak of medical education in these States I have been guided by the fact that this is a subject on which I have some special knowledge, and that it is a matter of importance to the whole community—lay as well as medical. *We* are interested in the quality and character of the young men who are daily joining our ranks as colleagues, and who are to be our successors, and those who are not of the profession are concerned in all that relates to men and women with whom at one time or another they must be brought into contact, and to whose skill and conduct they must probably at some time trust their lives and happiness.

To the public the technical details of our studies are of little importance : the wise man leaves law to the lawyers, medicine to the doctors, and navigation to sailors. But in every art and science the public is the supreme judge of its usefulness, and there are questions concerning our own art that have to be propounded and answered anew from time to time, almost by each generation. The most important aspect of the relation between medical practitioners and the public will always be that which is concerned with ethics. Does the profession maintain a high standard of conduct among its members ? A second question, or series of questions, concerns the working side or efficiency of the profession. Is education thorough, here and abroad ? Are the teachers and pupils alive and quick to grasp the needs of the hour, and alert to what is required of them ? And, curiously enough, do doctors really know anything about disease and the curing of it ? We have no

objection to these questions—not even to the last: they are as necessary for those who practise medicine as for their patients. Many people behave at any rate as if the answer to the last question must be in the negative. They take drugs in enormous quantities on the strength of advertisements, they entrust the care of their health to professed healers who have no qualifications except unbounded self-assertion, and they adopt theories of the most grotesque and absurd character. But large as this class is, it is not to it we appeal. It is to the sober and reasonable part of humanity; and, imperfect as the result may be, I shall try to define our position, both as to Ethics and Education.

There has been no little misunderstanding among all classes about Medical Ethics and Medical Etiquette; but there is really no mystery about either. As they have come to us, they are the product of many generations; they have been evolved from the experience of a profession which has always had special opportunities for studying the structure of organised human society. It has been represented at the universities and schools, and has known the best of scholastic learning; it has been engaged in the affairs of war and of peace; it has known the homes and lives of the rich and the poor, the powerful and the dependent, the most and the least intelligent, the good and the bad among men. The conclusion the profession as a whole has come to concerning their relation to one another and to their patients is the result of this wide experience. Its philosophy has been of the inductive rather than of the intuitive school; but it is interesting to notice that, with regard to the practical conduct of life, these conclusions are the same as have been arrived at by the highest religious teachings. "Do unto others as you would wish others to do unto you" is the foundation of all medical ethics. And this rule has been reached, not on sentimental or emotional grounds, but as a deliberate and—one might almost say—a scientific opinion as to what is necessary for the carrying on of human society. A deep sense of the duty of the doctor to his patients has been impressed on their students by all the best of our teachers.

The oath of Hippocrates (born about 460 B.C.) is famous and well known —"With purity and with holiness will I pass my life and practise my art. Into whatever houses I enter, I will go into them for the benefit of the sick, and will abstain from any voluntary act of mischief and corruption. What I see and hear in connection with my professional practice or not in the life of men I will not divulge." Such oaths are no longer taken or required, but they are the law of the profession notwithstanding, and have been taught by our great teachers by the best of methods—their own example. From Sydenham and Harvey to Simpson, Paget, and Jenner, the English School of Medicine has followed the Greek in this respect, and the law has added its powers to the moral forces. If any medical man is guilty of conduct deemed infamous in a professional sense his name is removed from the register of qualified practitioners, on proof being given of his guilt. But the moral force is the stronger of the two. Few men are to be found who will face the condemnation of their own profession, and every practitioner knows that he will deserve and get that condemnation if he breaks the moral code laid down in effect in the Hippocratic oath.

Elaborate codes of ethics are in existence, and perhaps they might be more studied in detail in our schools and societies than they are: but they deal only with details, and they are all founded on certain broad principles. Thus, no man may have anything to do with secret remedies or secret methods —he must make no profit out of such things. The Chamberlain brothers invented obstetric forceps, but they are remembered chiefly for their infamy in concealing, for personal profit, a discovery which was of universal value. If a new remedy is useful for the relief of suffering it should be placed in the

hands of all who can use it. If, as often happens, the supposed discovery is a mistake, it is the more quickly detected ; if it is useful, its range of usefulness is more quickly determined.

All the great discoveries of our time have been improved by others than the discoverer. Indeed the advance of any science would be impossible in the face of secrecy and jealousy in dealing with new facts.

In the complicated business of professional life many minor regulations are necessary. The need of courtesy and fair dealing between man and man is nowhere more needful than in the relations between doctors. There is nothing which more surely stamps a man with the seal of mental and moral inferiority than the habit of decrying the work of his professional brethren and exalting his own. Even without that there are numerous causes of quarrel and misunderstanding between the best-intentioned of men. Every association of medical men knows by experience how hard it is to keep the peace among a few of its members. The code of ethics tries, at least, to do this, and the more nearly it is followed the chances of misunderstanding become lessened.

Another feature of the code is its condemnation of advertisement—either direct or indirect—among medical men. I will not dwell upon this beyond saying that this conclusion is a well-reasoned one, not arrived at from any pharisaical belief that we are better than our neighbors. Methods which are unobjectionable in ordinary business are not suitable in ours.

But, if we object to a policy of hustle, we are fairly determined not to be hustled. We believe that our work is important, that for its best development it must be individual, and that the man who can do good work ought to be paid for it. We are at issue on some of these points with some very respectable gentlemen. The profession has always unstintingly given its services freely to hospitals, and there is no medical man in large practice who has not on his list a number—sometimes very large—of free patients. A system has grown up whereby men of small income may get cheap medical attendance by a process of mutual insurance—the well-known club system : and this has been developed with the entire approval and assistance of medical men. But many men and women, both in town and country, have not hesitated to take advantage of the free work done in the hospitals and elsewhere. They sometimes defend themselves by saying they pay taxes. So they do for charitable aid, but they do not seek to take this out in doles from benevolent boards, and the hospital is as much a charity as the benevolent asylum.

A scheme, which may be called the Medical Syndicate, has found wide favor in some quarters. It proposes to hire a doctor for a salary, not too liberal. and assign to him the task of caring for the sick of a given district. Everybody is eligible for membership, and every member is of course entitled to his full share of the doctor's time and skill. Such syndicates strike at the very root ideas of our profession. We believe, if they were to be successful, they would destroy us by taking away individual effort, ambition, and independence.

The club system, admirable as it is, has been terribly abused. Especially in country districts, well-to-do men, lawyers, bankers, men of business, farmers, &c., have become members of working men's lodges, and have saved their pockets at the expense of our profession. We believe that this development of the system is bad, for the same reason that the syndicate system is bad, and that the chief sufferers in the long run will be the public. Not that the club doctors are inferior to their fellow practitioners in any way ; that is not the case : but because too much work is thrown upon them—often of a trivial nature. They have little leisure time for holidays, and that personal

bond which is needed for sympathetic work is very often absent. We have to fight the policy of hustle in all these forms; and it is not altogether for our own sakes. We are of no use to anybody if we are not efficient, and we trust to that fact for the destruction of systems which would end in making us inefficient.

These differences of opinion are to be regretted, but, as quarrels go, ours with a section of the public is not a very bitter one. And as we are not a weak or disunited body of men, and only desire to see justice done, we believe we can discuss most of these matters with our temporary opponents with a good prospect of coming to an understanding.

It would be impossible to discuss the ethics of our profession without referring to these matters, which greatly affect the lives of all of us. The student of to-day will be guided less by any sentiments we profess than by our actions. On the whole, I think we are inclined to stand by one another—the strong tend to help the weak, the senior man the less experienced, in all legitimate ways; and it may be fairly claimed that the influence brought to bear by the profession itself on its younger members is as high as it has ever been.

Of this we may be sure—theories of practice and science will vary. We shall add our portion to the lumber heap. Many of the details on which we pride ourselves will be as dead as Galen's twenty-five pulses or Lister's spray—as absurd as Don Quixote's attack on the windmills—to other generations. But, both for ourselves and the public, it will never cease to be of the first importance that our standard of conduct should be high. We may use the Hippocratic formula, "With purity and holiness" or the mordern colloquialism, "To play the game," but the meanings are identical and the requirements are the same.

The part played in the education of the student by the times in which he lives—by the world spirit—is hardly second to the professional influences of which I have spoken. It was not by accident that the epoch of the Hippocratic school was the epoch of Socrates, Plato, and Phidias—of all that was best in Greek life—that age produced the School of Medicine which guided and dominated science, until science, philosophy, letters, art, and religion were overthrown by barbarism and superstition. Under the vigorous rule of the Arabs—while they were a conquering and expanding people—medicine, with other sciences, again prospered and advanced. A long time of mental stagnation in Europe ended with the Renaissance. It was a true re-birth when Italy—and, through Italy, Europe—found again the old learning of Greece and Rome: and this not only because of the infinite value of the art and literature of the past, but because it widened the horizon of the human mind, cramped by the centuries of struggle for existence which followed the downfall of the Roman Empire. These struggles were to decide whether a Tartar, or a Saracen, or a Christian force should be the master of Europe, and, in the clash of arms, Laws and Letters were silent. Then Dante and Petrarch spoke, and all Europe listened. The arts became alive again, and modern science was born. The new world of America was less to mankind than the new worlds which were thrown open to students who followed the teaching of the universities of Europe. Authority yielded to observation—not with meekness, we know; but the new force was irresistible. The revival of the old learning in medicine was followed by an earnest desire on the part of students to follow the methods of the Greek and early Alexandrian Schools. Nature must be studied, and above all the student of medicine must know the material on which he has to practise. But it was not until the sixteenth century, 1,400 years after Herophilus had taught human anatomy in Alexandria, that Vesalius in Brussels aroused the enthusiasm

of all the students of medicine in Europe by his anatomical demonstrations, and that the real study of anatomy began, and with it was laid the foundation of scientific medicine.

It is no part of my programme to consider details of medical history, except in so far as they illustrate the influence of environment on medical education. The force of the Renaissance has continued: it has moulded the whole constitution of society, and made possible the discoveries of science, the explorations, and the colonisations of the nineteenth century.

There has been much talk of political federations, but steadily, and without any apparent deliberate design, an intellectual federation of the whole world is gradually taking place. Each nation is learning that it has much to learn from every other nation. Every new discovery in science or philosophy is international, and has to stand the scrutiny of a thousand investigators. Theories which have stood for centuries are subjected to the same criticism. If a nation is backward in growth, if it has allowed itself to be smothered by sloth, corruption, and ignorance, every wind of heaven smites it, and—by reformation within or enemies without—it must be changed, or die.

We have seen but yesterday a new nation enter the field. If Japan has gained much from the Western peoples, she has added much to the common store—examples of patriotism, valor, honor, and thoroughness in all her enterprises, which cannot fail to stimulate and raise the standard of national aspirations.

There have been no such "spacious times" since the world began. Into this scene of international activity Australia has entered as a comrade and fellow-worker with other nations—it may be only on trial, for she has yet to prove herself worthy. But it is well to note that there is now, as there has always been, another spirit abroad. (It is impossible, in this connection, to avoid the language of metaphor—it expresses at any rate what we want to make clear). There is lust which seeks only its own pleasure, greed which seeks its own gain, a sneering devil which can pull down and destroy, but can build up nothing. There are tyranny, envy, and ignorance in abundance—evil influences which have called themselves by many names, porphyrogenitus born in the purple, autocrat, democrat, friend of the people: and all these influences are at work trying to destroy whatever of good the ages have built up for us. As this battle with the nobler spirit of the age goes, so shall we rise or fall.

A matter which deserves extended consideration, now and at other times, in connection with all medical schools and universities, is the health of the students. A sickly medical student is a satire upon our art. If a man cannot keep that body over which he has sole control in good order, how can he expect other people to trust to his skill in *their* ailments?

We talk about *mens sana in corpore sano*: about "health being a man's best asset," and so on. What good does it all do? Before a man has learned the meaning of such phrases he has often established faulty habits of study and living which stick to him. Too many men break down in their student career and afterwards; too many men graduate and find that they have lost mental and physical elasticity in the process of learning—not to regain it, perhaps, for years. Is there nothing to be done, systematically and steadily, in the way of attending to the students' health? Should we be content with the old rule-of thumb methods?

The making of Utopias has always been a fascinating pursuit, even if the ideal is never realised. The university might well start with the fundamental idea that it was its business to teach its students the art of living as well as learning—and through its students the whole community. This was the old Greek ideal, expressed in the words "Music and Gymnastic"—the care of the body as well as of the mind.

There would need to be a residential college. Remembering that we are dealing with young men, not with boys, a great deal of the management could be done by the students themselves. The senior medical students might form a health committee, whose business it would be to carry into practice the lessons they had been taught in their anatomy, physiology, hygiene, and medicine classes. On his first entry into the college each man would undergo a careful physical examination—surely as important a thing as matriculation; height, weight, chest expansion, muscular force, and other important matters being observed. Monthly, thereafter, his record would be noted, so that the earliest sign of any failure in health might be detected.

A sanitary committee would supervise the household arrangements, drains, ventilation, baths, &c.

A gymnastic committee would see that the right kind of exercise was prescribed for each student.

A catering committee would supervise the food arrangements.

We preach the virtues of fresh air: the bedrooms would be as well ventilated as those of a sanatorium phthisical patient.

We preach early rising and cleanliness: The student would rise at 7 in the winter, and 6.30 in the summer; and begin his day with some form of exercise and a cold bath.

We preach moderate diet: Here there would be room for observation and experiment. The food would be the best that could be bought; the cook the best that could be hired. In these directions there should be no false economies. But the food should be plain, suitable for students, who need no heavy or late dinners, no stimulants to appetite.

It is not beyond hoping that a university might be found bold enough to establish a school of domestic economy in connection with such a college; and if women could only be induced to believe that there was as honorable and profitable a career for them in this work as in nursing, or any other occupation, one at least of the crying social wants of the day might be satisfied. If the ideal could be carried out, such a college would be a practical school of hygiene. The doctor who had been trained in it could say—"I have done these things I recommend." He would know from practice much of what he now only learns in books or lectures. He would learn something about administration of affairs and responsibility.

Many possibilities might flow from such co-operation. We might see summer camps formed for boating, shooting, and fishing, for the study of nature, and as health resorts for those who were run down from any cause. For non-resident students the college would be a club-house, a place where a good meal could be obtained, and social intercourse encouraged. We might see fewer men going to cricket and football matches, and more men playing football and other games. Surely there is nothing more lamentable in modern life than the spectacle of thousands of able-bodied young men contenting themselves with watching others playing, and having no pursuits of their own. To the university man, at least, the study of "Gymnastic," ought to be an integral part of his daily life.

The influence of such an institution would not, and could not, end with the student himself: it would extend into the daily life of the whole State; and if our universities are destined to have their rightful influence, they must be prepared to play their parts in a wider field than they have occupied heretofore, and to take a broader view of their duties.

Much would depend upon the students themselves, but not all. They cannot be expected to have either the experience or the initiative of their elders; but if they discuss these matters among themselves, and show a desire to try such an experiment, they will be supported by all the special

knowledge and skill that the medical side of universities possess—of that we may be sure. But the young man is above most things conventional. He does not like trouble, he hates ridicule, and he has a wholesome conservative dislike of innovations. We are told that, in a state of nature, if a pack of hunting dogs has marked down a quarry and one of their number turns aside from the hunt on private business with a hare or a rabbit, the rest of the pack will tear him to pieces. Young men—perhaps all men—are pretty much like dogs in this respect. But if the quarry is likely to furnish meat for the whole pack, there may be found leaders who will try for it.

I have often thought the student at a university is too much like an egg in an incubator—so much heat, so many days, and the chicken steps out ready to fight for himself in the world. The university might do something better for him. It might encourage comradeship, make him feel more like a child who has an alma mater which educates him, cares for him, corrects him, perhaps, but is proud of him as a son who will one day be proud of her.

Leaving this question of the health of the student, let us now turn to the machinery whereby his technical education is determined. The day has gone by when the need of local medical schools might be questioned: the matter has been settled. Victoria has granted degrees in medicine to 665 students, Sydney to 285, Adelaide to 49, and New Zealand to 88. Most of these graduates are now in active practice in the colonies and elsewhere.

For an Austral student there is much to be said for an education in one of his own universities. The standard of education taught and required is not inferior to that which obtains elsewhere: the cost is less. The young man, at the most plastic stage of growth, is not cut off from those home and national influences which tend to bind him to his home and to his country. He studies the actual types of disease, and the local conditions he is most likely to meet with in practice. For the States themselves it is of importance that they should be able to produce their own doctors at home: it means that an honorable career is open to many who could not adopt it if the education could only be procured abroad: it means also a higher standard in the profession both among the teachers and those who are not engaged in teaching. For the poorer classes it means more abundant means of getting the benefit of special knowledge for the prevention and cure of disease, and for raising the standard of health.

On the other hand, a good many things may be urged in favor of educating the student in Europe. Twenty-five or thirty years ago society throughout these States was much more cosmopolitan than it is now. Most of the men engaged in active life had travelled at least from Europe to Australia, and many had a wide experience of nearly every part of the world. By the passing away of this older generation it cannot be denied that a strong tendency to provincialism has sprung up, and this has manifested itself, among other ways, in an inclination to magnify our own institutions, our own people, and, to some extent, a failure to understand our relations with the mother-country. It may well be urged that no better means of counteracting this spirit could be found than to strive to send as many of our young men as possible to seek at least part of their education in Great Britain. Not only would they in this way secure the best teaching by the best men in institutions of historical importance, but they would gain a larger knowledge of the world in which they live, and would be more likely to become citizens of the Empire rather than of one small State.

Probably things are best as they are. Some men go home, and return with a leaven of wider experience. Some remain in the States and are stimulated to better work by the knowledge that they have to compete with men from other schools.

In Australia and New Zealand there are four universities which have medical faculties. These universities are practically supported by the States in which they are situated. This arrangement is, perhaps, the best which can be imagined. It allows of unlimited expansion through affiliated colleges, while retaining the standard of education unaltered, and secures for the universities an independent position.. When we compare our condition in this respect with other countries, we can see. I think, that it has many advantages. In America, where great wealth has been given and is being given to educational institutions, the diversities of the teaching standard are very great —while the best schools are among the best in the world, the worst are among the worst. The average payment of teachers is small, so that in many cases the professors have to be men of private means, or be content to live in comparative poverty. A recent writer, Mr. J. A. Hobson, writing in the *Independent Review*, speaks strongly of the harmful influence exerted on university teaching by the endowments of millionaires. I would not venture to criticise any American institutions in an unfriendly spirit, but it may be well to see what an expert has to say about the dependence of universities on private benefactions. He declares that the independence of many of the universities has been greatly imperilled. He speaks of professors removed from their chairs because the teaching was unpalatable to those who had endowed them ; and, further, states that while it is of great importance to attract money to an institution, the heads of the university are often chosen less for their academic distinction and capacity for government than for their social powers and ability to please wealthy men. Who pays the piper calls the tune ; and Mr. Hobson thinks that a privately endowed university is not in a position to discuss freely many urgent social problems in ways which might be distasteful to the moneyed classes.

In Great Britain, although the Medical Council has done much to raise the standard of education, the great number of bodies which grant licences to practise medicine has not always tended to a high standard of efficiency.

In other ways the State universities have certain advantages over those of older countries. They have been able to pick out the best from many systems, and representatives of nearly all the educational bodies at home have from time to time helped to mould our newer ones. Not Oxford or Cambridge, Dublin or Edinburgh, or London can be said to have dominated our universities. Something different from any of these has been produced—I do not say something better. The main idea of the Austral universities is that their functions consist of collecting and dispensing knowledge of all kinds that can be useful to man. In medicine they have aimed at maintaining a high standard of preliminary education, instituting examinations which shall test the work done by students and teachers, and finally deciding when the students are fit to become practitioners of medicine. It is to be hoped that they may long continue to be free from sectarian and political influences ; from the presence of the idle rich, who, in so many cases in Europe and America, lower the standard of education and increase its cost—I may almost add, from the endowments of millionaires, which might lessen the independence both of the teachers and of the students. A wise statesmanship will aim at supporting freely its own universities without asking charity from any man, while always welcoming free gifts which may extend their power and usefulness.

Our universities demand either a degree or a matriculation examination modelled largely on that of London University.

While recognising that it is useless and unjust to launch a student into the enormous mass of detail which forms the medical curriculum without some evidence that he has the mental training and capacity to grapple with

it, opinions will continue to vary as to the nature and extent of this required preliminary training. The present state of the subject must be looked upon as provisional only. If time were no object few would be found to deny the reasonableness of requiring that Greek, the source of nearly all modern and ancient thinking, should be one of the foundation subjects ; that Latin, once the universal language of learning, should also be studied ; that logic should be taught formally ; and that French, German, and English, and the elements of Physics, Mathematics, and Biology should be known to the student.

But art is long and time is short, and there is a limit to the knowledge that one small head can contain. When scholarship meant Greek and Latin the field was limited, and it was possible to be profound. It still is, for the specialist ; but common experience has demonstrated that the time spent on the dead languages is for most young people time wasted, or at least not adequately represented by the knowledge gained. As far as the medical curriculum in Australia is concerned, they seem likely to be eliminated entirely.

One may regret the loss of Greek, but it does not mean that the Greek spirit need go—the spirit that urges men to be artists in Plato's sense, not craftsmen : and when Latin goes it will leave behind it a sense of loss as if an old friend and servant had left our house for ever. It was not only the key to a great literature, but it was once the universal language of learning. Among many disruptive forces it was once for all Europe the common bond which made the united work of science possible. But to-day things are different. The growing demands of science need the utmost flexibility of speech—an accuracy and precision which are not possible in an alien tongue, however carefully studied. If a man has anything to say he must say it in his own mother tongue.

For our students English is the language they ought to learn to use and understand first of all, and if any fault is to be found with preliminary examinations it is they do not sufficiently recognise this. Far too often we meet with students who can neither write good English nor give an account of their own literature and history. Much has lately been said and written about the study of nature, but the most important part of nature for us is the right use and understanding of words. I hope if we are doomed to lose Greek and Latin we shall have a more thorough study of English in their places. French and German are second in importance. The man who spends his scanty leisure in studying the latest discoveries of science in German, French, and English, and perhaps in other tongues, is somewhat of a fancy picture. Here, as in other branches, we have to depend on the specialist in journalism to keep us acquainted with what other nations are doing ; and we can be sure that nothing of importance is published in any language which is not quickly translated into English. It is good for those who possess a knowledge of these and other tongues, but we want no collection of smatterings. If they are demanded let the standard aimed at be that of Baltimore—a good working knowledge of French and German, both for colloquial use and for reading.

Once the student has passed the preliminary stage he finds himself at the beginning of a five years' course of hard work, the general arrangement of studies being the same in nearly all civilised countries. One year is devoted to preliminary science training ; he learns something of physics and mechanics biology and chemistry—subjects useful in themselves, and which help him to know something of the scientific conception of the universe and the forces which act on it, and useful also in helping him to methods of study. Purely scholastic methods which have prevailed in his earlier education now become united largely with laboratory work. Of the wisdom of this part of a student's training there can be little doubt : it is, in some ways, one of the most practical parts of his course. Year by year it is more necesssary to know something

of electricity, light, and heat. The X-rays have demonstrated how, from unexpected sources, new power may be lent to therapeutics. Finsen's experiments and practice with various colored lights on living organisms is only second to the X-rays in value; and in other departments of physics the studies are at least of equal value.

No less important are the lessons derived from biology; and perhaps the special application of these sciences to medicine is not always born in mind by teachers. We are still somewhat under the domination of the school which regarded botany as being specially concerned with medicinal plants, and with the naming and identification of species. More important is the consideration of the vital phenomena of plant and animal life, especially of the lower organisms and their relation to the higher living things. These lower organisms are now known to play an important part in the production of most of the diseases which attack men, animals, and plants. From a study of these diseases we can learn much of those which affect the human body. Pestilences occur among plants such as the potato or the vine. Examination shows that the causes of these diseases are to be found in the presence of a fungus with enormous powers of reproduction. There are two ways of dealing with the growth of this fungus: one is to attack it by germ destroyers, spraying, fuming, &c., and the other is by seeking to improve the condition of the plant and the nutritive properties of the soil in which it lives. These two methods are analogous to the two great methods in use in medicine: we can either attack disease by strengthening the organism against attack, and enabling it to throw off the germs which try to live upon it, or we can try, by the use of antiseptics and antitoxins, to destroy the lower organisms in and out of the human body. The causes which lead to the degeneration of individuals and of species can be studied freely, and, in the case of plants at least, without hurting the susceptibilities of any society for the prevention of anything: and the lessons to be learnt are invaluable, for life is one, and the laws which control the welfare of one set of organisms are the same as the laws which control the lives of all.

These studies are productive of valuable general notions which bear on the science of medicine, and are also becoming daily more essential for their practical details. The methods of investigating the life histories of bacteria and the lower animal organisms is now essential to an understanding of many diseases, and is begun at least in the biological course. The same things may be said of chemistry in all its branches, but especially where it deals with organic materials. It is true that all these subjects are so wide in their scope that the medical student can only touch the fringe of them. But, in his future career, he will be confronted from time to time by problems which must be referred to the chemist, biologist, or physicist, and he must be taught at least to know these problems when he sees them.

To a few students, no doubt, such studies are mere taskwork, to be got through mechanically, with as little effort as possible, but to many it is the introduction to that knowledge of nature which it has been the object of the wise of all ages and nations to strive to attain—a glimpse of that harmony and beauty "which will never die into nothingness," and which will be a retreat for him throughout life from the daily cares and worries of his business.

The studies which follow are all determined by his life and business, and are on the same lines in all medical schools. Anatomy and physiology, disease and the anatomy of disease, methods for the prevention of diseases, and their cause, the legal aspect of medicine—these are the objects of work for four busy years—or five, if the new proposals of the greatest English examining bodies are carried out. Of the details of most of these studies it is not my business to speak, but there are some general features which demand attention on such an occasion, both by the public and the profession.

We have always been expected to have accurate knowledge on all subjects connected with our work, and we have always been hampered, and often prevented, from gaining that knowledge. Here I would like to disclaim any idea of making a divinity of knowledge, and sacrificing anything and everything to gain it. It is bought too dear if it is purchased at the cost of cruelty to man or beast; and, unfortunately, the minds of men are so constituted that unlimited power of experiment and observation have led to abuses, and occasionally to horrors, which have brought about reactionary resistance to legitimate observation and experiment. I know of no profession or business which is not the better of outside criticism and restraint. Our medical schools and training hospitals are the better for lay management and criticism—our professional work is the better for it. We need never expect to be free from such influences; but it is our right and our duty to protest when we find superstition and sentimentalism and loose thinking interfering with the objects of our existence.

I have alluded to the long struggle which anatomists had to engage in for centuries. Much of the opposition was and is purely atavistic. It is rooted in the ideas of primeval man as to his future state. If his body was mutilated in this life or after it, he entered the spirit world a maimed being: if his body was treated with disrespect, his angry spirit would haunt and injure the survivors who permitted the dishonor. Consciously, or unconsciously, these ideas have stood and still stand in the way of the rational scientific study of the structure of the human body in health and disease. And only as the wiser view gains ground, viz., that the body is no more than the outward garment of the man, can we hope to find an absolutely scientific practice of medicine. Our knowledge of disease is founded mainly on our knowledge of normal anatomy and the alterations caused by diseased processes. We must have freedom in the States to examine with the scalpel and the microscope if we are not merely to become the depositories of other men's observations. This is one point on which we have to complain often of the attitude of hospital managers and of the public, and it cannot be too strongly urged that the loss is theirs as much as ours when these examinations are neglected.

Another point on which there has sometimes been misunderstanding is as to the free admission of teachers and students to public hospitals. This has sometimes been opposed on the supposed ground of the interests and feelings of the patients. I may say I have no sympathy with any who would claim for university teachers a monopoly of hospital appointments. Much of the most valuable work, both in practice and teaching, has been done by men who have had no connection with the universities. A public hospital should be open to all duly qualified men who are willing to give their services. It is true that unsuitable men are sometimes appointed, but the same thing applies to the duly accredited teachers. The advantages for good professional work are great in all hospitals, and these advantages should not be monopolised by any body of men. But it is not likely to be denied by anyone conversant with the subject that the standard of work is always higher among all the staff where there is a medical school than where there is none: thus it is in the interests of the patients as a whole to have such schools. But there are some principles of selection which ought to be observed, if the right men are to be chosen. Other things being equal, the man with special surgical qualifications should be chosen for a surgical post, with medical for a medical.

The senior staff should be recruited from the junior. Each hospital of any size should have a senior staff and a junior staff in all departments. It should be the business of the junior staff to assist the senior, to take their places during absence for holidays or other reasons, and in this way they

should be trained for the responsibilities of the senior position when their time comes. The present method in many hospitals of appointing men to the most responsible positions—those of physicians, surgeons, and specialists—without any preliminary training to demonstrate their fitness, is in every way absurd, unfair to the patients, and unfair to those practitioners who are prepared to take trouble to fit themselves for doing the very best kind of professional work.

There ought to be some continuity of work. Annual elections by hospital subscribers mean touting for votes, and that appointments are often given to the most unfit. Where medical schools exist in connection with hospitals the governing authorities of these schools should have a fair representation on the hospital boards. This is not undemocratic : it is only recognising that the medical school really represents the whole of the people who may have to depend in sickness on the skill and honor of its graduates, and who, through Government subsidies, pay a large part of the cost of maintenance of hospitals.

We in New Zealand, at least, have reason to recognise gratefully the attitude assumed, especially of late years, by our various hospital authorities towards our Medical School. If friction has occurred it has been temporary, and never serious. In advocating representation by the university on the boards of hospitals used for teaching purposes it is therefore not done in a carping spirit, but because I believe the change would result in a better understanding of the needs of the university on the part of the hospital trustees, and a better understanding on the part of the universities of the difficulties which have to be overcome.

If the presence on a hospital staff of specialists engaged in teaching is an advantage to the institution and to the patients, by increasing the precision and accuracy of the work done, this is largely due to the presence of students eager to learn and critical of methods. But besides this, the students themselves are, I believe, a great gain to the hospital. Less instructed than their contemporaries very often in the business part of life, there is no class of the community which has anything like the training and knowledge of human nature which these young men get. They bring into the wards an atmosphere of youth and energy and kindliness which is quickly felt even by those patients who have most dreaded the experience. Children delight in them—older patients talk freely to them ; they get at facts, often important, which their seniors would never get hold of ; they keep in touch with all the details of their cases, and render invaluable help in treatment. Follies they have, no doubt, and things could be, and are, cited to their discredit—as Paley said, the man who is never foolish is probably a fool all the time. But I know of no men who are in closer sympathy with the sick and the poor than are the medical students, nor who are more welcome and more trusted by them. And yet we have often had trouble to get them duly instructed—trouble arising chiefly from theorists and morbid sex-obsessed persons. They are the men who are to be the help-bearers to the sick and to women in their troubles, and yet we are told it is indecent, indecorous, improper, that they should have the opportunities of learning what these sufferings are. It is pitiful that we should have to protest against these objections. If we have to protest in vain, then let us shut up our medical schools and take again to amulets and charms and spells, or their modern equivalents—electrical belts, magnetic rings, and faith-healing.

At one time a man might hope to be master of the whole of the healing art, but gradually medicine has been split up into a great number of departments : and while it is possible and necessary for a man to have an intelligent comprehension of them all, it is not possible for any man to attain to expert knowledge in all.

As nearly all these departments are represented at the Congress, I need not speak of that special branch whose deliberations are inaugurated by the assembly. All have their foundation in anatomy and physiology, and all depend on a close study of nature, on the observation of innumerable details, and on the philosophical consideration of facts accumulated.

Let us look at the task of the student of medicine. First he has to learn from books and lectures the main facts about disease which have been accumulated by others. It has been the fashion—it may still be in some quarters—to sneer at books and lectures, to say that only practice can teach medicine, that the book man is good for nothing, and so on. All this is pure nonsense. Everything that we have in the way of knowledge is to be found in books, if we only had the art to extract and apply it. Theory and practice are not opposite and incompatible, but are complementary to each other—each is incomplete without the other.

The book man may be dangerous, but he is not half so dangerous as the so-called practical man who neglects books. Serene and imperturbable in his ignorance, he recognises nothing that has not come within his own narrow experience.

The student must study in books, and if he cannot transfer into his own head more than a small part of what he reads, he will at least know where to look for his facts when he wants them.

Good text-books are, fortunately, abundant, and it is due to the modern co-operative system of work that these text-books can be produced with the latest facts in pathology aud therapeutics in every part of the world, set clearly before the student. If we compare a text-book of to-day with that of a hundred years ago we shall see the difference in method.

Cullen, of Edinburgh, was one of the famous Edinburgh teachers, and his first "Lines of the Practice of Physic," 1777, had a great reputation well into the early years of the nineteenth century. The edition of 1816, edited by Dr. Reid, is a logical methodical book, clear and sensible always. Specially valuable and interesting are the references to digitalis and its uses, to Fowler's recent introduction of arsenic in the treatment of intermittents, to Currie's views on opium in fever, of affusion in scarlet fever, and to Baillie and Hunter on hydatids of the liver; but one is struck with the smallness of the circle of observers, and its limitation to practitioners in Scotland and England. Laennec's great discoveries are not mentioned, although we know they had then revolutionised medical work in France and on the Continent. This speaks partly of conservatism, but more of the want of communication between different centres.

If we look at a modern text-book we find everywhere the traces of the work of those useful specialists who devote themselves to collecting, from all sources, the work of each observer. No journal is too obscure, no language too little known, to be searched: no matter where a man lives, he has the means of knowing what workers are doing in any branch of science he is interested in.

The advantages to the student are clear. He is no longer a man of one school, however great—Edinburgh or London, Paris or Baltimore. Other factors, of course, come into play in the making of the text-book. The free communication between countries, the journals, the societies, all do their part; but the great bulk of the material produced makes it difficult to keep in touch with even a part of it, and makes the work of the co-operative specialist absolutely necessary.

If the personality of the teacher is a less dominating factor than formerly in the medical school, his duties are by no means at an end with the evolution of the most perfect of text-books. In the nature of things these books can be

no more than abstracts of knowledge, and they must be supplemented by the living voice, which will illustrate by accounts of cases, pick out the more from the less essential, emphasize by reiteration where it is needed, question, and demonstrate.

Not fewer, but more, lectures are needed—lectures on cases, on treatment, on underlying principles. The spoken word, representing experience, lives in the mind, attracts the attention, when the written word imparts but the shadow of a recollection. The business of the university is to teach : not merely to point out to the student where he may get his knowledge.

The same methods of co-operation have produced for the more advanced student—the practitioner who does not disdain to add to his knowledge—many admirable systems of medicine and surgery, consisting of monographs written by specialists who know their own subjects as thoroughly as they can be known. The man who in his former student days has learned to read can find in these volumes help and instruction in all the experiences of his medical life ; and if he systematically seeks that help he will find that, along with the practice of his profession, he in a few years will have educated himself to the highest standard that can be attained. We all know men who have done, and are doing, this in every department of practice.

There are two faults in all our books, which will probably never be entirely removed, but which might easily be lessened—the use of foreign words where English could be used, and the increasing use of personal names in describing diseases and symptoms. Why should we say Erythrocyte and Leucocyte instead of " red cell " or " white cell " ? And so on, with hundreds of words. The Greek used the simplest and the most direct language he could command to describe what he saw—white skin, red skin, swellings. With no knowledge of Greek, we use Greek to describe what we see, or, with as little reason, Latin, or French, or German ; and this method of naming things does not help to clearness of ideas. More mischievous is the abuse of proper names. Who can hope to keep up with the increasing torrent, however gratefully we may recognise the new facts which have been added to science ? Even the oldest and most venerated names become a snare to the student, instead of a help. What is Potts or Collis to him, that their names should be given to special fractures, for his undoing at examinations ? He does not care whether Bright was a statesman or a physician, and he is quite as pleased to think that the nodes of Parot are so called because they resemble the beak of a parrot as that they commemorate a distinguished French surgeon. And really such names are useless. If we want medical history, let us make it part of our curriculum. There is something to be said for that ; but there is nothing for this quite ineffective way of keeping great men in remembrance. A really serious effort to purify medical language is worth making, and I think that we would find that English could be used with advantage in a majority of cases of disease and symptoms.

There is still another class of books for which there is a future. Hutchison has led the way in his excellent " Archives." Byron Bramwell has done something of the same kind in his " Clinical Studies," and in his " Atlas of Clinical Medicine," a costly book, but surely the most magnificent volumes that have ever been produced in pure medicine. Lawyers have their " Leading Cases " : why should we not have the same thing done systematically in medicine ? Would it not be possible, and perhaps profitable, to the publisher to have the whole field of practice illustrated by actual cases, as Bramwell has done for a part ? In the medical journals of the world there is abundance of material which might be made available. There one might find thousands of cases accurately observed which could be selected, and added to, where necessary, by fresh observations and findings in pathology and therapeutics.

Of great interest and value would be the records of the original cases which led to the definitions of various forms of disease. Such records are to be found in the writings of nearly all original workers. Graves would yield the brief notes he made on exophthalmic goitre, Addison his cases of adrenal disease, Bright his observations on renal trouble, Jenner, and others of his time, their records of typhoid and typhus. Such volumes might be illustrated where necessary—not as elaborately as Bramwell's Atlas, perhaps, but well enough to help the text.

Such a record would never be out of date. Systems and theories change from time to time, but a case well studied is always of value.

When we are puzzled in practice, it is from the details of actual cases that we learn, rather than from general abstracts of knowledge, however skilfully done.

If I have tried to emphasize the value of bookwork in our plans of medical education, it is because no more dangerous attitude can exist among students than that of despising books, and not to exalt that branch above the equally necessary practice study which must go on at the same time in the wards and in the clinical laboratories. In these branches we have no new principles, nothing that our predecessors did not recognise as fully as we do. We have to study nature, and observe as closely as we can the actual facts which are before our eyes ; but we have the advantages of innumerable helping methods which were unknown to them, and which are continually being added to. The mastery of these methods is a great part of the student's work. If his training has been sound, he knows what a healthy body is : he has now to find out in what ways that standard may be departed from. Gradually every organ and every function of the body has been brought under exact observation and analysis. Thanks to Avenbrugger and Laennec the lungs and heart can be explored, the thermometer reveals the exact degree of fever present, secretions can be analysed, the microscope shows the minutest changes in the blood and tissues and the presence of harmful organisms, the Röntgen rays show change in the bones and soft tissues. It is not claiming too much for medicine to say that its part is to estimate the health value of the human body as the engineer estimates the strength and value of the materials he has to use, and the strains they may be subject to, and that he has, as a rule, the means at his command to make that estimate. We sometimes hear that medicine is not a science—that it is purely empirical, that we pour drugs, of which we know nothing, into bodies of which we know less. Never were gibes more empty and meaningless. What is science if medicine is not a science ? We know there is no such thing as absolute knowledge among men. But we do not say there is no science of navigation or shipbuilding because ships are lost at sea, or that there is no science of engineering because of a Tay Bridge disaster.

If science means the systematic accumulation of correct knowledge by observation within the limit, but to the full extent of, human powers, then medicine need not fear the test, and can claim its place with the other departments of human knowledge. And not only that, but it must remain where it has always been—at the head of all the branches of the science of healing. Its business is to take the most comprehensive view of the human body in health and in disease, and of all the methods whereby the one may be maintained and the other eradicated or modified. Whatever may be the skill of the specialist, the surgeon who deals with all the body or a part, it is at his peril and that of his patient that this view of the relations of the whole organism are disregarded. No one values the skill of the surgeon more than the physician ; and I believe that, equally, the wise surgeon recognises that in most of his cases there is a medical aspect which is better observed by the physician

than by himself. And, striking as are the achievements of the surgeon, it is still the province of the physician to deal with the greater part of the maladies of the body ; and the public may rest assured that the study of all the means which can help to make men efficient is being pursued in all parts of the world with an energy and enthusiasm which is exceeded in no branch of science.

It may be asked if the curing art has kept pace with the acknowledged advance in the science of disease. I have very little hesitation in answering "Yes" to this question. The most striking proof of general improvement in the healing art is to be found in the improvement in the value of life during the last hundred years, or less. In the early part of the nineteenth century it has been estimated that the average age of death was about 20 years ; it has now been raised to at least about 40 : and this has been obtained through the application of commonsense methods to daily life. Human beings flourish when they get a plentiful supply of fresh air, sunshine, simple food, and exercise. This has been the reasoning which, from the time of Hippocrates, doctors with more or less success have tried to instil into the public mind. In the matter of the life of the individual medicine has been no less successful. It has made it its first business to get an accurate grasp of the real nature of disease and degeneration ; and, having done that, it has aimed at combating these causes. Nothing is easier than to despise the past and to see nothing in it but its blunders ; but our whole knowledge is founded upon the work done by those who have gone before us, and they have left behind a vast treasury of material, which is of the greatest value in therapeutics. If we have dropped much of these methods we have retained more. Only those who know little about medicinal substances deride them, on the principle that it is the bad workman who quarrels with his tools. There are few diseased conditions which may not in some way be helped or modified by the judicious use of the medicines which are at our command. We are learning something day by day—something of the limits of our art and many new possibilities of its extension. There are obscurities and difficulties, for are we not dealing with Life—the greatest of all mysteries ? There are half truths and whole truths, false theories and true, in abundance, and it needs a cool and wary head to disentangle them from one another.

Once the student has finished his career at the university, and has duly passed his examinations—and, whatever may be said against the examinations, they remain at present indispensable as the only means whereby we can judge whether the student has made a right use of his opportunities or not—it is not to be supposed that the work of study is done, and that he is for all time a workman completely equipped for anything he may have to do. The view we have taken of this matter in New Zealand is that it is the duty of the local graduate to pursue his studies in other countries if possible ; in his own, if that is the only way open to him. More than 50 per cent. of our graduates have so far proceeded to Europe after graduation, and I think we may say the others are only waiting their opportunities to do the same. Of these about 10 per cent. have taken their F.R.C.S. of England, and most of the others have taken the conjoined qualifications of the English colleges of physicians and surgeons.

Whatever may be said in favor of the local study of the elements of the profession, it seems certain that great advantages must accrue to the student who knows by practical experience what is being done in other schools than his own. No one who is conversant with the history of recent medical work can fail to recognise the excellence of the great schools of France, Germany, Italy, and Spain. But to men whose mother-tongue is English there can be only two countries which make a supreme demand upon them, and those

are the great mother-land and the two Americas—Canada and the United States. Notwithstanding much idle talk, Great Britain still stands in the very first rank of those nations where the highest thinking and the best work is being done. If you want the best of anything—books, machinery, clothing, or instruments for scientific or any work—you know that you will get it in Great Britain. If you want the clearest ideas, the best exposition of new and old methods, I believe you will get them nowhere better than in that country we are all pleased to call "Home," and, above all, you get it in the tongue you know best.

A visit to America is of the greatest possible value for post-graduate instruction. The student will see there a great nation full of energy and enterprise. In all the large cities of the States and of Canada universities and medical schools are filled with men who are eager to put into practice the newest and best methods of universal learning. No men are more cosmopolitan in their teaching, and nowhere would the student see more excellent practical work. It is perhaps a fault of their virtues, that they are so eager to try whatever has been discovered in Europe that they have not produced that amount of original work that might have been expected from their numbers and their energy; but it is easy to over-estimate the value of originality. The discoverer of new methods and new principles is born not made, and it seems clear from the history of the last hundred years that the atmosphere of the old world is more favorable to the production of original work than that of the new one. It was said of the Portuguese in the early days of their geographical discoveries that they marked the quarry, and other nations ran it down. America has run down much quarry marked by the older nations. An Englishman discovered the anæsthetic power of nitrous oxide, and said it might be used some day for surgical operations: an American used it. Boerhave spoke of ovariotomy as a possible operation: McDowell of Kentucky did the operation. Laveran, Manson, and Ross showed the nature and method of malarial infection, and Americans made Havana a healthy city. Pasteur and Lister founded antiseptics, and nowhere in the world is the practice more thoroughly carried out than in America.

One of the greatest of modern teachers, Osler, late of Baltimore, now of Oxford, lately speaking of Johns-Hopkins College, said it had been his ambition to make it equal to one of the great German Clinics: and it is true that he has left Baltimore, with the help of his colleagues, in the first rank of teaching institutions of the world. But the saying is characteristic, I think, of the American view. You would not hear such language in Great Britain. There Edinburgh is Edinburgh, Dublin is Dublin, Guy's is Guy's—each with a great history behind it, doing its own work and looking to its own future. In Great Britain, in London, Edinburgh, and Dublin, the student finds himself in the middle of practical work where the ideas which have pervaded the world can be learned at first hand. Nowhere will he see better hospitals or better methods. Nowhere is there more abundance of special work in every department of his art, and the arrangements for the instruction of colonial visitors and the friendly spirit which actuates teachers and students alike in their dealings with visitors is above all praise. Such an addition to the education of the colonial graduate needs no arguments in its favor; and it is only by carrying out such a class of study that we can hope to found a high level of attainments throughout the colonies, and keep it in coming years. Nor are other reasons wanting in such an argument. The student who has passed through such an experience is likely to be a better man, not only professionally, but in every other way—less provincial, more in touch with modern ideas, better acquainted with the great Empire and race of which he is a part. Nor should he neglect to visit the other great European

countries. If he gains nothing else he will at least learn, if he is teachable, to lose some prejudices against foreign peoples and some conceit about his own.

Men from British schools, from America and from Australasia, go in great numbers to the schools of Germany, Austria, and France and meet together, and with the teachers and students of these countries, to the great advantage of all. In France the schools are practically free to everyone. If a man has been foolish enough to think of the French as an idle or frivolous people, twenty-four hours in Paris ought to be enough to destroy that idea. Now, as always, France is foremost in its work in all branches of medicine. It is a country of high ideals, of logical methods, of original and systematic work. The visitor will see in Paris every variety of hospital practice—while the idler has hardly begun his day, teachers and students have already done a day's work. You may visit the Salpetriere, where Charcot taught, and where his successors worthily carry on his traditions; the great hospital and museum of St. Louis, with its wonderful models by Baretta; the Hotel Dieu, and the other great general hospitals; the Pasteur Institute, with the adjacent hospital for infectious cases—a very "holy chapel" of hospitals, as perfect in every detail of construction as art and science can make it. And everywhere the visitor will be received with courtesy and kindness, for there are no more perfect gentlemen to be found anywhere than among the great teachers of the French schools.

I need not speak of what we owe to Germans in all the sciences, but especially in medicine. In the framework of their minds I think they are nearer to us than the French: we think and work together. The best men of both nations deplore all the influences which would separate us, and rejoice in everything that tends to make us understand one another. Medicine is one common ground, and whether in Berlin, Vienna, or Hamburg, the student will find abundance of practical help in all his studies. The man is fortunate to whom the difference of language is no barrier; but even for the man who speaks no language but English there are few difficulties. The Germans are good linguists—they give us cause for shame in this; but it makes study among them come near to be as advantageous to us as study in England or America.

The great International Congresses may not have fulfilled all the hopes of their founders, but they have done something towards breaking down national jealousies, and replacing ignorance by knowledge and mutual respect. I have had the privilege of attending two of these great meetings—the last at Madrid, two years ago. It is easy, of course, to find fault with details, but the man must be incased with self-confidence as with triple brass who does not feel humble in the presence of the famous workers of all countries who take part in these deliberations, and submit the results of their labors for criticism and consideration by their fellows. These international meetings are the outward and visible sign of that federation of the nations in the common bonds of science of which I have already spoken—they are crippled at present in their usefulness by the difference of tongues, but we must hope that our successors will overcome that difficulty by a better knowledge of German and French, or that we may find a common language, such as French, which may take the place of Latin for international intercourse.

There are many other agencies which help to keep up the educational standard of the doctor after he has gone into practice—such as books, journals, and medical associations. Of none of these is it necessary to say much; their usefulness is apparent. It is only to be regretted that so many members of our profession do not avail themselves, as they might do, of the advantage of local societies and such meetings as this for the discussion of their common interests and of their daily work.

We are sometimes told that our medical associations are great trades unions. I am afraid those who use this phrase do not always mean it as a compliment—even when they are unionists themselves. Many of us would be glad if our associations could emulate the virtues of the best of these great unions—the loyalty of their members to each other, and mutual helpfulness. Perhaps they might also learn some lessons from us, and we might then see the spectacle of one of these unions meeting, as we do, to discuss new ways of improving their work ; trying to find out what other nations are doing—European, American, or Asiatic—and selecting the best of these methods for the improvement of their own work.

There are many problems before us in medicine when we have mastered the knowledge that is available from the work of the past. We must still seek after new definitions of disease, and endeavor to understand better its near and remote causes. We must continue to seek for new methods of cure and prevention of disease. But, bearing all these things in mind, I am inclined to think that a great deal of the most useful work of the future must begin with ourselves—with putting our own house in order. If we can first of all teach our students how to live sensible health-bearing lives they will carry the habits into mature life with them. We have left these things to chance, to the uninstructed parent or pupil. We might as well leave any other branch of knowledge to the same influences. Logically, we ought to begin with the schools. We should examine the teeth, the eyes, the lungs, the digestive organs, the muscular systems of the pupils. But we know we cannot do this. We can, however, begin with the medical schools if the students will help us ; and, unless our theories are false, we ought to be able to turn out a body of young men whose physical and mental condition will be the best argument that can be used to persuade other classes of society to carry out the same system.

To go back to the vegetable kingdom for an illustration : we find that the best authorities tell us that for stamping out and avoiding disease among plants we want rather abundance of manure and food than mechanical methods of destroying parasites.

With human beings also disease is most often a manifestation of imperfect nourishment, due to causes often to be traced back for many years —not truly to the mere local parasite, which has found the tissues weakened, and has attacked them successfully.

I have tried, imperfect as I knew the result must be, to lay before you the standard of ethics and of work of the profession of medicine of to-day. I do not think we have reason to be ashamed of either, but individually we must know how far short we fall of carrying out these ideals. "The artist," said Plato, "works for the supreme end of his art, whatever it may be ; the craftsman works for his wage." We try to do honest work, to be help-bearers to the suffering, to shun greed, envy, and malice, and self-conceit, to take large views of life—and we fail often enough ; but because we try, I do think we gain, as few of our fellow men do, the trust and kindly feelings of those among whom we work, and that after all is our highest reward.

THE PRESENT POSITION OF THE RONTGEN RAYS IN MEDICINE AND SURGERY.

By L. Herschel Harris, M.B., Ch.M.

Mr. President and Gentlemen—Skiagraphy has already passed through two epochs of its history since the discovery of the X-ray by Professor Röntgen. The first epoch of discovery and publication was one of unbounded enthusiasm, during which wonderful tales were told of the new force and its posibilities. The expectations of the public and profession were raised to the highest plane, only to be followed by the second epoch of disappointment and condemnation.

During the first epoch, unfamiliarity with the characteristics and actions of the X-ray was the cause of many accidents and disappointing results from its employment. Accidents which occurred during the first two epochs, in the form of severe X-ray burns, dismayed many early operators, and caused a feeling of suspicion as to the expediency of its employment. Idiosyncracy was given a very prominent place, which later investigation has shown to exist only in a moderate degree; burns having been oftener produced from over-long or too frequent exposures in the hands of those who were unfamiliar with its actions.

Surgeons began to doubt its value in localising foreign bodies, as they had often failed to find the object in the spot indicated on the skiagram. Up to this time it had not been appreciated that the relative position of a body, with reference to the anti-cathode and the sensitised plate, often produced a deceptive location of the shadow of the image upon the skiagram. The causes of distortion have since become better understood, and, by cautious methods, localisation is made more accurate.

During the second epoch, the therapeutic uses of the X-ray began to be studied by a few early observers, but received no particular attention or credence till within the last four years or so.

The third, or present, epoch is the one of technical scientific study, and a better appreciation of the limitations of an agent supposed at first to possess more remarkable qualities. There is also, with the further realisation of possibilities, an accomplishment of results which tends to restore confidence and again bring the measure into universal favor: it is the epoch of realisation and substantial recognition of this valuable diagnostic and therapeutic agent.

There are still many questions calling for more careful study of the scientific application of the X-ray as a diagnostic means. At the present time, however, its use in accurately locating foreign bodies, diagnosing calculi, fractures, and various visceral conditions has become too certain to leave a doubt as to its value. What is now required is greater skill in its application, which cannot be expected of the general practitioner or student of medicine, who has not received the proper technical instruction in its different applications.

The subject is a progressive one, and in the near future many improvements will be made in appliances for more definite and precise localisation, rapidity in making exposures, and development of the sensitised plate, and improvements in tubes; as well as other features which will tend to perfect the methods in present use.

It often occurs to me, when I see anyone now starting X-ray work, how very easy everything is made for them. With a 10-inch coil, a good

interrupter, a suitable electric supply, and a choice of any number of first-rate tubes, it is possible for anyone to rapidly master the technique of ordinary radiography. The purely photographic side can be reduced to simplicity itself; whilst, with regard to the treatment of cases, such an enormous amount of information has been published by one and another worker, that it is comparatively easy to acquire sufficient information ; so that at any rate one can use the X-rays without the risk of doing very much harm. This is the view I take of radiographic practice at the present time ; and, thinking of the early days, from 1896 onwards, when there were innumerable difficultics, mistakes, and disappointments, one is inclined to envy the happy individual who is now only just starting the work.

At the same time, there is the other side of the question : and there is the pleasure of thinking of the conquered difficulties, the gradual acquirement of the knowledge—often gained by bitter experience—which enabled one to conquer those difficulties and problems ; the keen delight whieh successful, early, and first results brought ; the intense excitement of the first coin seen stuck in an œsophagus, and easily and safely removed with the eertain knowledge that it was there and to be removed ; the scarcely less pleasure and excitement when a case of rodent cancer was seen to yield to X-ray treatment—when those successful cases could be counted on the fingers of one hand. All those pleasures are lost to, and not to be obtained by, the present day beginner, who has to look for his pleasure in the more or less easy way of following where others have led, and his consolations in looking at his beautiful X-rays, full and rich in detail, and in comparing them with the unfinished, detailless, and indifferent radiograms that some of us were so proud of seven or eight years ago. One of my most valued possessions is the first X-ray tube I used, which I zealously preserve ; and, when looking at it often wonder how it was possible to obtain any result at all with it. Nevertheless it served its purpose, and helped to locate many a needle in the palm, and diagnose fractures in the extremities.

Professor Sylvanus Thompson, in his presidential address in 1897, said :—" Excepting only the introduetion into surgery of Lord Lister's antiseptics and the discovery of anæsthetics, no discovery in the present century has done so much for operative surgery as this of the Röntgen rays." This was a somewhat bold statement made at that time, but I think most surgeons would agree with the opinion thus expressed.

First, let us consider the use of the X-rays in thc diagnosis of injury to bones and joints.

I think I am pretty near the mark when I estimate that 90 per cent. of all bone injuries are now subjected to a radiographic examination. The rcason for this is very obvious when one comes to consider the numerous complaints made by many patients, even though a good result be obtained, and the numerous lawsuits which frequently follow. With many patients now there appears to be an idea, after they have sustained a most serious accident involving one or more fraetures, that if they cannot sue the Government for damages their medical attendant is the next best mark, no matter how good the result, and how little the deformity.

In the Sydney Hospital we make it a rule to have every fracture sereened or skiagraphed. With the indoor cases a copy of each skiagram is attaehed to the patient's case-sheets, and the plate is carefully numbered and kept for reference at any time. Sometimes one case may be skiagraphed four or five times, if necessary. Needless to say, the diagnosis is always made first and confirmed by the rays. A word of warning here may not be out of place. It is very easy to adopt the attitude " Oh, here is a bone injury ; we must

have it X-rayed, and the diagnosis and treatment must depend entirely on the report." The man who would systematically and as a routine follow this rule will, in time, put himself at the disadvantage of not having cultivated and brought to perfection those valuable qualities of careful observation of symptoms, &c., which enabled our pre-Röntgenite forefathers to diagnose and locate fractures with such great skill and success.

I would apply this suggestion not only to the use of X-rays, but would even go further and assert that there seems to be a decided tendency at the present day to trust rather and altogether to modern "instruments" of diagnosis, such as thermometers, microscopes, radiograms, inoculation of animals, exploratory operations, and so on; and so, almost unconsciously, to get into a habit of practice which certainly does not tend towards a training of, and increasing of, a careful and methodical faculty of observation.

Necrosis in bone and malignant disease can both be discovered by the X-rays. I remember well my first case of myeloid sarcoma of the radius, in the case of a lady who had sustained a fracture of this bone a few months previously. The diagnosis first made was "excessive callus pressing on the nerves." An operation followed the skiagraphic examination, and, as far as I know, the patient is still alive and well—nearly seven years ago.

Since the discovery of the rays there has been an apparent increase in the number of cases of congenital dislocation of the hip. Many of these cases were looked upon as acute anterior poliomyelitis, and treated accordingly; especially, too, if the case were bilateral, and consequently showed little or no variation in the length of the limbs. Tubercular lesions of bone are often well seen; though I can safely state that if the lesion is minute, and early, it is generally difficult to make a definite diagnosis.

Before leaving the subject of skiagrams of fractures or injury to bones in particular, and to all skiagrams in general, it is necessary that, if such are to be produced in a court of law as evidence, they must bear some mark of identification. My method is to take, say, one yard of ordinary fuse wire, and work it with the fingers into some much-twisted shape, and, having taken the skiagram with the wire superimposed in one corner, to hand this "brand" to some responsible person for safe keeping. It would be impossible to forge such a "brand"; and, without some such precaution, it is quite impossible to identify to the satisfaction of the court that a skiagram produced is the one taken of the patient in question. I also have small letters and numbers cut out of lead, and place these on the envelope beforehand—R. indicates right, L. left, and so on. The date follows.

It is marvellous to think of the strides made by radiography in connection with stone work. In 1896 Henry Morris wrote in the *Lancet*:—"Owing to the position of the kidneys, close to the vertebral column, the depth of the cavity, and the thickness of the overlaying tissues, it has so far proved impracticable to obtain a radiograph of renal calculi." In June, 1898, Mackenzie Davidson, in a short note, asserted that he had been able to see a stone *in situ* on the screen. This is probably the first record of this having been done, though, not long afterwards, several of us here will recollect our first cases when renal calculi were detected.

Strange to say one of my first successful cases was one in which I discovered a small ureteric calculus. I published the skiagram in 1900, which, so far as I know, was the first one published, showing a ureteric stone; but since then I have only had one other case in which the calculus has been in the ureter. I still maintain that about 4 per cent. is a fair average to allot to cases of ureteric calculi, and the remainder are invariably in the renal pelvis. Leonard, of Philadelphia, maintains that 50 per cent. are ureteric. Perhaps

no man in the world has done greater or better work in connection with renal calculi than Dr. Lester Leonard. He still adheres to the method he has used now for some years, viz., a long exposure with a low tube, using a high amperage, and he bases his negative diagnosis on the axiom "that in a negative possessing a differentiation in the shadow of tissues less dense than the least dense culculus, no calculus can escape detection."

In an interesting letter written to me by Dr. Leonard at the end of 1902, he states: "I do not claim to make the absolute negative diagnosis in all cases, only where the definition and contrast in the negative warrant it . . I, however, always tell very stout patients that they are difficult subjects, and that I may not be able to render an absolute negative diagnosis or to find a calculus."

About the same time I heard from Mr. Henry Morris, who, in the course of his letter, says: "In my opinion the Röntgen ray is a scientific toy in renal cases, and that our practice of surgery ought to be based upon other factors in every case," &c., &c. I think by now, probably, that he will admit that the X-rays are of the greatest service to renal surgery, for a negative diagnosis does not necessarily indicate that the patient should not be operated upon; it is still, I maintain, of the same value as a negative result in the case of examining for tubercle bacilli in the case of a suspected phthisical patient.

Then, again, I can quote five cases in my own experience where calculi have been located on one side, whilst all the symptoms were on the opposite side. Before the days of the X-rays we can all remember cases being operated upon for renal calculi and found—how often? Now it is the routine practice for any suspected case to be submitted to an experienced radiographer for an opinion; and rightly so, too.

The medical men who send their cases should themselves most carefully examine the resulting plates, and so gradually train themselves to interpret the shadows. By this means they will be in a position to give an opinion as good as the operator; and, indeed, this applies to skiagrams generally.

So far as my own experience goes, I have detected calculi in seventy cases, and they have been removed. I have made, so far as I know, twelve mistakes, and examined about 400 cases.

To Dr. Albus-Schonberg is due the credit of introducing a pressure tube apparatus through which the exposure is made for kidney cases especially. There are two advantages obtained by it. Firstly, by cutting off all but a small direct stream of rays, and by cutting off secondary rays, a much sharper picture is obtained; secondly, by the pressure of the tube, the respiratory movements are modified on the side being examined. This method I have tried, and can recommend it in other cases too, where a small area only is required—as in joint cases. Of course when the compression tube is used, only a small area can be exposed at a time, and if the result is negative, then subsequent exposures on different plates must be made. This is the great drawback to this method. I think, perhaps, it is better to expose as usual on a large plate, and to use the compression tube subsequently, if necessary. The exposure, too, is very different now to what it was. Whereas a good result in a suitable case can be obtained in fifteen seconds or so, a couple of years ago we would have given half an hour. The tubes have improved, and also the method of producing the rays; and, in this connection, I always feel deeply indebted to Dr. MacCallum, of Geelong, for having induced me to use a "Caldwell" interrupter. Even now at times I still use an accelerating screw. Judiciously used with a low tube, it is of great assistance. The heavy anodal tubes for the strong currents are very admirably made, and recently a water-cooled anodal tube has been placed on the market, which has many advantages.

The majority of operators, when examining for stones in suspected cases, make the patient lie flat on his back with knees drawn up and head and shoulders slightly elevated. Others simply let them lie flat on their back.

For versical calculi the rays are of not nearly so much assistance as in the former cases, for these can usually be easily detected by other means. As for biliary calculi, one or two men claim to be able to detect them. I have my doubts, and should very much like to see the original plates.

Turning now to the medical aspect, it may be as well to recall to your memories a statement made by Dr. Mansell Moullin in his presidential address, in 1899, when he said, "I am convinced that, at no very distant date, the examination of a patient's chest with X-rays will be considered as much a matter of routine, and as little to be neglected in all doubtful cases, as an examination with the stethoscope at the present time." And he enumerated a long list of cases in which their value from the diagnostic point of view was very great; and this list can scarcely be added to at the present time. But, owing to the improvement in apparatus, and to the better recognition of what can be seen with the screen, and by the study of good plates, and to the more exact knowledge of the meaning of the differences from normal that can be seen and radiographed, his remarks as to the value of this method of diagnosis now carry more weight.

The fluorescent screen examination has proved of the greatest service in detecting aneurism in the thorax, and other mediastinal tumors. Pulmonary hydatids have been seen, and, in several cases, a diagnosis has been correctly made of a hepatic abscess having ruptured through the diaphragm into the lung.

It is generally admitted that a diagnosis of pulmonary consumption can sometimes be made by the screen before any physical signs are evident. Besides the dull areas seen, the most important point is the diminished excursion of the diaphragm in the affected side.

So far as the treatment of tuberculosis is concerned by means of the Röntgen rays, it is now a matter of much controversy. In this connection I feel I cannot do better than quote some extracts from the presidential address of Dr. Bullitt, delivered at the Congress of the American Röntgen Ray Society at the end of last year. My results agree entirely with his, in spite of most glowing accounts being published every now and again by some new man.

In tuberculosis of lymphatic glands the term "cured" can hardly be understood to mean the entire disappearance of the affected and enlarged glands. It rather means a diminution of the size of the glands, and a disappearance of all symptoms which indicate any activity of the disease process. In the comparatively few detailed reports which have been made of cases of this kind the statement has been made in all, that while the condition had improved and the glands had diminished much in size, they still were present as palpable enlargements.

Some men have published most startling accounts of the results of treating phthisis pulmonalis by means of the rays. In all the cases mentioned the usual hygienic treatment, rest, diet, &c., have been included, and no doubt they did well in spite of the rays.

To sum up the treatment of tuberculosis generally, I agree with the statement that, "while the reports are encouraging, and while the probability seems to be that this method of treatment may be shown to be of great value, the statistics obtainable are too uncertain and not sufficiently explicit to warrant such an assumption." And let us not forget that our effort and duty must not be to prove that the Röntgen method is of value in the treatment of tuberculosis, but that it is to ascertain the truth as to whether or not the method is of value.

Passing on to diseases of the appendages of the skin, such as hypertrichosis, alopecia-areata, tinea tonsurans and Favus, sycosis, acne vulgaris and comedo, and inflammatory diseases of the skin such as eczema, psoriasis, lichen planus, lupus vulgaris, and erythematosus, prurigo, &c., we all know what good results have followed suitable X-ray treatment in these cases.

I must make special mention of the treatment of Keloid and hypertrophied scar tissue by the X-rays. Since I had the privilege of reporting the first case treated I have had many such cases, and if treatment be persevered with long enough, the cure is practically certain. Several severe reactions must be produced in every case.

Perhaps the most important consideration of the rays is in connection with the treatment of malignant disease. Further experience in the application of the X-rays to the treatment of rodent cancer, and other forms of superficial cancer, has tended, on the one hand, to clear up some points which were previously obscure; and, on the other hand, to raise fresh issues, upon which we still require enlightenment, and of which we have much to learn. There can now exist no doubt as to the beneficial effects to be obtained from scientific application of the X-ray in many superficial lesions, but we still have much to learn as regards the best methods of application and the proper dose to administer.

In many cases the X-rays act almost as a specific; but, like all "specifics." they occasionally fail to produce the desired result. Those few exceptions may, however, be taken as those that go to prove the rule.

An important question is that of recurrence in cases which, to all intents and purposes, have been cured, and this frequently after only a few months' freedom from distressing symptoms.

Mr. Malcolm Morris, in a paper read before the Harveian Society, has pointed out that the general principle of the up-to-date treatment of superficial skin lesions may be expressed in the single word "reaction." "Reaction." says Mr. Morris, "is a force which can work wonders if properly directed and controlled. In the present state of our knowledge we are but imperfectly able to control its operation. The clinical phenomena seem to me to warrant the belief that some part at least of the beneficial effects of the X-rays is due to the reaction which they excite." I think that most of us now hold this view: in fact, we generally, in treatment of our cases, produce a dermatitis of a greater or less degree, and thereby get some idea of proper dosage.

Dr. Hall-Edwards, in an admirable paper on this subject, in the *Journal of Medical Electrology and Radiology*, speaks of the advantages of a "soft" tube. I think I am safe in saying that these are not generally employed now in conjunction with a mechanical mercury break. With an electrolytic interrupter tubes become hard so quickly that the expense of procuring fresh ones would be endless. If "soft" tubes are to be used, so as to obtain their full effects, they must—even under the most perfect conditions—be constantly replaced by new ones, as no tube which has commenced to get "hard" can be again brought into its original condition unless it be filled with air and be then re-exhausted. A common plan often adopted—and I myself once was a culprit—is to keep the old tubes for therapeutic work. The results thus obtained will not bear comparison with those in which new tubes are employed. The vacuum of a tube can certainly be lowered by means of certain subsidiary tubes, which give off vapor or gas when the spark is made to pass through them. In these self-regulating tubes the vacuum may be lowered even past the point of "softness," yet they never produce again the results which were at first to be obtained from them. This, I think, is explained by

Sir Oliver Lodge, who has pointed out that, in addition to X and other rays which come from an excited tube, a certain number of cathode rays pass through the glass, and these carry with them ions knocked from the atoms contained in the vessel. No matter how many atoms are admitted to replace the lost ions, the contents of the vessel will ever afterwards consist of a mixture of (what we may term) damaged and complete atoms, and the conditions which at first existed cannot be reproduced unless it be filled and re-exhausted. Some manufacturers assert that the same conditions can always be repeated by measuring the amount of current taken by the tube. This must be incorrect, for as a tube is ever and always changing from a " soft " to a " hard " condition, it must follow that one state cannot be maintained by any regulation of the amount or tension of the electrical current passed through it. Could we by any means maintain a given condition for even a limited time, we should be in a position to administer measured doses with at any rate some degree of certainty. There is no doubt that the measurement of the amount of current passed through the tube is of importance, and gives a much better idea of the condition of things than did the old method of measuring the amount of current taken through the primary of the coil.

To Dr. Deane Butcher we all owe a debt of gratitude for having introduced for discussion before the Röntgen Society of England a paper dealing with the means of accurate measurement in X-ray work. He pointed out that in X-ray work it is useful to distinguish three distinct individuals—the maker of the focus tube, the driver of the tube, and the user of the tube. Of the maker of the focus tube we may fairly ask that he shall give us —(*a*) A standard tube of definite size and make, with a certain definite resistance, measured in ohms : (*b*) Means of maintaining this resistance constant : (*c*) Means of measuring the resistance and varying it at will. Of the driver of the tube we require that he may be able to maintain—(*a*) A constant current through the tube : (*b*) A constant vacuum in the tube.

Lastly, the user of the tube requires only two instruments of measurement : one to assure himself of the quality of the rays, and another to measure their quantity.

So far there are several methods in use. Holznecht uses a capsule of gelatine, impregnated with a salt which turns to a deeper shade of green under the influence of the rays. This color is measured by comparison with a painted scale of twelve different tints. Holznecht's chromoradiometer is very expensive, and difficult to procure, and from time to time he alters the composition of the capsules.

Freund uses a 2 per cent. solution of iodoform in chloroform, which darkens under the influence of the rays.

Sabourand uses a portion of an ordinary fluorescent screen, with the same object.

Baroist has supplied a radio-chromometer to determine the penetration of the rays. This instrument is based on the fact that the relative transparency of silver and aluminium to the rays varies with the quality of the rays considered.

Dr. Leonard, in his work, employs a milliamperemeter, which can be placed in circuit with the tube. The milliamperemeter measures the volume of electric energy passing through the tube, and hence the proportionate volume of Röntgen ray energy given out. The increased volume which is shown to pass through low tubes, he states, corresponds with their greater photochemic activity, and also with their effect in therapeutic work.

Leonard employs the following formula in expressing his standard dose in the treatment of malignant disease :—10m, 8″, 2′g, 2ma ; meaning ten minutes treatment, with the platinum 8 inches from the skin, the tube

having an equivalent air resistance of 2 inches, and energised by a current of two milliamperes. Like any other algebraic formula, an alteration in any of its terms alters its value.

This attempt of standardisation is a step in the right direction; but most of these methods have been threshed out by French physicists, and it was decided that they were unreliable, because the inconstancy of the tube defied all efforts of measurement. Mr. Dean, of tube fame, attributes one cause of this to the fact of not employing pure platinum in most of the tubes on the market. There is certainly something in this; and I think that if more care were taken in the manufacture of tubes our results would be much better than they are.

Some enthusiasts have gone so far as to measure the glass broken from different parts of the globe of a tube, and found great variation in the thickness. Let us hope that, as time goes on, and competition becomes keener amongst the manufacturers, our tubes will be considerably improved.

In Australia our best results have been in cases of rodent cancer. This disease with us is very common, especially with the working classes. In America and on the Continent it appears that no distinction is made between rodent cancer and epithelioma, and nearly all the cases which we call rodent cancer here they call epithelioma: hence the cure of this disease is spoken of by our confreres as a simple process. Dr. Grubbe, when addressing the members of the Röntgen Ray Congress last year, defied the profession in Chicago to produce ten cases of rodent cancer there.

In the treatment of these cases of rodent cancer there is great variation, though the ultimate object is reached by many eventually. I generally follow the following line of treatment:—As a preliminary, a thorough curetting of the affected tissue with subsequent antiseptic dressing. Two days afterwards begin the X-ray applications, with a soft tube, at 8 inches distance to start with. Applications are daily for ten minutes. During the treatment I constantly paint the part with pure carbolic acid: this serves as an antiseptic. and closes up the lymph channels, as well as assisting to hasten the "reaction." I never employ therapeutic tubes specially made, nor do I use special tube-shields: these have been found to interfere with the proper passage of the current through the X-ray tubes, which rapidly deteriorate and become useless. I use any good make of tube, and protect the patient with several layers of tinfoil. They are instructed to spread some mild antiseptic ointment on china silk, and cover over the affected area. When a good reaction is produced the sittings are discontinued for the time being, and, if not cured, a second reaction is produced, and so on. Great care should be taken to include an area around the affected region, to be on the safe side. To show how necessary this reaction is, I may mention that two of my cases some few years ago developed cellulitis during treatment, and when they recovered the rodents were also cured. Before discharging a case, the part should be perfectly smooth. Lately I have been applying a solution of fluorescin to the part during the application of the rays, hoping thereby to shorten treatment, as well as intensify the rays.

My results, on the whole, have been very satisfactory, showing only a very small percentage of recurrences within the past six years. I do not think statistics should be published till such cases have been treated for ten years, and even then they are misleading.

One statement I wish to make, which I have not seen mentioned before. When any bony sturcture is attacked by rodent cancer, an operation should be insisted upon. I have seen such cases heal over, apparently cured, and later on break down again. This will happen every time. The probable explanation for this is that the curative rays are the soft ones, and the bone

salts prevent them from acting on dense structures. So, no matter how little bone be affected, always insist upon surgical interference first, and subsequent ray treatment. I make it a rule of having every case examined microscopically, and often, to my surprise, I find that I am treating a case of superficial epithelioma. These respond just as well to ray treatment as do the rodent cancers, and operators all the world over agree on this point.

Glandular involvement should be carefully sought for, and, if present, nothing less than a thorough operation should be performed.

The treatment of sarcomata by the rays has not been so satisfactory, and though several successful cases have been reported, there is some doubt as to the diagnosis in these cases being correct, and also too little time has elapsed to speak with any authority on the success of such cases. My own results have been disappointing.

Passing on to malignant disease of the breast, one of the commonest malignant affections amongst women, I think we can justly and proudly claim that the X-rays have done very much good in the treatment of many such cases. Hundreds of such cases have been treated in various parts of the world, and the results throughout are good, on the whole.

There is some difficulty in considering seriously the numerous published results, for, as Dr. Bagg, of Pittsburg, says, " the Röntgen-ray workers may be classed under three heads : 1. Those who have made a careful study of electricity and physics, and are able to duplicate results, either in diagnosis and treatment. 2. Those whose work is chiefly confined to some other specialty, and who use the Röntgen rays without making any claim of having paid any particular attention to the subject. 3. Those whose work is done by head nurse, or some other inexperienced party."

All conscientious radiaographers agree that no malignant case should be treated by the rays without previous consultation with and sanction of an experienced surgeon. I am afraid, however, that this rule is not always carried out—and, to be lenient, let us say owing to ignorance on the part of the radiographer. I do not consider that any malignant case should be treated by the rays unless pronounced to be inoperable, or as an adjunct to a surgical operation. Pre-operative applications of the rays are not justifiable.

In these days of asepsis, I think we should insist on every breast tumor being subjected to an exploratory operation, at least. Why, Dr. Halstead, of the Johns-Hopkins Hospital, in a systematic cutting of the entire tumor in twenty-eight cases of apparently benign breast tumors, found foci of malignant disease in them all ! A careful study of the results obtained by Röntgen treatment in malignant disease of the breast shows that it has been most effective where it supplements, rather than supplants, operation—that is, where the focus of disease has been removed.

The Röntgen treatment has the advantage of being conservative and reconstructive, rather than destructive. It possesses prophylactic and palliative properties that make its first operative application of the utmost importance. It helps to reconstruct normal tissue, and hastens the healing of wounds. It can be applied immediately after opertaion in all cases without doing harm, and with the assurance that much benefit will be derived. It is the most potent palliative and prophylactive agent known in the therapeutics of this disease. In inoperable and hopeless cases, seen in the late stages of the disease, it affords relief from intolerable symptoms, lengthens life, and is the greatest chance that offers to the patient. That this is often very great is shown by many remarkable cases : patients given up as inoperable and hopeless have been restored to comparative health, comfort, and activity. In such cases local and constitutional treatment upon sound medical and surgical principles

is absolutely essential to success. Often in these cases a toxæmia, so common in malignant disease, occurs, and manifests itself in rheumatic pains, billiousness, and gastric attacks, with a general increase of nervous symptoms. In some instances, in the early stages, it can be combated by vapor baths, provided the vitality of the patient will permit. Tonics and sedatives are of value. Quinine is of special value, for its tonic and antitoxic properties. Some operators give quinine for its fluorescent properties as well, and it seems only feasible to think that fluorescent solutions absorbed by diseased tissues should cause them to re-act quicker and better. A good plan is to prescribe the patient a pill of quinine, iron, arsenic, and strychnine, a careful diet, and to drink large quantities of liquids.

Other forms of malignant disease have not given such satisfactory results as these just mentioned. Practically every form of malignant disease has been subjected to the rays, but the results with the deep-seated ones have been doubtful. In most cases, at any rate for a time, a stimulating or tonic effect follows the treatment. This is accounted for by Dr. Snow as being due to the disposition of the vibratory influences of the rays to first overcome local stasis, restoring tone to the muscular coats of the arterioles, and at the same time inducing a more active local metabolism.

I think, gentlemen, this briefly describes the principal uses of the X-rays since their discovery ten years ago, and their progress—though hampered by a few unscrupulous workers—has been slow and sure. What the next decade will produce it is hard to predict, for just now even an altogether new subject, that of azoo spermia, is engaging the attention of scientists.

All these considerations make us ask the question—In whose hands should the Röntgen rays be ? And I think unhesitatingly you will answer, in those of qualified medical men.

Let me close by quoting Dr. Thurston Holland in his presidential address before the Röntgen Society of England last year. He says : "Obviously it is possible for anyone to acquire the necessary knowledge to show, say, a fracture, or coin, or foreign body in the body ; but even in these simple cases, if the application is to be made of the fullest use and mistakes are to avoided, a medical man skilled in the use of the rays must direct the examination : I would even go further than saying that a qualified medical man only must use the rays for medical and surgical purposes, and would say that he must be at the same time an expert in radiography. Even in the hands of medical men a caution is necessary. Large numbers, now, so to speak, 'dabble' with X-rays. They order a good apparatus, and do not trouble to get proficient at the work ; as a consequence, their X-ray work is most inefficient, and when it comes to X-raying hips, giving an opinion on a chest case, or attempting the diagonsis of kidney stone, the results they show are quite worthless, and tend to bring X-ray work generally into disrepute amongst those physicians and surgeons who depend on others for this work. However, the use of X-rays for cases of this kind may be almost said to be unimportant when compared with their use for treatment ; and yet, on all hands, we see unqualified persons using powerful apparatus in this manner. And yet who is to be blamed for all this ? Primarily, medical men themselves, who have from the first encouraged instrument-makers, chemists, and generally non-qualified individuals by sending cases to them. The position of X-ray work, in my opinion, is this :—Any practitioner can set up an X-ray apparatus, and use it for his own special work—a surgeon for foreign bodies, fractures, and so on ; a physician, for chest cases, &c. ; a dermatologist, for the treatment of his cases. But there remains a field for the X-ray expert, who is ready and competent, to use the X-rays properly for any and every class of case to which they can be applied : and this man ought necessarily to be a qualified medical man."

VICTORIAN SANATORIA FOR CONSUMPTIVES.—NOTES ON FIVE YEARS' RECORDS.

By R. Bindon Stoney, M.B., B.Ch.

Having at my disposal the records for several years, I thought I might be permitted a few minutes to summarise these case-books and see if any lesson could be derived from them. Our sanatoria consist of a summer establishment in the mountains of Macedon, where a few carefully picked cases stop on during the winter, and a winter sanatorium in the dry northern district of Victoria, right on the Murray at Echuca. It is of this latter I am medical officer, and have charge of forty-five to fifty patients from May to November in each year.

As to climate, Echuca is usually dry, and affords a maximum of clear sunshine. The nights are clear, and slightly frosty in the middle of winter; we are thus able to give the patients the greatest amount of light and fresh air with a climate that enables them to be out of doors as much as possible.

The rules of the institution are that only cases in the incipent and early stages are admitted, but, unfortunately, we get a good many cases sent to us from Melbourne that are anything but in the early stages, and most of these have to be sent home disappointed after a short residence with us.

A certain number of cases of miners' lung come to us, but are all practically hopeless cases. Another bad class are the undeveloped, rickety, strumous youngsters, who certainly improve with fresh air and good food *up to a certain point,* but never do us credit.

The history of a large number of cases that come under our notice is :—1st. A course of patent medicines prescribed by himself, his chemist, or his friends, that does no good. 2nd. A course of disinfectants, under the family physician, of creosote, guiacol, &c., trying to poison the bacteria in the lungs, but, instead, destroying all the natural ferments and vitality of his digestive organs ; or he is saturated with cod oil and such like till his liver refuses to act properly. 3rd. He gets a course of tuberculine lymph, electricity, massage, or other so-called specific under the specialist, to the material benefit of the specialist.

It is difficult to say which of these courses is most harmful, as they give the patient the idea that he is being cured, yet daily his hope of recovery is getting less and less : finally, when he is thrown up by the chemist, family physician, and specialist, he is thrown on to the sanatorium as a last resort, or he goes to the incurable hospital, or dies at home.

All our experience is that it is exceedingly important that cases should be recognised in the incipient stages, and that no time must be lost. Time should not be wasted in purely medicinal treatment, or he soon reaches that stage when recovery is impossible. Of all the factors in the treatment of consumption fresh air and sunshine stand first, good nutritious diet and rest second, reserving medicine for complications.

During the five years under consideration, 1900-1904, 552 patients were admitted to the sanatoria. Now the question of real value to us is, what percentage of those *are cured to that extent* that, after several years, they are able to carry on their work and earn their own living *without difficulty?* How many are barely able to so do, and what number are totally invalided or dead? And, if possible, to see the condition of those who have done best, and what treatment has been carried out in their cases.

With regard to death-rate. Of the 552 cases treated up to end of last year 247 are known to be dead, being 44½ per cent. of total. Or, taking them year by year :—

1900......	66 cases—	39 dead,	59 per cent.
1901......112	"	54 "	48 "
1902......105	"	44 "	42 "
1903......118	"	59 "	41 "
1904......151	"	51 "	33 "

Showing that, after the first two years, the balance shows a much slower increase of death-rate. Of those alive 46 per cent. are well and able to do their work, 16 per cent. with difficulty struggle along, 26 per cent. are invalided, and of the balance, 12 per cent., we have no recent information. That is to say, of all the patients who have gone through the sanatoria in those five years, not quite 21 per cent. are now well enough to be able, without difficulty, to follow their daily occupation.

The next question is as to the conditions under which patients have done best: and great prominence has been put on the question of the patient putting on weight. A recent work states that a patient who increased a stone in weight was fairly safe. But what do we find ? Take the year 1901 (the oldest we have weight records for), during which 112 patients were admitted to our sanatoria (fifty-six to each) ; of these sixteen put on a stone and over in weight during their residence with us, yet after five years what do we find ? That 50 per cent. of these are, *to our knowledge*, dead, while of the whole 112 we know of only 48 per cent. of deaths. So that those who have gained over a stone in weight show a larger percentage of deaths than the rest of the patients; in more recent years, though numbers are not quite up to foregoing, yet the percentage of deaths among the stone and over is more than those who have put on from 5 to 14 pounds ; and, on the other hand, the patients who lose weight, or do not put on at least 5 pounds, show during the four years the very large mortality of 62 per cent.—the totals being :—

One stone and over	85 cases—	23 deaths,	27 per cent.
Five to 14 pounds gained....	149 "	35 "	23 "
Nothing to 5 pounds gained..	131 "	77 "	60 "
Lost weight................	105 "	70 "	67 "
Weights not recorded 1900 ..	79 "	42 "	—
Total..................	552 "	247 "	44½ "

This is still more accentuated when we go through the cases individually, and we find those who have been best fitted for the struggle of existence are those who have put on 7 to 10 pounds, not those who have been fattened up like prize oxen for the slaughteryard.

It sometimes happens that those who are sent away home as hopeless cases nevertheless pick up strength, return to work, and are comparatively well ; while, on the other hand, cases that appeared to have done remarkably well go to pieces as soon as they get out in the world again. For the latter the reasons very largely are, as far as I can see, that the patient has had such an easy lazy time in the sanatoria that his muscles are quite unfit for work. He gets among companions and drinks perhaps a little more than he ought, or, even where he takes every care of himself, the worries of this world and the fact of being tied down to his work, of having to do his daily round whether he likes or no, whether he feels fit or no, of weather and climate, and such like, which he has to put up with when he goes back to work, and a large number break down before they have been at home long.

Weight lists are liable to a great variation, and it is a mistake taking a week's weight list by itself, neglecting those previous—taking each patient's gain or loss for the week. The aggregate weight increase will be most materially altered by one or two bad cases, or if one or more are in bed and not weighed, or if those who may be dismissed just before weighing day are losing or gaining fast. Another thing is that a sudden change in the weather will cause a general alteration in clothing, which, though slight in each case, makes a considerable difference in the totals. Another trouble is that patients are weighed once a week on a particular day after dinner, and if one week's dinner is not quite so tasty, or another a little more so, it may affect the weekly list. Hence, I prefer as long an aggregate as I can get—or, as I have done above, to take each patient's weight at arrival or departure—if we want a true idea of how things are going.

As to heredity. In 52½ per cent. we get a family history; this to the patient means probably a congenital delicacy with increased opportunity for infection. In many cases we have two or three out of one family. As far as we can see, they do not do as well as other cases; but it is impossible to separate the figures, as the family history does not necessarily imply heredity, as infection may have been acquired quite independently.

Temperature is one of the most important indications, and I feel inclined to classify them as:—1st. Very high and irregular temperatures, often showing an alteration of two or three days' rise and fall; look out for dysentery, tubercular enteritis, or other acute complication (hopeless). 2nd. More regular than the last, may show a tendency to a three or four day periodicity running up to perhaps 103, generally with streptococcal complication (very hopeless). 3rd. A two or three day rise, say to 101.5, no mixed infection (hopeful). The temperature in complications runs much higher often than symptoms seem to warrant; a bilious attack, a slight cold, may run it up two or three degrees.

Hœmorrhage, when slight, has but little bad effect on a case if we do not knock the patient up with starvation and bed. Severe hœmorrhages are very rare with us.

The mental aspect should always be carefully considered—plenty of amusement to interest them; and we must remember that if we give a patient no bottle medicine he goes away and says he was neglected. He feels so, and it does him harm. Give him something to treat his mind.

Mild and convalescent male cases, who are smokers, are, I think, all the better for their pipe in strict moderation, if it does not make them cough.

What then are the cases that have done best? They are cases where one, or at most two, lobes are affected; where there has neither been any very rapid increase of weight or the reverse—where the weight has slowly increased to the full weight of the individual and stopped there; when the patient is naturally of an energetic and hopeful disposition, and has been both hopeful all through and energetic as far as allowed as soon as any temperature he may have had was gone. He does not worry about the past, present, or future, but cheerfully takes his part in any work or amusement that is going on. These are the cases that, before they go out, harden their muscles by fresh air, cold water, moderate regular exercise, and good food, so that when the work of this world has to be undertaken they are ready and willing to take their share in the battle of life. The grumbler, the loafer, the flabby fattened up case, the case who has had so much rest cure that he never gets fit to work again—they go out, and soon the last case of those men is worse than the first.

I ask for the digestion that it be not ruined with antiseptics and irritants, that the liver be not over-loaded with oils and fats, that the muscles be not

allowed to degenerate for the want of exercise, that the mind be not allowed to grow either apathetic, morbid, or introspective, for the want of rational interest and amusement. I ask that, while thinking of the tubercle bacilli, you forget not the diplococci, streptococci, the germs of rheumatism, influenza, etc., for it is on these other organisms that the prognosis and prominent symptoms of consumption are largely due.

A little less theory, a little more common sense; and remember always that when a patient leaves the sanatorium he has to take his part in the work-a-day world, or go under.

THE PRESENT POSITION OF THE SANATORIUM TREATMENT IN AUSTRALASIA.

By Arthur H. Gault, M.D., Lond.

The sanatorium treatment of consumption as far as Australia is concerned may still be said to be on its trial. Each of the larger States has a public sanatorium, but the palatial buildings of the old countries find no counterpart here: we have only small institutions—indeed, for the most part, colonies of cheap and temporary structures. For the working man a sanatorium is acknowledged to be the best place, but for the private patient the Australian practitioner is not yet convinced of its necessity. The general advice to the early consumptive is, to have as much fresh air as possible—to go for a change or sea trip, to reside in the north, or in a tent, and when these have failed try the sanatorium. It is hardly fair to judge of the value of this treatment under such conditions. I think I am correct in saying that the patient has been suffering from his disease for at least a year before, as a last resource, he comes under treatment in a sanatorium. The poor man works as long as he is able; the rich tries everything else before he yields himself to this treatment. The sanatorium enthusiast believes that he is adopting one of the most valuable therapeutic advances of the last century, and one destined to play an increasingly important place in the treatment of the disease, and he looks to it for the solution of the tuberculosis problem. It may seem strange that the death-rate from tuberculosis has not fallen in these States as it has in other parts of the world, the reason obviously is that the influence of this form of treatment has not yet asserted itself. I venture to predict that in another decade, when it has had a chance, the tubercular death-rate will decline here as it has done elsewhere. It is the duty of every medical man to make himself personally acquainted with the methods and results obtained, and thus be in a position to tender advice.

No doubt there is a strong prejudice on the part of the patients themselves; they think they could not stand the severity of the treatment—the exposure to draughts and cold, the over-feeding, the rigorous discipline. They say it would make them worse to be associated with so many suffering from the same disease as themselves; that life would be so monotonous away from home comforts and friends. All such fears are dispelled by actual experience. Everything is planned for the comfort and happiness of the patients, as well as their good. Patients are never allowed to feel cold—they take the food they like—there are no irksome restrictions. The presence of others suffering from the same disease, but so much better than themselves, acts as a continuous inducement to persevere. The quiet, non-exciting, yet, at the same time bright and cheerful, life of a well-conducted sanatorium affords conditions most favorable for recovery.

There is no doubt that a climate like Australia makes it easier to live an open air life, and therefore do without a sanatorium. The winter is the time when the advantages of a sanatorium are the most obvious. We would consider Samoa a most unsuitable climate, and yet it was just the one that suited R. L. Stevenson, because it was one in which it was impossible to live anything else but an open-air life: and, no doubt, the improvement that takes place on going for a change is due to the fact that the patient is so much out of doors. Our climate ought to give us a great advantage in the after treatment of this disease.

Briefly let me describe the Australian sanatoriums.

I. PUBLIC SANATORIUMS.—New South Wales has adopted an excellent idea—men and women are to be treated in different institutions. Thirlmere is being transformed into a modern sanatorium for women, the original substantial three-storied building will have to be modified, the large wards for twelve patients divided, and something done to get over the disadvantage of stairs. Thirlmere accommodates about forty patients, and is under the care of Dr. McCredie.

Kings' Tableland Sanatorium, Wentworth Falls, is under the care of a resident, Dr. McIntyre Sinclair, and provides accommodation for forty-four male patients. The buildings consist of pavilion blocks built of wood connected by corridors, some single shelters, and some for five patients. At an elevation of 2,700 feet one would think the climate rather severe in winter; but as most of the cases are early ones, it does not seem to interfere with results. Dr. Sinclair is to be congratulated upon the large percentage of recoveries—equal to anything obtained elsewhere and testifying to the value of a resident physician.

Victoria in its provision for consumptives is also unique in having a winter and a summer residence. There is a good deal to be said for this arrangement, but it is unnecessary if a moderate climate be chosen. The extra expense could only be borne by a rich State like Victoria.

The winter residence at Echuca and the summer one at Mount Macedon have each accomodation for about fifty patients. The buildings are of a temporary character—wooden shelters supplemented by tents; but they seem to serve the purpose, and are quite insufficient for the number of patients applying. Echuca is under the care of Dr. Stoney, and is looking out for a larger site. Mount Macedon labors under the disadvantage of having no medical man in constant attention.

The new Government Sanatorium at Greenvale has only been opened a few months. Seven tents have been erected to hold five patients each. More permanent wooden buildings are in course of construction.

Queensland is well supplied by the Jubilee Sanatorium at Dalby under the care of Dr. Stewart. The building is a simple one more like a large verandah, but the results seem equal to any. Dalby provides accommodation for forty patients. Dr. Stewart has recently investigated the after history of patients discharged as cured. Most have gone back to their original occupations, and, in spite of the unsuitableness in many cases, have maintained perfect health.

The New Zealand Government has recently opened an important sanatorium at Cambridge, with accommodation for fifty-eight patients, under the care of a resident, Dr. Pentreath. This sanatorium is splendidly situated, and has a large area of land (1,000 acres) in connection with it. The buildings are of wood for six, two, or one patient, chiefly single rooms. There is a separate colony of shelters for paying patients. We have no doubt the advantage of an experienced resident doctor will make itself evident in the excellence of the results.

There is also a small sanatorium at New Brighton, the Nurse Maude Camp, providing accommodation for twenty patients in separate shelters and tents. In addition to this, the general hospitals at Wellington, New Plymouth, Blenheim, Invercargill, and Waimate have annexes for consumptives, from which suitable cases can be drafted to a sanatorium. Now that hospitals do not admit consumptives, this is an arrangement which ought to be adopted at all such institutions.

South Australia has the Kalyra Sanatorium, Belair, with fifty-three beds, erected and largely maintained by the estate of the late James Brown. The buildings are all of stone of a substantial character. The three new wings are built on Nordrach lines with single rooms.

Western Australia has no sanatorium, but there is a small annex to the Perth Hospital.

Tasmania: A similar arrangement is in contemplation for the Hobart Hospital.

There is great need in all the States for colonies to provide suitable occupation for those in whom the disease is arrested.

II. Private Sanatoriums.—*New Zealand*—Avon Pine Sanatorium, Christchurch, under the care of Dr. Greenwood; for eighteen patients. It consists of isolated wooden shelters and tents, arranged in a colony. Flagstaff Sanatorium, Dunedin, under care of Dr. Stephenson; with accommodation for fourteen patients. The building is of wood, with central administrative block connected by covered-in corridors with the wings, which are composed of single rooms.

Queensland—None.

New South Wales—Leura, Wentworth Falls, a small home, under the care of Mrs. Robison.

Victoria—Belle Vue Sanatorium, Yackandandah, under the care of Dr. Johnson; for twelve patients. Buildings of wood, isolated shelters for each patient.

South Australia—Nunyara Sanatorium, Belair; with accommodation for thirteen patients. The building is of stone, with central administrative block and converging wings for male and female patients connected by verandah, and facing due north.

In comparing the various sanatoriums we would like to note a few points as to—

Altitude.—The favorite elevation seems to be about 1,000 feet: King's Tableland is, however, 2,700 feet, and Dalby 1,500 feet; Greenvale is only 600 feet, Echuca 400 feet, while Avon Pine and the Nurse Maud Camp are practically at sea level.

Aspect.—Generally N., N.E., or E. In the southern parts of Australia I am strongly convinced that the main building should face due north, with the wings closing in somewhat: this gives the full advantage of sunshine in winter, and affords shelter from winds. In rooms facing east the morning sun must disturb the patient's rest in summer.

Building Material.—As a matter of economy, wood has generally been chosen; but, if the building is to last any time, I think stone will be found cheaper, while it has the advantage of equalising temperature. Some sanatoriums have tents and shelters of a very light description, but, if the other States are like South Australia, strong winds must make them at times uninhabitable. Thirlmere is built of stone, but its three stories are quite out of keeping with Australian tastes. The Belair Sanatoriums are also of stone: the tiled roof of Nunyara is picturesque, and invaluable for mitigating extreme summer heat.

Rooms.—In the older institutions large wards are provided, and, for the sake of economy, some of the newer ones have rooms for four to six patients; but I think that every patient should have a room to himself. Verandahs, or their equivalent, are found in nearly all: the seclusion insisted on at Nordrach does not seem to suit Australian tastes. Artificial heating is, for the main part, not attempted, rugs and hot-water bottles being relied on. Open fires and stoves are found at Thirlmere, Cambridge, Avon Pine, and Kalyra, in dining and recreation rooms; but are seldom used. Nunyara has a complete system of hot-water radiators, with hot and cold water laid on to each bedroom. Lighting is generally by kerosine lamps. Cambridge has electric light, and Nunyara acetylene gas.

Drainage.—King's Tableland, Cambridge, and Nunyara have septic tanks; the rest more primitive methods of sewage disposal.

Treatment.—There seems a general disposition to avoid anything in the way of specific treatment, and to rely solely on simple hygienic measures. Even cod liver oil is never seen in most of the sanatoriums. Systematic inhalations are used in one institution, while two indulge somewhat in oil-massage.

The question of temperature is a most important one, but would require a paper for adequate discussion. The oral temperature is accepted as sufficiently accurate in all except King's Tableland and Greenvale, where the rectal temperature is recorded. It seems agreed that a temperature of 98°.8 in the morning, or 100° at night, necessitates absolute rest. The question of rest and exercise is, after all, the most vital to success, and one which requires the constant supervision and matured experience of the medical superintendent. It is in this matter, above everything else, that home treatment errs on one side or the other: it is very difficult to get people to realise the importance of this matter; one so frequently comes across patients with active disease who are practising deep-breathing, and thereby unknowingly doing their best to extend their trouble. When active disease is present, the quieter the lung can be kept the better, provided that perfect oxygenation of blood is taking place. The degree of lung movement varies with the amount of tidal air, and I have been experimenting to find out how much this is affected by different circumstances. It is very difficult to measure tidal air; but, by trying the experiments on different patients who had no idea of the nature of the experiments, I think our results are fairly accurate. I might summarise thus:—

Normal tidal air	500 c.c.
Patients walking gently up moderate incline	700 c.c.
After resting for half an hour	400 c.c.
Patient walking gently on flat	500 c.c.
After resting an hour	300 c.c.
Patient with limited lung disease, but at absolute rest on account of elevated temperature	150-200 c.c.

We must remember that laughing, coughing, or forcibly inhaling disinfectants cause a great increase in chest movements, and ought to be limited as much as possible.

Dr. Haldane has pointed out that the effect of breathing impure air is simply to lead to deeper breathing, and, in healthy persons, he suggests that this may be really a benefit: I disagree entirely with Dr. Haldane's conclusions; but his facts are undeniable. Impure air causes deep breathing; the perfectly pure air of a sanatorium, combined with perfect rest when lying down, enables a patient to reduce the amount of tidal air, and therefore the chest movements to a minimum. As the disease becomes quiescent, very slowly and gradually, and under constant supervision, the patient is

permitted to increase the respiratory movements up to the normal. I think I am right in saying that this is a process which can never be properly carried out except in a sanatorium.

In this connection I might say something about *Recreations* and *Amusements*. It is absolutely essential for success that a patient's life be non-exciting, yet at the same time bright and varied. Walking is by far the most important form of exercise, and its pleasurableness depends very much on the variety and picturesqueness of the walks. Croquet is allowed in most places for the good patient. Motoring forms an important alternative at one sanatorium. Games—such as draughts, cards, even billiards—and music are permissible, if they can be carried on without excitement. At one institution private theatricals are indulged in ; but I think that is too risky.

In conclusion, I would like to bring before your notice the desirability of agreeing to a unifom system of classification. Statistics are misleading : all classifications are open to objections. But, for public reports, we must have both. One classification consists of four classes, according to the number of lobes affected : Another, Class I., good cases ; Class II., hopeful cases ; Class III., cases which might be benefited, but cannot expect complete recovery ; Class IV., unsuitable cases. Dr. McIntyre Sinclair divides his cases into Early, Intermediate, Advanced. While the generally accepted Turban's classification—Stage I., slight affection, not more than one lobe or two half-lobes ; stage II., slight affection of not more than two lobes, or severe affection of not more than one ; stage III., all beyond stage II. "Class," I think, is a better word than "stage," and four classes give more latitude, without being cumbersome. I therefore propose a modification of Turban's.

Proposed Classification.

Class I. Incipient. Infiltration of one or both apices. Closed. No bacilli. With or without hoemoptysis.

Class II. Cases in which not more than one lobe is involved, or half of two lobes. Limited softening.

Class III. Cases in which not more than two lobes or equivalent amount, or cases complicated by superficial disease of larynx amenable to treatment.

Class IV. More extensive disease than class III., or such as are complicated with deep laryngeal trouble, intestinal disease, or persistent albuminuria.

DISCUSSION ON USE OF TUBERCULIN.

Dr. Gault thought that tuberculin might be more extensively used in sanatoriums, for the purpose of confirming diagnosis in doubtful cases. As for treatment, it was admitted that tuberculin was only successful in early cases. So long as sanatorium treatment gave such excellent results (80 per cent. to 90 per cent.), in such cases it was not likely that tuberculin would be much used for treatment.

Dr. Springthorpe congratulated Doctors Stoney and Gault on the practical papers which they had brought before the Section. Sanatoria met many of the constitutional requirements of tuberculosis in a certain number of cases, but only in a general and not in a specific manner. The underlying principles could be applied without sanatoria, and the specific antagonist was still wanting. Tuberculin was useful not only for diagnostic purposes, but also as a specific to help in building up a systemic resistance, if properly applied, in suitable cases, and many sanatoria cases were so suitable. The mixed infections, however, were probably so varied that the secret of individual immunity might depend upon antitoxins produced after manipulation

of the individual sputum. It was the constitutional factor that required strengthening, rather than the bacterial that required destruction. The healthy were practically immune so long as they remained healthy: hence the value of everything that promoted healthy existence. Even if the tubercle bacillus were exterminated, there was probably some related organism that would develop some new disease under the new conditions.

THE PROGNOSIS OF PULMONARY TUBERCULOSIS.

By M. McIntyre Sinclair, M.D., Glasgow, D.P.H., Camb.,
Resident Medical Superintendent, King's Tableland Sanatorium, Blue Mountains, N.S.W.

I propose dealing shortly in this paper with some points in the prognosis of pulmonary tuberculosis. Someone has said that the wise physician will not give a prognosis in this disease; but, unfortunately, it is impossible to deal with a sanatorium full of consumptives without attempting a forecast of each case for one's own satisfaction, if not for that of the patient and his friends. The disease is so variable in its course, so full of surprises, and the symptoms and complications open to so many interpretations, that one might well hesitate on the threshold of such a paper. I need make no apology if I do not attempt to treat the subject exhaustively. I think it preferable to pass over altogether, or in a few words, points on which we are more or less agreed, and to refer more particularly to other factors in prognosis regarding whose influence more or less difference of opinion may be expressed.

The fact that tuberculosis is absolutely curable has been fully established by *post-mortem* evidence. Pathologists daily find healed tubercular lesions in persons dying from diseases other than phthisis, and the records vary from 7.5 per cent. to 38.8 per cent. Osler has recently given it as his opinion that no one reaches old age without having a focus of tuberculosis somewhere, and he quotes the researches of Naegeli as establishing the truth of this contention. The latter observer made a special study of this question in a series of 500 *post-mortem* examinations. Every organ of the body was investigated, sections made of each part, and particular attention devoted to an inspection of the individual lymph glands. Tuberculous lesions were found in 50 per 100 of the bodies examined to the age of 15, 96 per 100 at the age of 18, and by the time the fortieth year was reached a tuberculous focus was found in every body. It is evident from this and other researches that tuberculosis is recovered from—or at least remains latent—in a very large percentage of the population, and that only a comparatively small proportion of those affected actually die from this disease. These records prove that the majority of the human race possess sufficient powers of resistance to cope successfully with the bacillus when once it has gained access to the tissues. In many of the cases under consideration the lesion remains practically insignificant, and the person attacked does not arrive at that stage in which definite symptoms of disease are presented. The remarks in the remainder of my paper will, for the most part, be directed towards that other less fortunate section in whom the bacillus has for the time being gained the ascendancy, and has produced sufficient constitutional or local signs of disturbance to bring the case under the notice of the physician. Unfortunately, in this group the prognosis is not so roseate as one might be led to infer from the more resistant group of Naegeli, to which I have just drawn your attention.

Generally speaking, we may assume at the outset that two factors determine the fate of the victim attacked—the virulence of the bacillus, and the natural resistance of the individual. These two factors are so closely related that it is somewhat difficult to dissociate the influence of the one from the other. The development of virulence in a particular bacillus will, I think, be to a large extent determined by the nature of the soil on which that bacillus is implanted. The resisting power to tubercle markedly varies in animals of different species ; it also varies, though to a lesser degree, in animals of the same species, and even of the same family. It seems somewhat strange that the influence of family hsitory on the prognosis of pulmonary tuberculosis still remains a most debatable factor. Most authorities affirm that family history has an unfavorable influence, not only on the incidence of the disease, but also on the prognosis of any particular case once the disease has developed. Others use the same series of figures to prove the opposite contention, that children of the tuberculous inherit an immunity against the disease. As a supporter of inherited predisposition, Turban goes further than most writers, and argues not only for inheritance of a general predisposition but for the inheritance of a specially vulnerable spot, a *locus minoris resistentiæ,* as it is termed. For instance, he relates a series of cases which have been under his observation where, in certain families, there was a tendency for the disease to begin at one particular apex, and to run a very similar course ; and, in support of his theory, mentions the somewhat analagous case of Brehmer's, in which a mother and her four eldest children all developed lupus on the same cheek.

Since reading this paper a few years ago, I have particularly watched a number of cases of pulmonary tuberculosis in members of the same families, and, while I have seen occasional cases to support the above theory, I have been unable to convince myself that it is anything like a common occurrence. After all, it is the usual thing for pulmonary tubercule to begin at one or other apex, and it should be no difficult matter to find cases in one family with the disease commencing at a similar spot. Not long ago I had a patient under my care with ulceration of the epiglottis, and tubercular infiltration of both cords ; his father and an elder brother both died from tuberculosis of the larynx, and in all three cases the lung symptoms played a subordinate part. This group is the most definite one I have observed to support the theory of *locus minoris resistentiæ,* but such cases are not seen every day. If I might express an opinion on a very much disputed point, I should say that I think hereditary predisposition has in the past been considerably over-rated, and that it has often led to gloomy prognostications not always warranted by the facts of the case. I think we must all admit having seen families where one member after another developed a virulent type of the disease, and rapidly died out. Such cases leave a strong impress on one's mind ; but, in looking over a large number of cases treated in sanatoria, I have found my percentage of arrests almost as good in those with as in those without family history. Conversely, I have looked up the records of the most virulent cases I have treated during the last few years—cases which progressed rapidly to a fatal issue without the slightest response to treatment—and I have been interested to find that in a number of these no trace of tuberculosis could be found on either side of the family tree. Provided the other features are satisfactory, my own rule is to let family history take a very subsidiary position in estimating the prognosis of any given case.

Reverting to the subject of the tubercule bacillus and its virulence, it would be interesting if one could connect a particular type of bacillus with a certain type of pulmonary disease. So far as I am aware, not much attention has been given to this aspect of the subject, though no one can make a routine examination of the sputum without frequently noting morphological variations

in the tubercle bacilli present. I believe it was Fränkel who first expressed the opinion that the small type of bacillus was more frequently associated with the more acute processes of tubercular lung disease; and I am inclined to agree with this observation. The length of the tubercle bacillus may be generally stated at 1.5 to 3.5, or 4u.; but considerable variations are not infrequently found. A year or two ago I made observations in a series of cases, and found the bacilli varying from short slender uniformly stained rods of 1.5u. or even less in length to the larger thread-like beaded bacillus of 8 or even 9u. in length. The former short variety I have usually found in more acute processes, and, when present, is frequently in large numbers—suggestive of rapid proliferation. I am also of opinion that congestive attacks, or lobular or even lobar extensions, of the disease are more commonly associated with the short slender type of bacillus than with its long thread-like prototype. The latter form is generally associated with the more chronic or fibroid types of the disease, and, from a prognostic standpoint, may, I think, be regarded less unfavorably than the short slender variety. Occasionally one finds branched and clubbed forms, sometimes regarded as involution or retrograde types of the bacillus; these are less acid-fast than the ordinary rods, and are therefore less seldom noticed in routine examinations of the spit. Some years ago Ransome asserted that tubercle bacillus could grow outside the body on sterilized filter or wall-paper, and that under those conditions it had a tendency to take on a thread-like or branched appearance, suggestive of the mycelial formation. Recent researches tend to place the tubercle bacillus among the streptothrices, and the involution forms met with in sputum may be regarded as a reversion to a saphrophytic or alternate type of existence. Rabinowitsch, Moeller, and others have demonstrated the fact that acid-fast organisms are widely distributed in nature—for instance in butter, in timothy and other grasses—and it is a probable speculation that the first case of pulmonary tuberculosis arose from one of these acid-fast saphrophytes becoming grafted on a weakened lung and assuming the parasitic type.

The statement that the long thread-like bacillus of pulmonary tuberculosis is of less grave import than the short slender variety must, however, be taken with certain reservations. Thus I have seen cases where the large type bacillus was present, and the lung apparently making good progress to recovery, when the development of a renal, meningeal, or other tubercular complication hastened the terminal issue. These observations appear interesting in view of the recent research by Moeller into the virulence of cultures derived from various human sources of tuberculosis. I need not refer to his work in detail further than to say that of the organisms examined he placed the tubercle bacilli from lupus as of greatest virulence, and next in order of virulence those from miliary tuberculosis, sub-acute pulmonary tuberculosis, chronic pulmonary tuberculosis, and fistula in ano. Taking these researches in association with the clinical observations I have just mentioned, I think it possible that a bacillus of retrograde type in the lung may develop a much higher grade of virulence when mobilised and translated to some other organ in the body. Such accidents would, of course, upset any prognostic calculations based on the type of bacillus present in the lung discharges. They also illustrate the difficulty of arriving at any certain prognosis from any one single factor, or even combination of factors, in an insidious treacherous disease like pulmonary tuberculosis.

I do not propose dwelling on the prognostic significance of the sputum further than to say that the number of bacilli present is largely a matter of discharge, and forms no certain criterion of the ultimate issue of any case. In the earlier stages of the disease the discovery of large numbers of tubercle bacilli on repeated examination must be regarded more seriously than where the numbers are few; but, in the later stages, large numbers of bacilli may

be discharged from a cavity which is fairly quiescent and circumscribed: and, indeed, these organisms frequently continue to be discharged for years in cases which are otherwise apparently doing well. In cases approaching quiescence a diminution in the quantity of sputum discharged is also worthy of note as of very favorable import.

Probably no factor is of greater prognostic significance in this disease than the condition of the circulatory system. A good deal of discussion has centred round the question whether the average subject of pulmonary tubercle has a small heart. Whether this be so or not, it is at any rate certain that a low blood pressure is a frequent concomitant of the disease, and that the rapidity and tension of the pulse may be taken as a most valuable aid in estimating the probable results of treatment. I always feel hopeful about a case whose pulse at rest is under 100 and of fair strength. Even where the disease has proceeded to localised cavitation, a pulse rate of 70 to 80 may determine a favorable issue. On the other hand, cases of even early disease with a rapid dicrotic—or, still worse, hyperdicrotic—pulse of 120 or over will as a rule go progressively downhill. I have never known a case of pulmonary tuberculosis, with a pulse rate of 140 regularly at rest, to recover: I know such recoveries are recorded, but I am satisfied they must be extremely rare. In the malignant type of case the pulse is generally of a soft rapid character. Such a pulse may represent a naturally feeble circulation in the subject of attack, or it may be an indication of extreme toxæmia resulting from a virulent infection. In either case the outlook is serious. Naumann studied 100 cases of tuberculosis, uncomplicated and without fever, in order to determine the blood pressure of such subjects. In sixty-nine the blood pressure was over 130 m.m., in thirteen cases from 115 to 130 m.m., and in eighteen under 115 m.m. These figures may be considered high, normal, and subnormal values. I have been unable to make an exact comparison with Naumann's figures, since most of the cases admitted to King's Tableland have some slight pyrexia; but, so far as my observations go, I think Naumann's records in the tuberculous subject distinctly too high. Our cases, which are mostly in the early and intermediate stages, are selected by the medical references on the grounds of their possessing a reasonable hope of recovery; and, as a matter of fact, 54 per cent. have proceeded to complete arrest of the disease, while all but a small percentage have improved to a greater or less extent. With such patients I have found it very much the exception to have blood pressures exceeding normal, and most of them are the very reverse. Of the last 100 consecutive cases the blood pressures on admission worked out at seventy cases subnormal, twenty-six normal, and only four above normal. The figures varied from 75 Hg. m.m. to 136 Hg. m.m., and in the 100 cases the average blood pressure was only 107 Hg. m.m. In twenty-five uncomplicated cases of moderately early disease, and with evening pyrexia not exceeding 99°.6, and in all of whom the disease proceeded to apparent recovery or arrest, the blood pressure averaged 110 Hg. m.m., or of a slightly subnormal value. My results are therefore contradictory of Naumann's, and are more in accordance with the general accepted clinical observation, that low tension is one of the commonest symptoms in pulmonary tuberculosis. The observations in my series of cases were taken on a Martin's mercurial hæmo-dynamometer. I look upon blood pressures under 100 Hg. m.m. as generally bad from the prognostic standpoint, with, of course, a corresponding improvement in the outlook as the values approach normal. I need not pursue the subject at present further than to say I have not observed any essential relationship between the recorded blood pressures and the extent of lung involved.

The temperature in pulmonary tuberculosis is a valuable indication of progress, and of considerable prognostic significance. Like other factors,

the temperature chart is open to different interpretations, and I shall only attempt to deal with a few outstanding points. Generally speaking, apyrexial cases are more favorable than pyrexial, and the more marked the pyrexia the less favorable the outlook. Occasionally, however, one sees exceptional cases of advanced progressive disease in which the temperature remains persistently subnormal. In these cases there appear to be deficient powers of reaction, and the prognosis must be founded on the physical signs and general condition, rather than on the degree of temperature present. In cases of marked pyrexia one should carefully watch the patient for a time before pronouncing an adverse opinion; under proper treatment such temperatures may subside in the course of a few weeks. In other cases the pyrexia is the indication of a fulminating type of disease, which progresses rapidly to a fatal termination. Koch and his pupils have taught that temperatures in pulmonary tuberculosis exceeding 100°.4 (38° C.) are due to the presence of mixed infection, the more usual complicating organisms being the streptococci, pyocyaneus, tetragonus, staphylococci, and the bacteria of influenza. The steep hectic fever, chills, night sweats, and rapid wasting characteristic of the later and terminal stages of pulmonary phthisis are attributed to the action of these organisms. The presence of pyrexia due to such infection would of course add gravity to the prognosis of the case involved. The subject of mixed infection is one on which considerable diversity of opinion exists, and it would be beyond the province of this paper to enter on a consideration of the various conflicting views: I would only say that, while acknowledging the occurrence of mixed infections and their gravity from the prognostic standpoint, I am also satisfied that pyrexia exceeding 100°.4 may often be due to processes of a purely tubercular character. Conversely, one will sometimes find cases in which foreign pathogenic organisms are present, but which nevertheless continue to run an apyrexial course from beginning to end. In the latter event the organisms are no doubt in a latent condition, and, comparing my experience with that of others, I think it probable that such latency is a more common occurrence in an open-air sanatorium than under the ordinary conditions of practice. No doubt the subsidence of temperature resulting from sanatorium treatment is in many cases due to the fact that a mixed infection has been reduced to a purely tuberculous process, with a corresponding improvement in the prognostic outlook.

There are other studies in connection with the temperature chart—such as the relative values of oral, axillary, and rectal temperatures; the significance of "exercise" as compared with "rest" temperatures; and the interpretation of subnormal, inverse, and other types. It would take up too much time to consider these fully, though they must all be of interest to those engaged in this class of work. I would only say that a careful record of the temperature chart enables one with considerable certainty to follow, day by day, the progress of the disease, and to anticipate, or possibly avoid, extensions and complications of various kinds. In the event of intercurrent complications, resiliency of temperature is a point of good import—the term "resiliency" being applied to the degree of ease with which an elevated temperature curve subsides to normal after the presence of some disturbing influence.

One of the most promising fields for research on prognosis and treatment is opened up by the recent researches of Leishman, Wright, and Douglas on the opsonic indices of blood serum. Modern studies on immunity show that the phagocytic effect obtained when bacteria are introduced into the blood is dependent upon an action exerted by the blood fluids directly upon the micro-organisms. This action, which prepares the micro-organisms for phagocytosis, has been termed by Wright an "opsonic" action, and a method has been devised whereby the opsonic index of the blood serum of any indi-

vidual may be expressed in terms of almost mathematical precision. Taking the normal index as unity, Wright and Douglas have found that the indices in cases of pulmonary tuberculosis may be as low as .3, or even less, and that generally speaking the opsonic indices in the various forms of tuberculosis are considerably subnormal. It has also been found that the injection of certain vaccines may be followed by an increase in the protective substances in the blood with increased phagocytosis, and, of course, a more favorable prognostic outlook. The vaccines used in Wright's research on tuberculosis were the old and new tuberculins (T.R.) of Koch. The injection of an over-dose of vaccine will be followed by a negative phase, in which the protective substances in the blood are decreased after inoculation; and this untoward result is said to be due to an overtaxing of the machinery of immunisation. In the course of treatment the endeavor would of course be to produce a positive or favorable phase, and to avoid the production of a negative phase. By a series of successive inoculations, Wright has shown that one positive phase may accumulate on top of another till finally complete immunisation is the result.

Bulloch has followed up Wright's work on similar lines, and agrees that the opsonic indices are, as a rule, low in tuberculosis. He found 65 per cent. of lupus cases had an index below .8, and, in a general way, there was a correspondence between low opsonic index and a failure to cure by means of the Finsen light. In active tuberculous bone disease the index was also low. In pulmonary tuberculosis the results appear somewhat confusing. In this disease Bulloch found the indices very variable, figures being often obtained considerably above the normal, while a large number of cases of "arrest" or "cure" of early pulmonary tuberculosis showed an opsonic index considerably below the normal. The importance of these researches from the points of view of treatment and prognosis is very apparent, and further work on the same lines by bacteriologists in association with the clinical physician would appear to promise results of much value in the near future.

I feel that this paper has already exceeded the limits I intended at the outset. You will notice I have not referred to many material points in prognosis—such as the factors of nutrition and assimilation; to the physical signs and area of lung involved; to the graver prognostic outlook presented by such complications as diabetes, alcoholism, syphilis, tuberculosis of the larynx, meninges, intestine, genito-urinary tract, or evidence of disseminated tubercular disease. If I have not considered these, and other factors, it is not from any want of appreciation of their significance. In forming a prognosis in pulmonary tuberculosis one has to consider not only each individual factor present, but to take a comprehensive view of the whole case. In the present paper I have simply attempted to outline a few outstanding points, some of more or less debatable value; and I trust the consideration of these has been not altogether without interest to the members present.

REFERENCES.

(1) Osler, Revue Internationale de la Tuberculose, June, 1905.
(2) Maxon King, Trans. Brit. Cong., February, 1901. Vol. 3, p. 641.
(3) Die Vererbung des Locus Menoris resistentiae bei der Lungentuberkulose. Von Dr. K. Turban, Leipzic, 1902.
(4) W. Ransome, Weber Parkes Prize Essay, 1897.
(5) Moeller, Vergleichende experimentelle Studien über Virulenz verschiedener Tuberkulbazillenstamme menschlicher Herkunft. Zeitschrift für Tuberkulose und Heilstättenwesen B. 5 H 1.
(6) Naumann in Janeway's Blood Pressure, p. 232.
(7) On the Action exerted upon the Tubercle Bacillus by Human Blood Fluids. By A. E. Wright, M.D., and S. R. Douglas, M.R.C.S., *Lancet*, October 22nd, 1904: and A. E. Wright, in *British Medical Journal*, September 10th, 1904.
(8) Bulloch, Path. Soc., London, *Lancet*, January 21st, 1905.

RADIUM AND ELECTRO-THERAPEUTICS IN THE TREATMENT OF DISEASES OF THE SKIN.

By Herman Lawrence, M.R.C.P., &c.,

Dermatologist to Melbourne Hospital, and Physician for Diseases of the Skin to St. Vincent's Hospital, Melbourne; Hon. Consulting Dermatologist to Queen Victoria Hospital.

Ladies and Gentlemen—Since we met at the last Medical Congress, radium has, so to speak, come into the world, and with its discovery science has received many shocks. This new element has proved itself to be of no small importance in the matter of the treatment of certain skin diseases. Likewise during this time the wonderful powers of radio-therapy, actino-therapy, and the high frequency currents as applied to diseases of the skin, have become recognised; and as I have had unusual opportunities for the application of these methods, I have chosen this subject for my paper. When radium was discovered it was stated that there could be very little of this new element in the substance of the earth. Certainly this prediction seems to still hold true, for specimens of radium—at least, of any therapeutical value—are still practically unattainable, out here. You are aware that pure radium, being so great an oxidisable element, has not yet been obtained in a pure state. The radium salts, the chlorides and bromides, which are used therapeutically are constantly emitting three varieties of rays, called the A, B, and G rays; these rays are considered to be produced by the spontaneous disintegration of its molecular parts. In my specimens the radium is encased in little aluminium boxes, the aluminium being one hundredth of an inch in thickness; this covering cuts off the A-rays. The A-rays are charged with positive electricity and have very little penetrative power, being stopped by a sheet of paper, they are of little value therapeutically. The Beta rays, charged negatively, have much greater penetrative power; and the G-rays, which are supposed to resemble the X-rays, have still greater powers of penetration. The therapeutical value of a specimen of radium depends upon the penetrative power of its Beta and Gama rays. I have here thirty-five milligrammes of radium, and will now exhibit a radiograph taken through a partition wall separating one bedroom from another. The radium was being placed near the wall in one bedroom and some steel instruments placed upon the other side of the wall—that is, in the next room—then a photographic plate was suitably supported behind the instruments. The rays from the radium, passing through the wall, produced a shadowgraph of the steel instruments upon the photographic plate. As I have an electroscope handy, I will also demonstrate the power the radium has of rendering the air near to it conductive of electricity, that is to say, when I charge the electroscope—whether it be with positive or negative electricity—the radium has the power of rendering the air capable of conducting away the electrical charge from the electroscope.

I now charge the electroscope with positive electricity, by rubbing glass-rod and silk together, and you notice the radium, even at the distance of several feet, quickly causes the charge to escape; and, likewise, when I charge it with negative electricity, the radium still causes the charge to escape. This is a very delicate test for radium, and was used by Mdme. Curie for separating out the radio-active substance from crude pitch-blend.

The Effect of Radium upon Micro-organisms.—Some write positively as regards the bactericidal effects of radium; others, however, have not obtained any definite results. I exposed a mould, taken from a decaying strawberry

and grown upon agar for the purpose of this experiment to radium for two hours ; this specimen was then placed in a fresh sterile tube of agar, and it grew apparently without having suffered any injury.

The Therapeutical Effect of Radium.—When radium was discovered it was thought, on account of the wonderful penetrating powers of its rays, it might prove of value in the treatment of cancer. This hope, however, was not supported by practical results, and the value of radium as an agent in treatment was then altogether belittled, for it has since proved itself to be one of the most potent therapeutical agents ever yet discovered. Your radium specimen is always ready for use—it cannot get out of order ; whereas, with the X-rays the amount of attention necessary for their successful application is never ended. Personally, I combine the treatment by radium with radio-therapy or actino-therapy, as I find it convenient or necessary to do so. In extensive or ulcerative cases of lupus I apply the X-rays, and when the case is, so to speak, toned down, but with the so-called yellow spots markedly in evidence—especially so when the part is pressed upon with the dermoscope—I then apply the radium to these spots, and it has proved itself of veritable use just when the X-rays seem to have ceased to be effective. Many rodent ulcers have healed fairly quickly under the influence of radium alone. Warts, which have proved intractable with the usual applications, have disappeared when the radium has been applied to them. Lupus-erythematosus, more particularly the chronic discoid patches, have cleared up, leaving a scar such as the disease itself would probably have left after many years duration. Some patients describe the sensation they feel while holding the radium in contact with the diseased part as burning and drawing ; others, apparently, are unaware of any sensation whatever. Radium will cause necrosis of the skin, and one has to use judgment as to whether the case has had sufficient treatment. One must be careful that the radium is not itself producing destruction of the tissue, as I have seen the disease soon heal up when the applications of the radium were discontinued. The rays from the specimen of radium marked No. 1 can be detected in a dark room penetrating ten pennies placed one upon another. The aluminium covering is likely to be eaten away by the free bromide, and radium contained in these aluminium cases should be carefully watched. Rays from the radium can be detected in the dark with the eyes shut by placing it upon the eyelids ; and this fact is made use of in cases of cataract, and as regards the question of the advisability of operation.

X-rays.—Although the Röntgen rays were discovered some ten years ago, and were later on used in the treatment of lupus, rodent ulcer, hypertrychosis, it was not until the last two years that the marvellous effect of radio-therapy upon that large group of skin diseases which might be brought under the heading of seborrhœa, eczema, psoriasis, was recognised. Norman Walker, a student of dermatology of marked ability, who, having had the opportunity of seeing the treatment of skin diseases as carried out by the authorities of the world, used the following words in expressing his opinion at that time upon the treatment of skin affections by radio-therapy, viz.—" The most potent of the recent additions to the dermatologists' armamentarium are those mysterious electrical vibrations known as the Röntgen or X-rays." : and that opinion was formed before the actual wonders of radio-therapy in the treatment of skin affections was realised. In the treatment of intractable cases of acne vulgaris I consider radio-therapy plays a most important part. In tinea tonsurans, resisting all other treatment, radio-therapy can be relied on to cure the disease ; but of course care is necessary here in the degree of application, as well as in a great many other cases. Sycosis, whether it is a case of tinea barbæ or so-called non-parasitic variety, you can almost name the number of applications for successful treatment.

The previous duration of disease, even many years, does not alter the prognosis. Alopecia areata frequently improves rapidly under this treatment. I have had many cases of eczema, psoriasis, and seborrhœa apparently cured by radio-therapy. And if radio-therapy could be relied upon to successfully treat all the skin troubles which may be brought under the headings of eczema, psoriasis, and seborrhœa, the question would arise, what are the skin troubles which radio-therapy could not cure? One must, of course, at once recognise the fact that there are many skin affections which do not require treatment by radio-therapy—for instance, impetigo contagiosa is generally so readily cured by suitably applied parasiticide applications, dermatitis traumatica, drug erruptions, feigned eruptions, eruptions due to plants, and other recognisable external influences; all such diseases, of course, require such treatment as is obviously suggested in each particular case.

The question of dosage, or efficient application of the rays, must necessarily depend upon the nature of the case, that is to say, one is compelled to use extra care in treating acne of the face; but if the case be one of rodent ulcer or lupus, a stronger exposure—producing reaction—may be necessary. As a rule I seldom find a reaction necessary, and believe that the pathological alterations in the skin—such as atrophy of sebaceous and sweat glands—can be produced without ever having applied the rays to the extent of producing any apparent reaction in the tissues.

Dermametropathism in the Light of Radio-therapy.—At the Congress held at Hobart three years ago I read a paper called "Skin Markings," in which I contended that "the irritability of tissues in general may possibly, in some diseases, be calculated by the irritability of the tissues of the skin; at any rate, that certain markings prognosed the chronicity of certain diseases, and advise, in some cases, the near onset of a relapse of the disease, and that certain markings explain the exaggeration of the symptoms in some individuals, and that probably suitable treatment may be worked out by observing the effect of treatment upon the markings of the skin."

Twelve months afterwards I read a paper called "Dermametropathism," which was a further contribution upon the subject of skin markings. In this paper I contended that, by the markings, one could calculate or gauge the intensity or chronicity of the disease, and that the markings could be made use of for prognosticating the value of treatment. Although I used the commoner skin troubles—as acne vulgaris—for demonstration purposes, I laid special attention upon the marking of Urticaria pigmentosum, which marking was so unique and so plainly evidenced upon the application of pressure; but as the disease was practically incurable by treatment, it was impossible to demonstrate that the marking would gradually become normal, and its tendency to become normal would precede the cure of the disease. However, under the influence of radio-therapy we are now able to cure the disease in a reasonable time, and the return of the marking to normal has preceded the disappearance of the disease. Likewise with many other conditions, where there is now a comparatively quick change from the diseased state to normal, under treatment by radio-therapy the marking has similarly changed quickly to normal.

The Effect of X-rays upon Micro-organisms.—W. Allan, of New York, quotes twelve authorities who positively state that the X-rays have no harmful effects upon micro-organisms! One authority having exposed certain micro-organisms to the X-rays for twenty-four hours without doing them any apparent harm! On the other hand, he also quotes twelve authorities who positively state that the X-rays have the power of destroying micro-organisms! I now exhibit a few growths of micro-organisms, which have taken place in spite of some fairly severe exposures to the X-rays. In fact, in the case of the mould which grows so readily upon strawberries, the X-ray exposures were apparently followed by a decidedly exaggerated growth of the mycelium as compared with

a control growth. However, whether the X-rays destroy micro-organisms directly or not, they must certainly have the power of rendering the soil unsuitable for the continued growth of the micro-organism, as the micro-organisms disappear with the cure of the disease. In fact, radio-therapy opens up the question as to the actual attention the micro-organisms should receive as regards treatment of some diseases. Take, for instance, acne vulgaris; the primary etiological factor is credited by some to the bacillus acnes, and I believe that Koch's four postulates have been bacteriologically obtained in connection with this disease. Even so, I would not be prepared to look upon the bacillus acnes as the primary etiological factor. We must remember that, practically, acne vulgaris is a disease of age—roughly from 13 years to 20 years; it is likewise a disease of region—face and shoulders. The bacillus acnes is evidently only able to invade the skin at a certain age, and in certain regions. I am inclined to consider that the success of radio-therapy points to the primary etiological factor being, as I pointed out in my paper, "Skin Markings," viz., that influence which causes instability of the vaso-motor system, as evidenced by skin marking, and which is constantly present in the acne regions during the acne age; and that the success of radio-therapy in this disease is due to its power of controlling or remedying the altered conditions of the skin following upon an increased irritability of the vaso-motor supply of these regions. These altered conditions are hypertrophy of the sebaceous and sudoriporous glands, thickening and stiffening of the corneal layer, retention of sebaceous matter, greasiness of the skin. This condition gives rise to retention of sebaceous matter, formation of papules, exudation of serum, and then the staphylococci; and the bacillus acnes finds the soil suitable for its growth. Radio-therapy seems to me to be the last step before we discover the ideal treatment, namely, the prevention of the instability of the vaso-motor system which makes it possible for the disease we term acne vulgaris to occur.

The X-ray Bath.—What I have termed an X-ray bath is a method of applying the X-rays so that the whole skin of the patient may be exposed to the rays at one time. The necessity for such a procedure may be gathered from the following:—Pusey, of Chicago, describes a case of mycosis fungoides which was improving under the X-rays treatment, but, owing to the tediousness of the exposures and the generalisation of the tumors, the disease, so to speak, kept ahead of the treatment. So that with a case of granuloma fungoides, if the disease is well advanced, as pictured in the photograph I now exhibit of a case which occurred in my own practice some years ago, then the prognosis is—excluding X-rays treatment—practically hopeless, the patient probably dying within a few weeks. Applying the X-rays from one tube at a distance suitable to be effective would mean a great number of applications to get over the whole body, and one can hardly devote the whole of one day to the X-rays treatment of one patient; in fact, the patients themselves often tire of this method of treatment. With six tubes working at once, however, you can see that the disease—though universal—can be treated in a reasonable time, daily exposures being given if considered necessary. I generally move the patient around so that his skin is exposed to each set of tubes for about three minutes, that is, twelve minutes in all. I have also found this method of great service in treating general eczema, psoriasis, and also cases of urticaria. It could be tried in a case of generalised sarcoma. In children one need not use more than three or four tubes at a time, the surface of the body being so much less than in an adult. In cases of pruritus, where there has been no clinical evidence of skin disease, I have found this general exposure to X-rays followed by good results. I have used general exposures in localised troubles—as cancer of the breast. I believe this general exposure has an increased

action upon a localised disease, as compared with the application from a single tube to the part. This photograph is taken of the X-ray bath with the six tubes at work; three coils, you will notice, are worked with mercury motor breaks, whilst the others are worked with electrolitic breaks. The current supply to coils and breaks is described in accompanying chart. In the photograph I am working two tubes from one coil, as one of my coils for this work was out of order at that time. I have not found this method of working two tubes from one coil satisfactory, as it is impossible to keep the tubes at the same vacuum. Of course there is plenty of room for danger with this so-called X-ray bath, and one can easily understand that a generalised reaction from over-exposure could be followed by a fatal result. The same remark would hold good as regards the application of chloroform; and I advise anyone trying this method of treatment to use the greatest care in its application. In front of each tube will be noticed little radiometer discs for gauging the effects of the X-rays from each tube. The patient should stand in a cage made of cane, and the head protected with a lead foil mask. A trained nurse should be in attendance to guide the movements of the patient. A linen cover-all should be worn.

Treatment of Hypertrichons by Electrolysis and Radio-therapy.—If necessary, I anæsthetise the part by cataphoresis, using solution of adrenalin and cocaine.

Three needles are connected with fine wire, same as used in winding secondary of X-ray coil; these are connected with negative pole of suitable number of dry cells.

After removing the hair by electrolysis, I apply the X-rays, and have found that the X-ray exposures seem to considerably lessen, or even prevent, the electrolytic action of the needles being followed by any noticeable reaction in the skin.

This action of the X-rays seems to suggest that their efficient application in the form of an X-ray bath might lessen, if not prevent, the pitting of small-pox.

EPILEPSY: IS IT INCURABLE?

By George E. Rennie, M.D., M.R.C.P., Lond., Sydney.

Although epilepsy has had a place in medical nosology from the earliest times, and much speculation has been indulged in as to its true nature and pathology, we must confess that even at the present day the extent of our exact knowledge of its pathology and etiology is limited. There is much diversity of opinion as to what should be included under the term "epilepsy," and several definitions have been given which are considered by one and another to cover the whole ground of the symptomatology and pathology of the disease.

Now, before we can discuss the question of the curability of this disease, we must have some clear idea as to what we mean by epilepsy and what we mean by "cure." When we attempt to give a clear and comprehensive definition of this disease we find at once that we are called to a most difficult problem. Our difficulty arises from the fact that the chief symptom of the disease we commonly call epilepsy—namely, the convulsive attack, with more or less loss of consciousness, and the sequence of clonic and tonic spasms —is also a symptom which may occur under a variety of conditions, but in all of which we find, first of all, a state of irritability of the brain (to use a

rather coarse term), and some reflex exciting cause, either peripheral or central. For example, we know, as a clinical fact, that the brain of the infant or growing child is irritable, or reflexly excitable (whatever interpretation we may be able to put on this somewhat crude term), and that convulsive attacks, which are manifestations of uncontrolled cerebral motor discharges, are easily induced in a child. The convulsive attacks which we meet with in children who are teething, and in which the reflex exciting cause is considered to be the condition of inflammatory irritation at the terminations of the sensory branches of the fifth nerve, or who are the subjects of some gastro-intestinal irritation, owing to the invasion of some specific disease or to the presence of some undigested food or irritant, are manifestations of this uncontrolled excitability of the cerebral cortex. [But we know, also, that in some children, when the peripheral exciting causes are removed, no further convulsions may occur, and the child grow up perfectly strong and healthy, both in body and mind; but we also know that in some children, in whom convulsive attacks have been excited in the first instance—apparently as the result of some simple reflex irritation—after the removal of the specific irritation the convulsive attacks recur, and the child presents the recurrence of these attacks without any apparent cause, and we then pronounce the case one of "epilepsy."] We know, also, that convulsive attacks accompanied by an aura and loss of consciousness, with clonic and tonic spasms, occur at intervals in patients who are the subjects of gross organic cerebral disease—such as cerebral tumor—or who are the subjects of some congenital brain defect, in whom there are not only defective cerebral development (a negative condition, which, according to Hughlings Jackson, cannot produce positive symptoms), but also some irritation—a positive condition—which causes the positive symptom of convulsions. These cases are sometimes spoken of as symptomatic or Jacksonian epilepsy, if the convulsive attacks are confined to one side of the body or part of one side of the body, and not necessarily accompanied by loss of consciousness. It is clear that here we have a definite organic irritation of the cerebral cortex, and cases of this nature are supposed to be distinct from those of so-called "idiopathic" epilepsy, in which frequently no gross macroscopic lesion of the brain can be detected.

One of the chief difficulties which have existed, and will continue to exist, in the way of elucidation of the true pathology of the idiopathic epileptic convulsive attack is the fact that there is no tendency in the disease to lead to a fatal termination—at any rate in the early stages—that if a fatal termination does take place owing to the disease itself, it is generally after a series of attacks, or in a condition of status epilepticus, which itself gives rise to a state of congestion of the cerebral veins, accompanied by minute hæmorrhages, pathological conditions which tend to obscure rather than to help towards a true understanding of the pathology of the disease. Moreover, if the patient survive for some years and present recurring attacks of convulsions, it is not always easy to definitely state how much of the pathological conditions which may be found in the brain is due to the recurrent convulsions, with their accompanying cerebral congestion, and how much to the original—and what we might call the fundamental—cause of the disease. And here I would remark that it appears to me that a great deal of the experimental work which has been done towards solving the problem of the epileptic convulsion is beside the mark, and only tends to show—what we already know from our clinical and *post-mortem* experience—that uncontrolled motor cerebral discharges can be excited by disturbances of the cerebral circulation, or as the result of some irritant poison acting apparently upon the cerebral cortical cells themselves. We know, for example, that in uræmia, the condition in which the kidneys are failing in their functions of excretion, we get convulsive

attacks which are indistinguishable from epileptic convulsions; but this does not help us much towards solving the question of the pathology of idiopathic epilepsy, except in so far as we may reason from analogy that in idiopathic epilepsy we may have a similar kind of irritant, or toxin, or poison of some kind which irritates the brain and causes the discharge of uncontrolled motor activity. Clinical pathological investigations of cases of idiopathic epilepsy have shown that some change in the metabolism of the body takes place immediately antecedent to the occurrence of the fit, manifested by an increased toxicity of the blood and the excretions; and we are justified in concluding that there is probably a development of one or more complex chemical bodies which act as irritants on the cerebral cortical cells, and are the immediate cause of the convulsive attacks: in other words, the irritant is the result of auto-intoxication; and the balance of all modern investigations goes to support this view. But this alone is not sufficient to determine all the clinical phenomena of the disease. The microscopic examination of the cerebral cortex tends to show that, at any rate, in all long-standing cases of the disease, there is a wasting of the sensory cells of the second cortical layer with neuroglia hyperplasia. We have thus evidence that in true idiopathic epilepsy there is organic disease of the cerebral cortex, plus irritation from auto-intoxication.

Now, if we take a survey of the various morbid conditions which are commonly associated with the occurrence of convulsive attacks, we cannot shut our eyes to the fact that for the production of the typical convulsions with loss of consciousness and spasms, such as are met with in idiopathic epilepsy, some special condition of irritability of the cerebral cortical cells must be present. Why do we sometimes get typical convulsions of this nature for years before the symptoms specially characteristic of cerebral tumor manifest themselves, and in other cases of cerebral tumor no convulsions occur? The situation of the tumor in the brain alone will not explain it. Or, again, why do we get some cases of meningitis in which there is generally some encephalitis as well, with no convulsions, or at any rate only in the early period of the disease—a condition in which we have, *par excellence*, one would say, an irritation of the cerebral cortical cells by inflammatory products? Or, again, in some cases of uræmia we get various nervous symptoms, such as mania, or some paralytic conditions, and yet no convulsions? Surely these facts point to the conclusion that, for the development of a typical convulsion, there must be some special condition of the cerebral cortical cell, or else some special kind of irritant, whether that be toxic or vascular. We must, of course, recognise that for the production of the convulsive attack —whether this be what we may call a simple convulsion or a convulsion which is repeated at intervals in a patient who is pronounced to be an epileptic—we must always have an irritant, whether we can recognise its exact nature or not, that is, an irritant which is conveyed by a peripheral sensory nerve, or the nerves of the special senses, or a chemical irritant conveyed by the circulation to the brain; and we must have the cerebral motor cells in a condition capable of being reflexly stimulated to discharge; and there must also be an intact motor path. These conditions are necessary for the production of a typical convulsive attack; and just as under these conditions a convulsion may be excited by some peripheral sensory stimulus, so we also know that the motor discharge may be inhibited by a powerful peripheral stimulus.

We have, however, further to note that in some patients we do not get the fully developed convulsive attack, but only some slight loss of consciousness, with or without some very slight motor manifestation, either of the nature of a spasm or a relaxation of muscles. These so called attacks of petit mal may occur alone as the only symptom of "epilepsy," or they may occur associated with the fully developed convulsions.

Another fact which we must bear in mind is the frequent mental impairment which is associated with the recurrence of the convulsive attacks. This is commonly stated to be due to the recurrence of the fits; but this appears to me to be an erroneous view, for, if it were correct, we ought to find every patient who is the subject of recurrent convulsions to be the subject of some degree of mental impairment. This is, however, hardly in accordance with facts.

I think you will see the bearing of these remarks on the question raised in the title of this paper. We must, I think, regard convulsive attacks as of two kinds: first, those which are manifestations of an undue excitability of the cerebral cortex, which is aroused as a result of some peripheral sensory irritation, the main factor being the reflex irritation; and second, those in which there is a definite morbid condition of the cerebral cortex, that is, an organic structural change, a degeneration of the cells in the sensori-motor cortex. The former condition may possibly pass into the latter, but not necessarily so, and, under appropriate treatment, by the removal of the source or sources of peripheral irritation, the undue excitability of the cerebral cortex may be subdued, and the patient be cured of his convulsive attacks. To this class belong, I think, those cases of recurrent convulsions which are not accompanied by serious deterioration of mental or physical health. To the latter class belong the large proportion of cases of recurrent convulsions which are accompanied by signs of mental enfeeblement, and by various abnormal psychical manifestations—such as epileptic amnesia, the unconscious committing of crimes, &c.

We are thus driven back to review our position in regard to the definition of what we mean by "epilepsy." Are we to include all these different groups of cases under the one name "epilepsy," modified possibly by some adjective—such as "major," "minor," "psychical," &c.? I should consider that a patient who is suffering from recurrent convulsive attacks, which cease as a result of the removal of some peripheral irritation, *e.g.*, the removal of worms from the intestine, the correction of some refractive error, or the abstention from some chemical irritant—such as alcohol or absinthe—is not suffering from true epilepsy, but is the subject of reflex convulsions. But if, after the removal of all possible sources of peripheral irritation, the attacks occur, and specially if they recur in spite of an improvement in hygienic surroundings, and in spite of the depressant effects of the bromide or other drugs on the excitability of the cerebral cortex, then that patient is suffering from true idiopathic epilepsy; and such patients, I believe, invariably present some mental deterioration, whether this be a mere loss of control of temper, or increased irritability, or the more definite signs of dementia.

In my opinion the definitions usually given fail, in that they do not differentiate between these two types of convulsive attacks; namely, those depending on some simple reflex irritation, which can be recognised on careful investigation, and recurrent attacks for which no obvious cause can be found.

Within recent years several groups of statistics have been compiled, by different physicians, bearing on the question of the curability of epilepsy, and the conclusions arrived at have been that in from 5 to 15 per cent. of cases a cure is effected. But statistics are notoriously unreliable; and yet they are of some service, in enabling us to take a broad view of the general progress of patients suffering from recurrent convulsive attacks under different methods of treatment.

Now, before we can arrive at an answer to the question of the curability of a disease we must clearly understand what we mean by "cure." And here we are at once brought face to face with a difficulty. Some years ago the

American physicians discussed the question of what should be regarded as a cure of epilepsy. Some were disposed to consider a case as cured in which there was no recurrence of the convulsions for ten years, this being considered a sufficiently long period for the brain to have recovered a normal degree of stability. We know that it does take a very long time for the brain to lose its condition of reflex hyper-excitability, for patients who may have had no fits for perhaps two or three years while taking bromides will experience a return of the symptoms almost immediately on leaving off the medicine, the fits having been merely kept in abeyance, the disease not having been cured.

If we consider for a moment other kinds of disease, and ask ourselves are patients cured of them, we shall be obliged to answer that in many cases the disease is not really cured ; the patient is relieved, from time to time, of the symptoms, and may be free for years, and yet the tendency is towards a recurrence of the symptoms. Take, for example, the common disease rheumatism. A patient may have an attack of rheumatism. After some time the symptoms subside, and he returns to a normal condition of health, and he is considered to be cured. But we know that he is liable to a recurrence of the disease from time to time. although he may never have a recurrence of the symptoms for the rest of his life. Or take bronchitis : we know that a patient may be cured of an attack of bronchitis ; we also know that the bronchial mucous membrane has been damaged by the attack in such a way that the patient is more susceptible to a recurrence of the disease, and as he grows older the attacks may become more and more frequent, until ultimately be becomes a chronic bronchitic, and is incurable. Although he may have been relieved of the symptoms of the previous attacks, he cannot be considered to have been cured of his disease.

Now, I think a similar condition of things holds with regard to epilepsy. A child who inherits no tendency to epilepsy may have convulsions during teething, that is, convulsions due to some reflex peripheral irritation, and after the subsidence of the irritation no further convulsions may occur. But is it not true that the occurrence of these convulsions predisposes to the recurrence of convulsive attacks later in life ? The rapid development of the brain and nervous system coincident with puberty may favor the recurrence of the attacks, which, however, may subside under treatment. The patient may thenceforward have no further attacks. Or, on the other hand, later in life the convulsive attacks may recur, and the patient present the clinical phenomena of a case of so-called idiopathic epilepsy, and he may become a confirmed epileptic. One cannot predict the exact course which any individual case will pursue ; one can only say that the patient has been cured of his epilepsy when he remains free from the recurrence of the attacks up till the time of his death. The mere fact of a patient remaining free from the convulsive attacks for many years does not, in my opinion, mean that he has been cured of his disease ; and when statistics are adduced to show that epilepsy is cured in a certain proportion of cases under one or another line of treatment. I maintain that such are fallacious. All that they show is that. in a certain percentage of cases, as a result of treatment, the conditions favorable for the development of the convulsions do not recur ; but the potential state still remains. I would not be taken to mean that I do not believe in any method of treatment as being efficacious. On the contrary, I believe that, by a judicious use of drugs or other method of treatment, we may be able to prevent a recurrence of the fits to a greater or less degree. But drugs cannot cure a condition of organic degeneration of the cerebral cortex. such as underlies all cases of true organic idiopathic epilepsy ; all that we do is to lessen the reflex excitability or remove causes of peripheral irritation, but we do not cure epilepsy.

To recapitulate, then, in conclusion I would say that the answer to the question I have raised in this paper depends upon the view we take as to what we include under the term "epilepsy." If under this term we include all cases of recurrent convulsions, whether these be due to some recognisable reflex irritation or not, then we may agree with those physicians who maintain that in a certain proportion of cases a cure of the disease can be effected. If, however, we restrict the term in the way I have indicated, we must regard it as an organic disease of the cerebral cortex and, as such, as incurable.

DISCUSSION.

Dr. Jamieson said that before attempting to answer the question it was necessary to ask and give some reply to another : "What is epilepsy ?" There must always be a distinction made, if possible, between true epilepsy—called idiopathic—and all kinds of eclamptic or epileptiform attacks. This, of course, might be difficult enough, since eclampsia had so many causes ; many of them very obscure. They might be poisons, as in chronic renal disease, or irritations—peripheral or central. And it had to be remembered that the proneness to the occurrence of convulsions differed greatly in different persons, and at different ages. All young children had a considerable, though unequal, liability—what he had been in the habit of describing as *convulsibility*. This tended to pass off with advancing age, and the fact of an infant having had one or more attacks of general convulsions did not practically show that there was risk of the later development of true epilepsy. But the "convulsibility" might be so marked and persistent that the fear of such development was naturally and properly excited. One of the commonest causes of infantile convulsions, in his opinion, was acute indigestion ; pieces of meat, cheese, raisins, &c., might readily produce them in an infant, but would not do so in an ordinary healthy schoolboy. He once knew a case where such a cause as eating green fruit brought on convulsions in a boy of about 13, ordinarily in good health. In such a case it was natural to look forward with considerable misgivings to the boy's future as a probable epileptic. Where attacks began without obvious cause, recurred at intervals without any obvious excitant, and were not associated with any progressive signs of disease, either of the brain or other organ, the condition was probably epilepsy ; and, though the attacks might be suppressed for a time, cure was, to say the least, extremely unlikely. In his opinion true epilepsy, when once established, was practically an incurable condition. And perhaps the hopelessness was best seen where the petit mal attacks were a prominent feature. The bromides would often suppress the fully-developed convulsive attacks, but seemed almost always to be without effect against these less violent but really more maleficent manifestations. His own opinion, therefore, was that Dr. Rennie's question must be answered in the negative, in the present state of our knowledge.

DIETING IN RENAL DISEASE.

By George E. Rennie, M.D., M.R.C.P., Lond., Sydney.

In introducing this subject I wish, at the outset, to state that I intend to confine my remarks to the subject of dieting in the various forms of acute and chronic nephritis.

Some preliminary considerations are essential. In the structure of the kidney we have two important elements—(1) The epithelial lining of the

glomeruli and of the tubules, and (2) the blood vessels, both of which take an important share in the physiological function of excretion, and both of which may be involved to a greater or less extent in the morbid conditions of the kidney which determine the clinical manifestations of acute and chronic nephritis.

It is pretty certain that the water of the urine is derived from the blood, partly by a process of osmosis and partly by an active secretion on the part of the glomerular epithelium; and we know that the quantity of water excreted is directly related to the amount of blood which circulates through the kidneys, and also—though to a much less extent—to the blood pressure. The mineral salts and pigments in the urine are excreted from the blood by the secretory activity of the glomerular epithelium. The organic constituents, namely, urea, uric acid, and the extractives, are excreted by the tubular epithelium. In the case of one substance, hippuric acid, there is good reason to believe that it is formed in the kidney from benzoic acid. This shows that the kidney is capable of synthesising complex organic substances, and this function of the kidney will consequently suffer as well as the others.

In the case of acute parenchymatous nephritis we must remember that the kidneys are in a condition of acute inflammation, that is, they are responding to the irritation excited by the presence in the blood of a toxic substance or substances which are being excreted, and, in the process of excretion, are irritating the renal structures: the blood vessels are dilated, the circulation is slowed, there is exudation of serum into the surrounding tissues, and the tubular and glomerular epithelium is impaired in its nutrition, and consequently also in its selective excretory functions. Further, where there is general renal dropsy we know that the nutrition of the capillaries is impaired, so that exudation of serum into the connective tissue spaces is taking place. When such a condition passes into chronic parenchymatous nephritis we have a failure of the kidney to excrete the normal waste products of the body—the result of the ordinary metabolism, plus the waste products resulting from the food which has been ingested—and these excretory products in their turn irritate the kidney and also react upon the whole system, producing a condition of chronic toxæmia, or uræmia, as it is called. In this condition the stress of the disease falls upon the epithelial structures, which are impaired in their nutrition and manifest various degrees of degeneration; consequently they are unable either to synthesise or excrete as they should.

In chronic interstitial nephritis, or granular kidney, the primary lesion is probably in the blood vessels, this leading to overgrowth of the connective tissue and subsequent impairment of function and ultimate destruction of the epithelium of the glomeruli and of the tubules. This process is much more gradual in its development, but ultimately leads to a considerable reduction in the actual amount of actively functionating kidney substance. When we get an attack of acute inflammation superadded, or when the heart begins to fail and a condition of passive congestion in the kidney is set up, the excretory powers of the epithelium are almost in abeyance, and we get evidence of uræmia.

In considering the diet which is most suitable to be adopted in the treatment of these various conditions, it is necessary to bear in mind that the digestive powers of the stomach are impaired, the power of retaining and digesting different articles of diet varies, and this, to a certain extent, independent of the effect on the kidney of the metabolic products of the particular articles of food. We must remember that the nutrition of the body must be maintained somehow or other, and that, to effect this, a certain amount of the staple food constituents—namely, proteids, carbohydrates, and fat—

must be administered. We have further to bear in mind the end products which result from the digestion of the different articles of diet, and to consider how these end products are excreted. Hence we have the problem before us—how are we to so adjust our diet scale for patients suffering from acute or chronic renal disease so that a sufficient number of calories will be produced to maintain the body nutrition and, at the same time, while giving the kidney as little work to do as possible, the waste end products of the food shall be excreted as completely as possible? The ordinary constituents of the urine, as you know, are water, mineral salts, nitrogenous matters, and extractives; and these various constituents are normally not excreted with the same degree of facility. Von Noorden gives the following list of the relative facility of excretion of these substances:—Those excreted with difficulty: urea, creatinin pigments, hippuric acid, phosphates, inorganic sulphates, potassium salt (?), water; those well excreted: uric acid, xanthin bases, aromatic substances, ammonia, amido acids, chlorides, carbonates, water. This of course applies to kidneys in normal conditions; and the difficulties will be still greater if the kidneys are in a state of disease. We must, therefore, in selecting our diet list, avoid, as far as possible, using those articles of food which lead to the formation of such end products as are difficult of excretion.

We shall consider first acute parenchymatous nephritis. The pathological condition of the kidney varies somewhat in different forms of this disease; for example, in the nephritis which follows scarlet fever the stress of the disease falls specially upon the glomeruli, while, in other forms of the disease, the tubular epithelium is the seat of an irritant proliferation with congestion of the blood vessels. Hence, in scarlatinal glomerular nephritis we must endeavor so to adjust the diet that the glomeruli are relieved as far as possible from the function of excretion. Remember, therefore, that we must specially limit the amount of water and salts. In other forms of acute nephritis, where there is a diminished excretion of urine, partly, perhaps, owing to mechanical blocking of the tubules with epithelial debris and blood exudates, in which the glomerular epithelium is not specially involved, a more liberal supply of water might be indicated with a view of flushing the kidney, provided we can be sure that the kidneys are able to deal effectively with the hydræmic blood. It is this very point which has led to some diversity of opinion as to whether it is advisable or not to give large quantities of water to a patient suffering from acute nephritis. It is generally taught and believed that in acute nephritis a diet restricted to milk only is the best. Milk, of course, contains a large percentage of water, and in restricting the diet to milk only we are giving a large amount of water along with a relatively large amount of phosphoric acid, fat, and a smaller amount of proteid carbohydrate. Milk contains so much phosphoric acid that adults who are living on a pure milk diet show a daily increase in the urinary phosphorus excretion amounting to three or four grammes. We have seen above that phosphates are excreted with difficulty; consequently, when giving a pure milk diet, we are giving a fairly large amount of a substance which is excreted with difficulty by the kidney. To counteract this ill effect, von Noorden, some years ago, advised that patients suffering from acute nephritis should take a small quantity of carbonate of calcium several times a day along with their milk. By this means a large amount of the phosphoric acid is combined with the calcium, and is excreted by the bowel. Milk also contains a relatively large proportion of proteid, and a patient who is taking four or five pints of milk a day will receive from 110 to 120 grammes of proteid; thirty to thirty-five grammes of this will be converted into urea and require excretion by the kidneys, an amount which the kidney in a condition of acute inflammation

will be unable to deal with satisfactorily. Moreover, if the patient is able to take three or four pints of milk a day without vomiting, a considerable amount of water must be absorbed and the blood rendered hydræmic; when we keep the skin and bowels active a certain amount of this superfluous water is removed, and a comparatively speaking small amount of water remains to be excreted by the kidneys, and consequently very little flushing effect will be produced. If, in addition to the milk, we give our patients several tumblerfuls of distilled water per diem, we still further increase the hydræmic condition of the blood, we increase the amount of blood which is conveyed to the kidney, but unless the glomerular capillaries and the glomerular epithelium are in a fairly good condition so far as their excretory power is concerned, we do not get a flushing effect, and the hydræmic condition of the blood tends rather to increase the amount of dropsy, even if it have no other ill effect. If, however, the stress of the disease has fallen upon the tubular epithelium, then a flushing of the tubules may be effected; and this method of treatment and the removal of the epithelial debris and blood exudate will tend to relieve the congestion of the intertubular blood vessels and favor the return of the normal excretory power of the tubular epithelium. Hence, in my opinion, if we know the case to be one of scarlatinal nephritis, we should limit the amount of water and only give a comparatively small quantity of milk, with the addition of some carbonate of lime. If, on the other hand, we know the nephritis to be due to exposure to cold or some toxic influence other than scarlet fever, then the administration of a larger amount of prepared milk, together with large draughts of distilled water, may prove of considerable benefit; but should only be persevered in if there be a definite increase in the amount of urinary water, thus proving that the kidney is in a condition to deal with a large amount of hydræmic blood. If, on the other hand, there be no increase in the quantity of urine and the œdema increases rather than diminishes, then there should be a limitation in the amount of fluid ingested. We have seen that milk is not, in every case, at any rate, so perfectly harmless and beneficial as an article of diet in acute nephritis, and we have to consider what other articles of diet may be given either in addition to or in substitution for milk, since we have further to bear in mind that we are occasionally brought face to face with a patient who is unable to retain or digest the necessary amount of milk. It is well to remember that it has been ascertained that it is possible to administer very small quantities of proteid for a short time without impairing the health of the individual, provided a corresponding amount of carbohydrates is administered at the same time; consequently in the early stages of acute nephritis we can, if necessary, be content with administering only a small quantity of milk and increase the amount of fats by adding cream, and give carbohydrates in the form of oatmeal, groats, or maizena, with the addition of butter, sugar, honey, or golden syrup. By this varied diet we can give the desired quantity of calorics with only a small percentage of proteid. With the increase in the urinary water, and the diminution in the amount of œdema, and the subsidence of other symptoms, we may gradually add other articles of food—such as bread, vegetable soups, and later on, fish, chicken, or meat.

In chronic parenchymatous nephritis the pathological conditions are different from those in acute nephritis. We have not the condition of acute congestion of the blood vessels with sluggishness of the circulation, but a condition, as I have already said, of defective excretory powers in the renal epithelium. Hence we should avoid giving those articles of food whose end products are only excreted with some difficulty. This would necessarily lead us to restrict the amount of animal proteid and to increase the amounts of carbohydrates and fats. On these indications we should withhold meat

and meat extracts, both red and white, and supply the necessary amount of proteid in the form of milk albumen, eggs, or vegetable proteid. With regard to the administration of water, we must be guided by the same indications as in acute nephritis.

With regard to the dieting in chronic interstitial nephritis, or granular kidney, we are brought face to face with more difficult problems. This is essentially a chronic disease, and patients suffering from it may live for many years ; it is, consequently, difficult to lay down a diet which will be suitable at all periods of the disease, and which will be sufficiently varied to prevent patients from becoming nauseated and disgusted. We have, moreover, in this condition to pay particular attention not only to the kidney but also to the associated cardio-vascular disease. In consequence of the general high blood pressure, and the large amount of blood circulating through the kidney, there is always a large excretion of water ; the amount of water tending to increase, as Bradford has shown, as the amount of functional kidney substance tends to diminish. The only exception to this being during periods of intercurrent acute nephritis, or when the heart begins to fail and we get a condition of passive congestion of the kidney. As a general rule, too, at any rate in the early stages of the disease, the kidneys appear capable of excreting an almost normal amount of urinary solids. Bearing in mind these facts, there can surely be, not only no special advantage, but, on the contrary, positive harm in binding down patients suffering from this disease to a strict milk diet.

In view of the fact that milk contains a large amount of water, we should rather restrict the amount of milk which is drunk, and also restrict the amount of fluid of any kind. The digestive powers of the stomach are, to some extent, impaired, and we must give food in such a state and of such a nature as can be easily digested and assimilated. The administration of much fluid of any kind, whether it be plain water, or broths or soups, should be avoided, and the meals should be as dry as possible, and ample time allowed between each meal for its full digestion.

Now let us consider the special articles of diet as regards their suitability in this disease. First, as regards proteids, in what form should they be given? It has been commonly taught that all red meats are injurious, and that so-called white meats alone should be allowed. This opinion is based upon the supposition that red meats contain considerably more of the nitrogenous extractives that are injurious to the kidneys than white meats. Some investigations on this point were undertaken by Offer and Rosenquist ; and their experiments tended to show that the nitrogenous extractives were found to be sometimes more abundant in red meats and sometimes in white meats ; but greater differences were found in the same class of animals, and between several samples from the group of white meats on the one hand and of red meats on the other, than between the average amount contained in white and red meats. Certain kinds of white meat—such as poultry and hare—were even found to be richer in extractives, on an average, than beef. Two of von Noorden's assistants (Kaufmann and Mohr) studied the elimination of urinary excretion after the ingestion of equal amounts of white and red meats. Their results were ambiguous, but certainly tended to show that the amount of albumen in the urine was not influenced by the form in which proteid is taken. After all, clinical experience furnishes the best answer to this question. My own experience, so far as it goes, agrees with the statement made many years ago by Goodhart, that an occasional administration of red meat is not only harmless but is actually beneficial, in enabling us to give a more varied diet and relieving the monotony of a diet of fish and chicken. Von Noorden has arrived at the same opinion. We may say that red meat, in a form easily

digestible, may be allowed daily, provided no complication is present to contra-indicate it. There can be no objection either to the administration of albumen, in the form of eggs, either boiled or poached, or in the form of custard, in moderation. But the total amount of animal proteid, whether it be in the form of fish, fowl, meat, or eggs, must be restricted—but not to such an extent as to impair nutrition or physical strength ; and these must be presented in such a form as to be easily digested. It is important to remember that the functional activity of the kidneys depends upon the maintenance of a free flow of blood through them, and the cardiac hypertrophy which we find in cases of granular kidney secures this result. But the hypertrophied heart muscle demands an extra supply of nourishment in the shape of proteids and fats, and we must see that our dietary scale includes these in fair amount.

As regards vegetables, while, in general terms, one may say that these are harmless, we must remember that some—such as green peas, cabbage, &c.—are difficult of digestion, and tend to cause flatulence and thus to embarrass the heart. Some, on the other hand—such as radishes, celery, and asparagus, &c.—tend to directly irritate the kidney, and may lead to the occurrence of hæmaturia, or at any rate increase the amount of albumen. For the same reason, spices and condiments should be avoided by the subjects of granular kidney.

As regards the use of the ordinary beverages—such as tea, coffee, and cocoa—we have to consider their action from different points of view. As I have already remarked, the digestive processes in these patients are somewhat impaired, and anything that will tend to still further delay digestion and cause flatulence is to be avoided. The infusions of tea and coffee appear somewhat to delay digestion ; and, by reason of the theine and caffeine which they contain, they act as stimulants to the heart, possibly by direct action or through the cardiac ganglia. This action is not a powerful one in healthy persons, but when we have the heart hypertrophied, and the muscular tissue perhaps imperfectly nourished, by reason of the degeneration of the coronary arteries, the effect of these infusions may be detrimental unless taken in moderation. They also tend to raise blood pressure, and so increase the predisposition in these patients to the occurrence of cerebral hæmorrhage. There is reason to believe that caffeine and, probably, theine, have also a direct stimulating effect upon the renal cells, which leads to an increase in the ordinary excretion. But such stimulation, while it may be of therapeutic value under other pathological conditions, can hardly be considered beneficial when the renal epithelium is impaired in its nutrition, or actually degenerated. On the contrary, this action must be injurious, and accordingly tea and coffee should only be used sparingly. The same objection cannot be taken to cocoa, but, by reason of the large amount of fat which it contains, it may be digested only with difficulty.

As regards other beverages, which contain more or less alcohol, from the point of view of its poisonous influence on the kidney we should advise complete abstention from their use in any form. But here again other organs have to be considered besides the kidneys, specially the heart and circulation and the stomach. The use of alcohol may be indicated when the heart is flagging, and a cardiac stimulant is needed ; as, for example, when some complication—such as pneumonia or bronchitis—has developed. Then, again, the appetite may be more or less lost, and, according to some physicians, the use of a little wine or champagne with dinner may promote an appetite and aid digestion. But it is doubtful if the use of alcohol for these specific purposes can be defended, in view of its otherwise detrimental effects. especially as other drugs can be used to secure these ends ; and our safest course is to interdict its use absolutely. Some of the other so-called temperance

beverages, which contain only a minute quantity of alcohol, are injurious by reason of their other constituents; for example, ginger beer and ginger ale not infrequently contain capsicum, and this is a direct irritant to the kidney.

One further word on the use of water. Opinions differ as to the amount of water which should be allowed these patients. Where the amount of urine is largely increased, thirst may be a troublesome symptom, and a certain amount of water may be allowed to relieve it; but this is the only condition in which the administration of large quantities of water would be justifiable.

Tobacco has a more powerful action upon the heart than either tea, coffee, or alcohol, and the heart of the patient who is suffering from chronic granular kidney will not tolerate, as a general rule, a cardiac poison like tobacco. But here again circumstances and idiosyncrasies must be considered. Cigarettes must be unhesitatingly condemned; but a moderate use of the pipe or cigars—provided no cardiac irregularity, palpitation, or depression is produced—may be allowed if the patient has been an habitual smoker; for we must not attempt to deprive our nephritics of anything more than is absolutely essential. By attempting too much restriction we dishearten the patient, and, instead of enjoying a fair measure of good health for perhaps many years, he will probably become a confirmed hypochondriac.

DISCUSSION.

Dr. Jamieson said that, on such a large subject, it was difficult to make other than general statements. In acute nephritis, when the kidney had almost completely lost function, there should clearly be as little work as possible put on it. In the glomerular form there might almost be complete suppression, and it was manifestly undesirable to introduce more fluid into the blood and tissues when the outlet was practically closed. Even milk, therefore, desirable as it might be on other grounds, should be given only in small quantities; and of solid food none. On the other hand, when secretion had again begun, it might be eminently beneficial to prescribe milk and other mild fluids liberally. It was doubtful if at any stage of renal disease the stronger animal broths were even desirable. When the state of chronic disease was fairly established, he thought the most important thing to keep in mind was that the capacity of secreting the organic nitrogenous constituents of the urine was more or less, and often greatly, diminished. And it was perhaps specially important in the granular kidney. The patient may long be in comparatively vigorous health, able to enjoy, and perhaps digest, solid food. And all the while there might be a growing incapacity to throw off the waste products of metabolism, with consequent standing risk of accumulation and auto-intoxication. A liability to a uræmic attack in some form might thus arise, almost unexpectedly. As regards the prescription of a diet, under these circumstances, the important thing seemed to be to have regard to quantity quite as much as quality. Red meat might, in practice, be better than white meat, if less satisfied the appetite; and the same was true of the least stimulating articles, be it fish or eggs. When it is a question of satisfying the mere needs of the body, it had been clearly shown by the experiments of Chittenden, following up the teaching of Fletcher, that the average person could live comfortably on one-half or one-third of the quantity habitually consumed. Diet, to put it shortly, was more a question of quantity than of kind, on the supposition that there was a proportioning of its elements in respect of proteids and carbohydrates.

TWO CASES OF LEAD-POISONING IN CHILDREN.

By J. Macdonald Gill, M.D., Lond., Sydney.

Case I.—Robert H., 5 years of age, was admitted to the Children's Hospital, Sydney, on January 26th, 1905, and discharged on March 21th.

The history was as follows :—He had always been a healthy child. The present illness had lasted seventeen days, with pain and weakness in the back and limbs ; he could not bear to be touched, and lay in bed with his knees propped up. There was also a history of severe pain in the abdomen in the early part of the illness, probably colic, which had passed off. No history of lead-poisoning was obtained at first, but the mother afterwards found out how the poisoning took place. A wall at the back of their premises was being painted, when this boy was seen by the older children to suck his fingers after getting them covered with paint. The mother said it was brown, not green, paint. About four days after this took place he became ill with pains and weakness in the legs. On admission to the hospital the child looks dull and heavy, but does not cry out much unless touched. He lies in bed with his knees drawn up. The mental condition is good. He has had headaches, but none since admission.

Eyes.—The pupils are equal, and react to light and convergence. Has weakness in both external recti, but no ptosis. The optic discs are normal. Eyesight good. There is slight facial paresis on the left side, especially of the lower face muscles. The tongue is protruded in the middle line. The senses of smell, taste, and hearing are apparently normal. Speech is nasal in character, and articulation is defective, but deglutition is unimpaired ; the soft palate contracts fairly well when tickled.

Upper Limb.—There is partial wrist drop and inability to extend the fingers. The reflexes cannot be obtained. There is no special tenderness on pressure on the posterior interosseous nerve.

Lower Limb.—There is almost complete loss of power in the extensor muscles of the thigh and leg, so that the child lies in bed with the knees flexed, the ankle joints extended, and the toes flexed. The muscles are wasted and flabby. There was some anæsthesia up to about the middle of the thigh. The knee jerks are both absent ; the plantar reflexes are present, of the flexor type, though feeble. There is no tenderness on pressure on the muscles themselves ; but intense pain is caused by pressure on the great sciatic and popliteal nerves in each leg, behind the hip and knee joints, where the nerves are not covered by muscle. Similarly, when the nerves are put on the stretch, by fully extending the knee joint, or by flexing the hip with the knee extended, the child screams with pain. Such tenderness is not observed in the arms, probably owing partly to the deep course of the posterior interosseous nerve, and partly to the mildness of the affection there.

Bladder and Rectum.—There is almost complete loss of control over the bladder ; he has absolutely no control over the rectum, the sphincter ani being completely paralysed. The muscles of the back are very weak. The abdominal walls are retracted ; the diaphragm contracts well. Respiration is easy. The abdominal and cremasteric reflexes are present. There is no intestinal colic, the bowels being open daily without difficulty. The urine contains a trace of albumen. There are no diphtheria bacilli in the throat. There is no blue line on the gums, and his teeth are quite clean and free from tartar. On February 2nd it is noted that he is improving slowly, but that there is complete paralysis of the left external rectus, whereas the right is normal. The bladder reflex is normal, while the anal sphincter is still completely paralysed. While in the hospital he improved rapidly and continuously. On discharge it is noted that he can walk well, and has regained

most of the power in his arms and back. The muscles of the legs are much wasted, and the knee jerks still absent. The temperature remained normal throughout his stay in the hospital.

Case II.—Samuel Henry H., 10 years of age, was admitted to the Children's Hospital on April 3rd and discharged on April 17th, 1905.

History.—The boy was brought to Sydney for treatment from his home at Kempsey, on the Macleay River. Four years ago he had a fall on to the back of his head, which was followed by convulsions. Eight months later he had a severe febrile attack, with constipation : he then had convulsions for thirty-six hours, and recovered completely in four weeks. Two years ago he lost the use of all four limbs, and was in bed for six weeks ; he gradually recovered till he could run about. Four weeks ago the weakness appeared—in the hands especially. He has been pallid for some months. Their house was painted before the attack which took place two years ago. The bowels are regular. Has no headaches.

On Admission.—The boy is small for his age, and, intellectually, is very backward ; he is quite childish in his habits. The teeth are very dirty, and have a considerable deposit of tartar on them. There is a definite blue line on the gums. There is well-marked foot and wrist drop : the muscles are not completely paralysed, but are very weak. He can walk with his feet in position of talpes equinus. The knee jerks and other reflexes are present. The nervous system is otherwise normal.

The Urine.—Sp. gr. 1014—acid, contains a trace of albumen. During his short stay in the hospital, the boy remained in about the same condition ; he was discharged with directions to attend the out-door department, but did not come again.

Remarks.—Peripheral neuritis, due to lead-poisoning, in children, is of particular interest to us in Australia, and has become familiar to the readers of the *Gazette* from the writings of Drs. Lochart Gibson and Jefferies Turner, of Brisbane. There has been no satisfactory reason given for its rarity of occurrence in Sydney, as compared with its frequency in Brisbane. As far as I know, these cases are the first recorded in Sydney. Of course it might be thought that cases do occur, but pass unrecognised : that can hardly be the case, as peripheral neuritis (apart from post-diptheritic paralysis) is so unusual in children as to at once compel attention.

Taking the cases in detail, the diagnosis in Case I. was finally established by the detection of lead in the urine. When the question of lead-poisoning was raised, I asked Dr. Cooksey, of the Public Analyst's Department, Board of Health (Sydney), to examine the urine : this he very kindly consented to do, and found distinct traces of lead and copper, the copper being, perhaps, due to some green paint being mixed with the brown. However, that is a matter of no importance, as small quantities of copper are not poisonous. It was after this that we obtained the history from the mother. The rapid improvement in the hospital was very satisfactory, and has been noticed before in similar cases. Case II. was quite easy of diagnosis, and belongs to the more familiar type of this disease. In this case no lead was found in the urine ; probably owing to the case being a chronic one. How this boy got the lead was uncertain : perhaps the illness he suffered from two years before was lead neuritis ; if so, a similar explanation to that in the first case would suffice, more especially as the boy is but half-witted.

I have brought these cases before the profession, as it is of the highest importance to the little patients that the true nature of their complaint should be recognised. My thanks are due to Dr. Blackburn for his assistance in the examination of the first case, and to Dr. Plomley, Resident Medical Officer at the Children's Hospital, for the notes of both cases.

NOTES OF A CASE ILLUSTRATING THE SYMPTOMS OF TUMOR OF THE OCCIPITAL LOBES OF THE BRAIN.

By James Jamieson, M.D.

Of late years there has been a growing dissatisfaction with the doctrine of the existence of tolerant, or silent, areas in the brain. With that has come an increasing unwillingness to accept the notion of latent disease of structural or destructive kind affecting it. That there are regions comparatively tolerant of injury may be admitted, and that even large abscesses may form in the substance of the brain, with the production merely of symptoms of a very general kind, must of course also be admitted.

The white matter of the frontal, temporal, and occipital lobes has particularly been looked on as comparatively tolerant of disease or injury. But, with increasing exactness in clinical methods, and more careful observation of the grouping of symptoms, localising signs as regards even these areas have been formulated. There will probably never be such definiteness of knowledge about the exact location of destructive or irritative lesions of these regions as we have about those of the mid-brain, with its mapping out of motor areas. Experiments on animals, which have added so much to our knowledge of this part, cannot be expected to help us in a similar way with them. Helpful signs and symptoms there are—progressive changes of character or lessened power of judgment in tumors of the pro-frontal lobes, with perhaps development of aphasic conditions. Visual defects of a special kind may be very significant in those of the occipital lobe ; notably word-blindness, and hemianopsia. But these by themselves can only point to some great region being involved, failing perhaps to tell us, in the latter case, whether the lesion is superficial or deep-seated.

It must be by means of clinical records, supplemented by *post-mortem* examinations—and these in large accumulation—that more exact differentiation will be made possible.

The following case—though, unfortunately, defective in details of early symptoms—has considerable interest, and so suggestiveness.

M. R., female, single, aged 30, was admitted to the Alfred Hospital on July 19th, 1904, with a rather vague history of increasing impairment of vision and loss of power, especially in the legs. She had been ill for three or four months, but had been confined to bed for about one month only. She was very ignorant and unobservant, and the same was true of any of her friends who could be questioned, so that details about the onset and early symptoms could not be got. There had not been vomiting, but she had been losing flesh. She could only say that for a month her sight had been very bad, and when examined it was found that she could not with either eye see fingers held up before her. When asked about headache she put her hand on the post parietal region on the right side as the seat of it.

Nothing of importance could be learned of the family or personal history. She gave no account of any previous illness, and there was no evidence of syphilis. There had been amenorrhœa for three months ; the pulse was 99, respiration normal, temperature 97. The urine was acid, and free from albumen. The apex beat was in the normal position, and there was no sign of valvular disease or arterial change. The pulse tension was 124, and the red blood count and hæmoglobin measurement were but slightly below normal. There was no sign of lung disease, and the abdominal organs seemed to be healthy. The knee jerks were both exaggerated, but there was no ankle clonus, and the plantar reflex was active, but of normal character.

Her eyes were examined by Dr. Gault, ophthalmic surgeon to the hospital, who reported that the media were clear and the discs normal. Previously to this it had been found that the pupils did not react to light, and it was also shown that there was external ophthalmoplegia, considerable in range though not very complete. Notably there was paresis of the external rectus, double, but most distinct on the right; and there was loss of power to roll the eyeballs up, especially on the left side. There was also apparent drooping of the upper lids; but these facts were ascertained with great labor and difficulty owing in part to the blindness and in part to the mental condition.

The loss of power in the limbs became steadily greater, and the leg reflexes became gradually less distinct till the knee jerk on the right side was lost, and that on the left obtainable only on reinforcement. The plantar reflex remained normal in character but became less active, especially on the left side. The loss of power also became steadily greater, and most in the legs—though the hand-grip was much weakened, and most on the right. Towards the end there came to be some right facial paresis, and Kernig's sign was found to be present on both sides. Her eyes were again examined on the 8th of August, with the result that both discs were still normal.

She died on August 10th, having become deeply unconscious on to coma.

An attempt to interpret the connection of these multiform symptoms, and to decide on the nature and seat of the lesion producing them, led to much discussion. The widespread paralytic condition associated with complete blindness—and that blindness not due to any retinal change, no atrophy or retinitis, no choked disc or œdema—made a somewhat unusual combination. And there were also the important features of absence of vomiting and fever, and of any sign or history of syphilis or of circulatory disease.

The steady progression of symptoms, without any sudden onset or rapid aggravation, corresponded best with the growth of some tumor, though the course was somewhat short, and two out of the three so-called cardinal signs of cerebral tumor were lacking. Blindness, of course, took the place of the usual retinitis, but it was manifestly blindness of some unusual kind. This blindness had to find some reasonable explanation on any supposition as to the pathological condition. It might be accounted for by some destructive pressure at the chiasma; by some bilateral lesion affecting the visual centres in the occipital cortex; or by some lesion cutting both optic tracts in their course between these centres and the optic nerves. For any lesion seated at the base and pressing on both optic nerves, and also implicating both pyramidal tracts and various cranial nerves, to be accepted as the cause was almost inconceivable in the absence of retinal congestion and other changes, and of bulbar or severe neuralgic symptoms—so apt to mark the growth of basal tumors.

The possibility of meningitis exerting destructive pressure on both visual centres and extending forward along the base was suggested by the presence of Kernig's sign. But useful as that sign sometimes is, it is not all pathognomonic of meningitis—and, indeed, is met with in cases of cerebral hemiplegia with various modes of causation. But a meningitis going on steadily, as this condition did, without rise of temperature, rigidity of the neck, or almost any of the ordinary signs of the disease, was not an acceptable solution of the problem.

The existence of a very large abscess, or of an abscess in each hemisphere, was excluded by the absence of fever or any sign of sepsis, even if there had been any cause discovered from which cerebral suppuration could have been supposed to take origin. No abscess could have lasted so long while also growing steadily—almost rapidly. We seemed to be left with a tumor of

some kind, as best in accord with all the conditions. It almost certainly must have begun on one side—the left, near the middle line, growing both laterally and forward so as to cut the optic tracts and gradually involve the pyramidal tracts. On this supposition the blindness was regarded as being really a double hemianopsia.

The necessarily large size of the tumor thus postulated made the absence of retinal changes difficult to understand, though their very absence was in a way helpful in suggestion as to the nature of the blindness. The range of implication of cranial nerves, if this tumor was the sole cause, was also a difficulty. But brain pathology has not yet been so simplified that every symptom, in a complex group of this kind, can easily find full explanation.

Post-mortem examination revealed the substantial correctness of the diagnosis. There was difficulty in extracting the brain, the anterior aspect of the basal portion of which was firmly adherent to the dura mater, and the dura to the bone. There was found to be a soft, almost fluctuant, mass of gliomatous nature, involving the origin of the Sylvian fissure and extending backwards along the basal aspect of the temporo-sphenoidal lobe. It involved the left optic tract, and stretched across so as to impinge also on the right tract. The pons presented a curious white gelatinous layer immediately under the pia and firmly adherent to the pontine substance. On section the soft tumor was seen to extend back into the left lateral ventricle, not involving the internal capsule or the great basal ganglia, but just impinging on the external capsule and the claustrum. The ascending parietal region was slightly encroached on.

The condition found on the surface of the pons may be taken as accounting for the bilateral paresis of the external muscles of the eyeball, and possibly for the appearance of Kernig's sign. The reason for the absence of retinal signs with such a large tumor most probably was to be found in its softness, and in the fact that it grew rather by incorporation than by pressure displacement of the structures in its path.

A case of tumor of the centrum ovale, presenting some likeness to that here recorded, was reported in the *British Medical Journal*, May 21st, 1904. The special points made were the wide range of symptoms produced and the absence of optic neuritis.

PRELIMINARY COMMUNICATION ON THE NO-FOOD TREATMENT OF TYPHOID FEVER.

By O. L. M. Abramowski, M.D., Berlin, Mildura, Victoria.

Mildura, situated on the River Murray, in Victoria, is well known over the whole Commonwealth as an irrigation settlement, producing the finest crops of fruit in Australia. But there is one crop in Mildura produced annually, from November to May, which is only known to the local residents and to the Department of Public Health in Victoria: I mean the crop of Typhoid Fever. This made its appearance soon after the first people erected their tents on the bank of the river in 1888, and has been with us ever since. Up to January, 1903, 270 cases of this disease have been registered, 35 of which ended fatally —a death-rate of 13 per cent.; 175 of these cases were treated in the local hospital, with a death-rate of 25, or 14 per cent.—being, on the average, the

more serious cases. The causes of death were—Paralysis of the heart, 33 per cent.; perforation and peritonitis, 25 per cent.; hæmorrhage, 19 per cent.; congestion of lungs, 13 per cent.; congestion of kidneys, 5 per cent.; pneumonia, 5 per cent.

Since February, 1903, I treated 63 cases of typhoid fever—42 in the hospital and 21 in private practice—without one death. According to the previous death-rate this means a saving of more than eight lives. They were all typical cases of our own local typhoid, with the same symptoms as the previous cases. The diagnosis was not confirmed by Widal's test, and only in two cases by the Diazo-reaction; but roseola appeared in 48 cases; the typical fever with recrudescences, &c., in 10 cases, hæmorrhage from the bowels in 3, and relapses in 2: so that there can be no doubt about the true nature of the affection.

The triennial death-rate from typhoid in Mildura is as follows:—1891 to 1893, nine; 1894-6, eight; 1897-9, nine; 1900-02, nine; 1903, January, two; 1903, February, to 1905, January, none.

The general death rate in the local hospital was, for the year 1900-1, 14 per cent.; 1901-2, 15 per cent.; 1902-3, 12 per cent.; 1903-4, 8.3 per cent.; 1904-5, 7 per cent.; and in my last report (July A.C.) I attributed this reduction to the elimination of typhoid as a cause of death! This astonishing, and, as far as I know, quite unique result, was caused through the no-food treatment.

Every year, since I passed my medical examination in 1875, I have been treating typhoid patients, and with every patient my anxiety was renewed regarding his diet. We all know that the intestinal lesions and the tendency to diarrhœa, hæmorrhage, and perforation of the bowels in typhoid call for the most careful dietetic *regime*, in order to preserve, as far as possible, intestinal antisepsis. It is clear that as little unabsorbed food as possible should be allowed to accumulate in the intestine, there to undergo fermentation and decomposition. With fever patients the question is almost uppermost—Can they digest and assimilate any food? The great emaciation of typhoid patients who had been fed with the most nourishing diet always seems to prove to me that their food was absolutely wasted. When, in 1902, I read the admirable work of Pawlow on "The Work of the Digestive Glands," it became clear to me that feverish patients cannot digest and absorb food. Pawlow's extremely interesting experiments prove beyond a doubt the correctness of his contentions—that the appetite is the first and mightiest exciter of the secretory nerves of the salivary glands and of the gastric glands; that the work of any section of the alimentary canal is connected with and dependent upon that of the previous segment; and that the saliva is necessary, not only for mouth-digestion, but also for stomach-digestion; that without saliva there will be no production of gastric juice, without gastric juice there will be no secretion of pancreatic juice: consequently, without saliva there will be no digestion and no assimilation of the food.

Has our medical practice acted upon these principles? Decidedly not. On the contrary, we have usually acted in direct opposition to these observations by forcing our patients to eat when mouth and tongue were dry, and when the most appetising dishes could not make their mouths water. We have fed our patients in order to keep their strength up, under the mistaken impression that a fast of a few days means risking the patient's life. At the same time we did not reckon the cost on strength and energy incurred by the body's endeavor to push on the undigested food, put into an unwilling stomach, and to get rid of a fermenting and decomposing mass, producing poisonous substances through the whole length of the alimentary canal. The latest publications with regard to diet in typhoid advise even a more generous fare

than has so far been considered correct, and try to ignore altogether those physiological truths proved by Pawlow. At any rate, Pawlow's experiments convinced me that the untoward accidents observed in typhoid patients under every diet are the absolutely natural consequences of forced feeding, *i.e.*, of the feeding in the absence of the first and mightiest factor in digestion, viz., the appetite juice. This conviction was the basis of the no-food treatment of typhoid; and when, in December, 1902, our typhoid epidemic began, I decided to put it into practice. However, it took me some time to overcome my own prejudices, and during December, 1902, and January, 1903, I treated ten typhoids with abstinence from food for from eight to ten days only. I observed a decided diminution of dangerous and distressing symptoms; but I had not the courage to refuse food until the natural appetite, with sufficient production of saliva, appeared; and I lost two of these cases—one a boy of 11, on the seventeenth day, with pneumonia; the other a man of 37, weakened through abuse of alcohol, on the twenty-eighth day, with heart failure. At this time I read about a book by Dr. E. H. Dewey, entitled "The True Science of Living," but could not get it in Australia, and had to order it from America. Before the book came to hand I had carried my theory into full practice, and had treated twenty-two cases of typhoid with total abstinence from food until they produced enough saliva to properly masticate and enjoy a hard dry cabin biscuit. Great was my satisfaction when I found that Dr. Dewey had come to the same conclusion as I, although starting from other premises; and his success in typhoid treatment encouraged me to go on and to perfect my own treatment. I found Dewey's observations correct, that after three or four days of enforced fasting—that is, after the habit-hunger had been overcome—there was usually no desire for food at all: but that an unmistakable hunger appeared with the saliva, usually at the end of the pyrexia. Sometimes this desire for food was delayed after the temperature got normal and the tongue got moist, and even after the patient had left the bed. Thus, in case 17, the patient fasted—because his hunger did not appear earlier—for fifty-one days; eleven days beyond the pyrexia. Number 25 was up and doing clerical work for three days before he started eating on the twenty-first day; and number 26 was walking about and doing little jobs about the place for five days before he could chew and enjoy the biscuit, after a fast of twenty-four days. I have added a few tables giving the highest temperature, the duration of pyrexia, of fasting, of convalescence, and of the whole time of observation and remarks, re-complications, &c., in every case; and shall here only enumerate the general results of these statistics and the most prominent complications, &c.

The temperature was usually highest on admission, or at the beginning of the treatment. In the hospital patients it never rose to this height again in twenty-four cases; in twelve cases it was highest on the sixth day; in five cases only it rose above the initial height after the sixth day. The private patients, being always the milder cases, had all their highest temperature on the first day of observation.

The length of pyrexia amongst the hospital patients averaged 21 days: amongst private patients, 11.2 days; amongst the whole number, 18 days. The usual evening temperature was from 101° to 102°.6 amongst all the patients. Amongst the hospital patients it reached 102° in nine cases, from 102° to 103° in twelve cases, from 103° to 104° in fifteen cases, 104° and over in six cases: and all these higher temperatures were only observed on a few days—usually on the first days of treatment. The temperatures of the private patients were not regularly registered, but they were considerably lower. The time of fasting was, on the average, in the hospital 22 days, amongst private patients 11.2 days; giving a total of 18.3 days. The average duration of the convalescence was 20 days in the hospital, 10.8 days in private practice:

or, amongst the whole number, 16.5 days. The average duration of the whole case—that is, from the beginning of the treatment until the patient could resume light work—was, amongst hospital patients 41 days, amongst private patients 22 days; a total average of 34.5 days, or about five weeks. Complications were very few and mild. Perforation, usually observed in about 4 per cent. of all cases, was totally absent; hæmorrhage was seen only in three cases; delirium of short duration in four cases; recrudescence of fever in four cases; relapses in three cases; diarrhœa in four; sleeplessness and headache in three; abscesses and furuncles in three; albuminuria in two; meningitis in one; neuralgic pains in one; and double suppurating parotitis in one case. All the other complications were conspicuous by their absence; besides perforation we never saw vomiting, meteorismus, singultus, pneumonia, or bronchitis. Especially grateful was I for the complete absence of thrombosis, which, in years past, had crippled a great number of our patients.

The usual remarks in the case-book were: Patient slept well, feels comfortable, easy and cheerful, sensorium quite free, reading, &c.; and this in the second and third week, when under ordinary treatment, the typhoid patients were usually delirious, flushed, and restless. Ten of the hospital cases—those marked with a cross on the tables—were so severe and so protracted, and showed so little power of resistance, that under usual treatment they would have probably died.

The no-food treatment which proved so successful in Mildura during the last three years does not consist in the abstinence from food only. The patients were allowed pure sterilised water as much as they wanted; and it was astonishing how little some would drink, not taking more than one or two pints a day, whereas others—usually great salt eaters—would drink from five to six quarts in a night. Every day in the beginning, and every second day after the motions had lost their smell, the patients got an enema of sterilized water of about 90°: this removed some fæcal matter almost every time; the fæces soon lost every trace of diarrhoic or pea soup characteristics, and took the shape of round or oblong masses of the size of a marble or a walnut, and almost free from any smell, and of a greyish-brown color. Even after the fast of fifty-one days these excretions continued to appear at intervals of from three to five days. In four cases, after a fast of from fourteen, sixteen, twenty, or twenty-four days, respectively, and regular flushings of the colon, a teaspoonful of castor oil removed big motions—once nearly a chamberful. This brought in every case a recrudescence of the fever, of short duration. Excepting a few doses of acetozone in the first few cases, a few cold baths and spongings during high pyrexia, and an occasional dose of castor oil (as above), no other treatment was resorted to. Drugs or alcohol were not given—excepting a few doses of 15 min. of Tt. Opii in the three cases of hæmorrhage; but, in my present opinion, even that is not needed as long as the digestive canal is empty. The cases of recrudescence, or relapse, cheerfully submitted to another fast as soon as the fever re-appeared, and never gave any further trouble.

Delirium, headache, sleeplessness, usually subsided after the first three or four days. Only in two cases of a very prolonged pyrexia (numbers 20 and 32) the delirium lasted in a mild form three and four weeks, respectively. These two cases—of which one was a very frail hysterical woman, and the other was complicated with suppurating double parotitis, and both of which developed abcesses in the fifth and eighth weeks, respectively—would surely have succumbed under any other treatment. All the other complications, such as diarrhœa, albuminuria, meningitis, furunculosis, disappeared without any further interference, medicinally or dietetically.

The convalescence in every case was uninterrupted, and there was never any trace of that protracted invalidism we so often see after the usual treatment. When once these patients started to eat, they made a marvellous recovery. It seemed that after the fast the system is in such a splendid condition for assimilation of food that every morsel given is made use of to its utmost capacity. The feeding usually started with fruit and fruit juices, oranges, bananas, grapes, followed by milk, biscuits, triscuits and shredded wheat, porridge, puddings, and eggs. Bread, meat, and vegetables were usually the last addition to the diet. All the convalescents had a good appetite ; there was no picking or daintiness about the food, and the increase in weight and strength was rapid. As for the loss of weight through the prolonged fast, I am sorry I was not able to ascertain it on the scales in more than three cases : the first, a very fat man of fourteen stone, reduced his weight by four stone after twenty-eight days' fasting ; the second lost two stone after twenty-four days' fasting ; the third twenty pounds after twenty-one days' fasting. Generally speaking, these convalescents were not thinner, but certainly much stronger and heartier than all those treated before by me.

The most pleasing feature of these fasting patients was the absence of the usual urgent and distressing mental and bodily symptoms ; so that, in a ward full with cases of typhoid in the severest stage, there was no delirium, no mutterings or carpholagia, no cough, no tympanitis, no diarrhœa or involuntary discharge from the bowels or bladder, no decubitus or restlessness ; the tongue was usually clean, and, to a certain extent, moist ; there were no sordes on lips or teeth. During the night, and very often during the day, the patients would sleep quietly and peacefully, like children, so that the duties of the night-nurse were very light indeed.

Before the merits of the no-food treatment were known amongst the people it took sometimes a certain amount of persuasion to induce the patients to submit to the fasting : but even then they complained about hunger only for two or three days. After that they found the water quite sufficient for their wants, and would gladly acknowledge how much better they felt now than whilst taking food, before they had my advice. If after a week or so their faith began to flag, and they expressed the opinion that a little food would not hurt them, I did not absolutely decline, but would allow them a small quantity of milk or plasmon, and wait for the result. At the afternoon visit their temperature would be high, the pulse rapid, the tongue dry, the mouth nasty and sticky, stomach and bowels distended and disagreeable. After this it did not need any further persuasion to return to the water diet. With some patients we had two or three trials like that ; but the effect of even the slightest dose of food was so sure and so quick to raise the temperature and render the patient uncomfortable that we now know to a certainty " that feeding keeps up fever."

Infants are the easiest to treat in this way, as they will be quite satisfied with water as long as they are feverish. But children used to three meals or more a day will give you very little peace ; and, until you have the hearty co-operation of parents or friends, it is a hard task to constantly refuse the little sufferers what they seem to want so apparently. But when after a few days the temperature is low, and a trial is made with the lightest food, the effect is so palpable in rendering the patient worse that the parents soon get convinced ; and after a second trial I always had them in favor of water against food, and the further progress of the case would be undisturbed.

The no-food treatment has robbed typhoid of most of its horrors, has reduced the time of unfitness to work from the usual three months to about five or six weeks, has not only secured my patients a comparatively easy time,

but has made patients and nurses, parents and friends, during the whole time, so cheerful and free from anxiety that it always was a great pleasure and satisfaction for me to be with them and to encourage them just for a day longer of fasting and water. I did not think it wise in the beginning to frighten the patients by telling them that they might have to fast from two to four weeks or more; and the gradual extension from day to day they did not seem to mind so much.

At the present time, since the beneficial effects of the no-food treatment have become known through the district, people in Mildura do not dread typhoid any longer, and willingly undergo the fasting; but when first I started the treatment, I had not only the resistance of the patients and their friends, but also the prejudices of the nursing staff to overcome; and many a lecture I had to deliver inside the ward and outside of it on the benefit of keeping an ulcerated intestinal canal free from any fermenting and dangerous substance.

The number of cases treated by me with abstinence from food has not been as large as I would have wished, and my observations are far from being conclusive: but the subject is of such great importance to medical men, and humanity generally, that I did not feel justified in withholding my experience until I could put a greater number of cases before the profession.

The difference between the usual style of feeding and this no-food treatment is almost incredible to anyone who has not tried it, and I therefore beg to place these short and incomplete observations before the profession of Australasia, hoping that they will soon be verified by the experience of others, and that the no-food treatment will prove in their hands the same blessing for suffering humanity as it has proved in mine.

SUMMARY OF A SERIES OF 63 CASES OF TYPHOID UNDER THE NO-FOOD TREATMENT.

	Average Duration (in Days) of—												Remarks.
	Pyrexia.			Fasting.			Convalescence.			Total Observations.			
	Male.	Female.	Total.	Male.	Female.	Total.	Male.	Female.	Total.	Male.	Female.	Total.	
Hospital patients	19·5	25	21	20	25	22	19	20	20	38	45	41	Severe cases
Private patients	10·7	12·2	11·2	10·7	12·2	11·2	11·1	11·4	10·8	21·8	22·6	22	Light cases
Total ..	16·5	21·8	18	16·8	21·8	18·3	16	15·1	16·5	32·4	37·9	34·5	

Diagnosis confirmed by roseola in 48 cases; typical fever and hæmorrhages in 2 cases; typical fever and recrudescences in 11 cases; typical fever and relapses in 2 cases.

Highest temperature (hospital cases only): On admission in 24 cases, before sixth day in 12 cases, on and after sixth day in 5 cases; up to 102° in 9 cases, 102°.1 to 103° in 12 cases, 103°.1 to 104° in 15 cases, more than 104° in 6 cases.

Complications: Delirium, diarrhœa, recrudescence in 4 cases each; relapse, hæmorrhage, headache, sleeplessness in 3 cases each; albuminuria and furunculosis in 2 cases each; abscesses, neuralgic pains, parotitis, duplex suppurat in 1 case each.

Recovery in 63 cases; death in none.

A SERIES OF 63 CONSECUTIVE CASES OF TYPHOID UNDER THE NO-FOOD TREATMENT.

No.	Name.	Age.	Diagnostic Symptoms.	Temperature, Highest.	Duration (in Days) of— Pyrexia.	Fast.	Convalescence.	Stay in Hospital.	Date of— Admission.	Discharge.	Remarks.—Complications.
					Hospital.—Males.						
1	B. D.	37	Roseola	101·5° on admission..	13	14	18	32	31–1–03	4–3–03	Normal course
2	A. McK.	45	"	102·8° " ..	16	19	5	21	16–2–03	9–3–03	"
3	H. J.	20	Typical fever	102·0° " ..	18	18	8	26	17–3–03	4–4–03	Short relapse after 4 days normal
4	J. H.	35	Roseola	102·5° " ..	15	14	7	22	3–4–03	25–4–03	Normal
5	W. C.	29	"	102 3°	19	19	3	22	3–4–03	25–4–03	Sleeplessness and headache on admission for 5 days; bilious subject
6	A. M.	23	"	102·2°	14	14	4	18	6–5–03	24–5–03	Normal
7*	Ch. B.	18	Typical fever	103·6° on 4th day....	33	33	39	72	28–4–03	9–7–03	Hæmorrhage on 11th and 13th days; mother and sister have died of hæmorrhage in typhoid
8	G. A.	27	Roseola	104·0° on 3rd day....	17	17	16	33	4–5–03	6–6–03	Milk and water after 8 days increased fever and gave pain in stomach
9	R. R.	31	Typical fever	102·0° on admission..	32	23	29	61	9–5–03	9–7–03	Relapse 9 days after normal, with albuminuria
10	H. H.	20	Roseola; spleen	102·6° on 2nd day ..	9	10	21	31	4–12–03	4–1–04	Diarrhœa on admission, with very foul peasoup stools
11*	S. B.	25	Roseola; headache; epistaxis	103·4° on 20th day ..	27	35	33	68	21–12–03	27–2–04	Recrudescence on 17th day, after dose of castor oil
12	E. B.	36	Typical fever; pronounced smell	103·0° on ᶠth day....	22	20	12	34	7–1–04	10–2–04	Albuminuria disappeared with fever
13	S. W.	22	Roseola	103·4° on admission..	16	20	10	30	28–1–04	27–2–04	Normal
14	T. C.	33	"	104·8° " ..	12	15	13	28	5–2–04	5–3–04	Delirious first 3 days: after that always rational
15	A. G.	25	"	103·2° on 2nd day ..	9	12	6	18	15–2–04	5–3–04	Headache; characteristic smell
16	F. L.	9	"	103·4° on admission..	8	4	1	9	17–2–04	26–2–04	
17	D. F.	45	"	103·6° on 4th day....	40	51	17	57	17–2–04	13–4–04	Recrudescence on the 21st day; very fat stout man; walking, reading, writing 14 days before eating
18	G. G.	19	"	103·0° on admission..	8	8	25	33	21–2–04	27–3–04	Ill 14 days before admission
19	M. Y.	41	"	102·0° " ..	36	36	14	50	24–2–04	15–4–04	Recrudescence after castor oil on 22nd day
20*	H. N.	15	"	104·8° on 2nd day ..	36	36	70	106	9–3–04	23–6–04	Parotitis duplex suppurat; abscesses on back; delirious at times

21	R. D.	24	"	104·8° on 16th day ..	36	36	31	67	12-3-04	18-5-04	Rigor on 16th day, followed by diarrhœa, which ceased after 10 days, spontaneously
22	J. McL.	24	"	102·0° on 4th day....	13	13	11	24	20-3-04	13-4-04	
23	W. H.	25	"	101·6° on admission..	14	14	9	23	21-3-04	15-4-04	
24	A. St.	14	"	102·4° " ..	14	11	19	33	15-12-04	17-1-05	
25	W. E.	30	"	103·4° on 6th day....	18	21	16	34	25-1-05	28-2-05	On admission, sleepless; headache for 4 days; doing clerical work before eating
26	Th. McD.	50	Roseola; diazo-reaction	102·6° on 3rd day....	12	24	23	47	31-5-05	18-7-05	16th day, cannot eat biscuit; teeth sore. 24th day, never enjoyed anything like it; walked about before eating; lost 4 stone
				FEMALES.							
27*	M. McPh.	23	Typical fever; hæmorrhage	103·0° on 3 occasions.	31	30	21	52	21-2-03	14-4-03	Sleeplessness; hæmorrhage 13th, 15th, 17th days
28*	M. L.	26	Typical fever	104·0° on admission..	52	32	24	76	17-3-03	1-6-03	Two recrudescences after attempts to eat; very weak woman, anæmic
29	E. O.	16	"	103·5° " ..	36	36	25	61	21-4-03	21-6-03	Two recrudescences after attempts to eat
30	L. Cl.	48	Roseola	101·5° on 3rd day....	15	22	21	43	6-6-03	19-7-03	Comfortable while fasting; uncomfortable with barley water
31	N. B.	22	Roseola; pea-soup stools	103·6° "	22	22	25	47	8-7-03	22-8-03	On admission, diarrhœa, lasted 6 days; 18th day, milk and water; increased temperature
32*	E. B.	29	Headache; epistaxis; typical fever	†104·5° on 15th day ..	35	24	31	66	2-2-04	9-4-04	Insomnia; hysterical; retention urine; abscesses on head
33*	M. H.	8	Roseola	103·6° on 6th day....	24	24	18	42	8-4-04	20-5-04	Diarrhœa for 7 days; stopped through fasting
34*	L. M.	18	"	104·0° on 4th day....	20	18	23	43	11-5-04	23-6-04	Very fat; tremulous on admission, and delirious for 3 days
35	A. C.	5	"	102·2° on admission..	5	4	4	9	12-5-04	20-5-04	Was ill some time before admission
36	M. St.	17	"	103·0° on 10th day ..	20	20	10	30	15-12-04	8-2-05	Headache; epistaxis
37	M. M.	9	"	103·0° on admission..	7	7	24	31	24-1-05	23-2-05	
38	M. G.	28	"	104·2° " ..	30	35	22	57	31-1-05	28-3-05	19th day, castor oil; very big motion; recrudescence
39	F. C.	7	"	104·0° on 3rd day....	28	29	28	57	25-2-05	23-4-05	Boils on head
40*	L. B.	17	"	104·8° on admission..	22	23	28	51	3-3-05	23-4-05	Fat; delirious first 5 days; hæmorrhage on 16th day, small
41*	Ph. A.	5	"	104·0° " ..	27	26	13	40	14-3-05	23-4-05	Meningitis
42	E. B.	25	Roseola & diazo-reaction	103·6° " ..	26	24	21	47	2-6-05	19-7-05	Three eruptions of roseola; always hungry; bad smell

* Would probably have died under ordinary treatment. † Extremely hot weather, 114°.

Cases of Typhoid Treated under the No-food Treatment—continued.

No.	Name.	Age.	Diagnosis Based on—	Highest Temperature.	Days of— Pyrexia.	Fast.	Conva-lescence.	Observa-tion.	Date of Beginning of Observation.	Date of End of Observation.	Remarks.
				PRIVATE.—MALES.							
43	A. F.	45	Roseola	103·4° after eating ..	14	14	9	23	14–2–03	9–3–03	On 9th day and 14th day temperature rose after taking some food: fell again by fasting
44	M. H.	19	"	103·0° on 1st day....	10	10	10	20	23–2–03	15–3–03	
45	G. McN.	28	"	102·5° "	7	7	7	14	18–4–03	2–5–03	Had typhoid 10 years ago
46	W. E.	34	"	103·0° "	7	7	6	13	21–4–03	4–5–03	
47	R. N.	34	Typical fever	103·2° "	10	12	11	21	4–5–03	25–5–03	Had typhoid 10 years ago
48	W. M.	27	Roseola	103·0° "	8	8	4	12	27–1–03	8–2–03	" "
49	F. E.	40	Typical fever; smell	102·5° "	10	10	6	16	1–12–03	15–12–03	Lives in house of 59
50	H. W.	21	Roseola	102·6° "	14	14	17	31	5–12–03	5–1–04	
51	W. M.	44	"	103·3° "	15	15	27	42	12–2–04	26–3–04	
52	R. V.	50	Typical fever	103·2° "	12	12	16	28	16–2–04	15–3–04	
53	Ch. C.	15	Roseola	103·4° "	12	12	24	36	7–4–04	12–5–04	Brother of 55
54	J. M.	8	"	103·0° "	10	10	11	21	16–1–04	6–2–04	
55	Cl. C.	10	Typical fever	102·5° "	10	10	7	17	21–6–04	8–7–04	Brother of 53
				FEMALES.							
56	E. L.	10	Roseola	103·2° "	12	12	5	17	28–4–04	14–5–04	
57	M. C.	5	"	103·5° "	14	14	13	27	29–4–04	25–5–04	Temperature rose every time after taking food first week
58	L. D.	13	"	102·7° "	10	10	15	25	28–5–04	21–6–04	
59	S. M.	45	Typical fever; smell	103·0° "	14	14	5	19	18–1–04	5–2–04	Lives in same house as 49
60	N. M.	18	Roseola	103·4° "	15	15	6	21	31–1–04	20–2–05	Daughter of 59
61	E. B.	38	"	102·6° "	12	12	14	26	15–2–04	13–3–04	
62	S. T.	14	Typical fever	102·5° "	7	7	17	24	16–2–04	12–3–04	
63	M. A.	13	"	103·6° "	14	14	8	22	28–12–04	19–1–05	

THE NO-FOOD TREATMENT OF TYPHOID.

Dr. Springthorpe remarked that Dr. Abramowski's results, if accurately recorded, showed what the human frame could resist. Hitherto he had regarded forty days' fasting as illustrating either miracle or mal-observation. It was unfortunate that there was insufficient proof that the cases treated were really typhoidal. No doubt many cases used to receive too much food, but it was illogical to suppose that if food could be given without any mal-assimilation, as it could, the system was better without it, and it was notorious that the patient's desire was by no means a safe guide as to quantity and quality. It was unwise to use the word "impossible" outside mathematics, but it was scarcely possible to regard Dr. Abramowski's paper as conclusive.

IS THERE SUCH A DISEASE AS CROUP?

By Harvey E. Astles, M.D., F.R.C.P.E.

I have written this short paper chiefly to show the different opinions that exist on this subject, and to endeavor to arrive at a satisfactory conclusion.

Mr. Edmund Owen, Surgeon to the Great Ormond Street Children's Hospital, London, writes as follows:—Croup, diphtheria, and membranous laryngitis are forms of the same disease.

Dr. Osler, late Professor of Medicine at the Johns Hopkins University, and now Regius Professor of Medicine at Oxford, states, in his "Practice of Medicine":—That membranous croup is either genuine diphtheria or diphtheroid in character; and, in 286 cases in which the disease was confined to the larnyx or bronchi, 229 Klebs Loeffler's bacilli were found. It is to be regretted that further particulars of the fifty-seven cases in which it was not found had not been given.

Dr. Foord Caiger, in Allchin's "Practice of Medicine," speaking of the extreme difficulty of diagnosis, says:—It practically resolves itself, at the present, into whether or not the Klebs Loeffler bacillus can be detected in the discharge.

Dr. Robert Macguire affirms, in his article in "Quain's Dictionary of Medicine":—There remains still a certain proportion of cases of membranous laryngitis which are not due to the actions of diphtheritic poison.

Hilton Fogge believed in non-contagious membranous croup.

Whitlaw is of opinion that different writers have described two totally different affections under one name. Dr. Whitlaw's position as a physician to the Belfast Hospital and Lecturer on Clinical Medicine, together with his reputation as a physician and author, enables him to speak with authority when he says the affection known as true croup in the north of Ireland—and which presents the chief clinical features described by French and English physicians—is not diphtheria, but a non-contagious acute inflammation of the air passage.

The pathology of croup is a congested condition of the blood vessels, producing a swollen mucous membrane with the inflammatory exudation, which may or may not form a false membrane.

Dr. Lazarus Barlow, in his "Manual of General Pathology," touches at the very root of the matter. He says: "Now, in the neighborhood of the pharynx and larynx two classes of cases have long been known, in which inflammation is accompanied by the formation of a membrane. In the one, croup, the membrane is superficial; in the other, diphtheria, it is interstitial. Either condition produces more or less laryngeal stenosis, with hoarseness of voice

and continued difficulty of breathing. In croup we have an inflammatory disease, non-contagious, with little or no tendency to complications, nor is it followed by paralysis.

Before the days of Loeffler bacillus, I can remember many cases when, by the aid of emetics, complete casts of the trachea were brought up. These casts were then diagnosed from diphtheria by the comparative facility with which they were detached and the thinness of the membrane itself, together with the absence of membrane from the tonsils and pharynx: no undue swelling of the glands, the freedom of the urine from albumen, no history of contagion, nor any infection arising from cases diagnosed as croup. Nor were there precautions taken to prevent it.

The following is a very fair example of a case of croup. I was called in to see a boy of 7 years of age. His mother stated that he had been ill for three days, suffering from croup: she said he was subject to it; but on former occasions, after a bath warm, a hot poultice to the throat, and ipecacuanha wine, he soon recovered. On this occasion it had not given way to the usual treatment. He was living in a low-lying locality near the Swan River, and had a temperature of 102°, voice hoarse, and continued bad breathing, with great recession of the thoracic wall. As far as could be seen the throat was clear of false membrane, but the mucous membrane looked congested, and was exuding a good deal of mucus. There was no glandular swelling, no albumen, or any history of contagion. I am unable to say whether there was a false membrane in the larynx or trachea; but the stenosis was bad enough for it to be there. I ordered him iodide of potassium, aconite, and ipecacuanha. The hot poultices to be continued, and to breathe through a sponge wrung out of hot water. This boy made a good recovery; and although there were other children in the house who had frequent access to the room, not the slightest sign of illness occurred amongst them.

A short time ago I was called in to see two little girls (sisters), who were suffering from what I diagnosed as follicular tonsilitis. They made a rapid recovery. A few days afterwards an infant, fifteen months old (one of a family of five), was taken ill in the same house. There was extreme difficulty of breathing, a hoarse voice, and rise of temperature. There was no swelling of the glands, no false membrane on tonsils or pharynx; but the stenosis was sufficiently grave for the probability of its existence lower down. I stated the case to be one of croup. In follicular tonsilitis its etiology is put down to either insanitary surroundings, a damp situation, or a wetting. This house was situated in a damp locality, but otherwise its sanitary arrangements were good. The infant made a good recovery, and no case of illness occurred with the other four children, nor was it followed by any further outbreak in the neighborhood. I am of opinion that there are undoubtedly localities where croup is seldom or ever seen, and I believe these places have given birth to the belief that true croup is either genuine diphtheria or diphtheroid in character. In localities where croup is prevalent practitioners, as a rule, become expert in their differential diagnosis.

The name Croup—which is derived from the Saxon Kroopan, "to cry aloud"—is certainly misleading, and is a better appellation for laryngismus stridulus.

Dr. Dawson Williams, in his work on "Diseases of Infancy and Childhood," speaks of croup as acute laryngitis. The symptoms, he says, are identical with those of laryngeal diphtheria, and it is often impossible to be certain that laryngitis is not diphtheria. I would say, if there is a doubt, certainly use anti-toxin as well as your croup remedies.

It is, to my mind, an important matter that this subject should be cleared up—especially for medical students, who, when qualified, should commence practice with as clear a conception of these diseases as possible.

ON LEUKÆMIA AND ALLIED CONDITIONS IN CHILDREN.

By J. M. Gill, M.D., Lond., Sydney.

Leukæmia in children is rare, and the thorough examination of such cases apart from hospital practice very difficult: moreover, the accounts given of this disease in the various textbooks of children's diseases and of hæmatology is so imperfect that I have been led to publish these cases.

First of all, it is necessary to bear in mind the chief differences between normal blood in children and in adults. The most striking is the much greater proportion of lymphocytes in children, small lymphocytes forming 50 to 70 per cent. of all leucocytes in children, instead of 20 to 30 per cent. in adults; while large lymphocytes form 6 to 14 per cent. instead of 4 to 8 per cent. in adults. Again, in children nucleated red corpuscles readily appear in any case of severe anæmia, and quickly disappear on recovery

I propose to submit to your consideration to-day three cases of leukæmia, and one of the condition generally known as "anæmia infantum pseudoleukæmica" ("splenic anæmia of children" of some writers): these being all the cases of the kind which have come under my care during the last two years. Of the cases of leukæmia one, of the acute myelocytic type, has been already published in the *Australasian Medical Gazette* for October, 1904; the second was of the acute lymphatic type; while the third is of the "splenomedullary" form, the patient being still alive.

The first patient was a boy aged 3 years, who came under observation in August, 1903, and died after a fortnight in the hospital. There was no enlargement of the spleen, and the only glands enlarged were the submaxillary ones. There were many small subcutaneous hæmorrhages. The blood count was as follows:—Red cells—1,900,000 falling to 1,000,000 shortly before death; white cells—35,000 rising to 115,000 before death; hæmoglobin—not taken; polynuclears—32 per cent., the last count was 18 per cent.; lymphocytes—41 to 33 per cent.; myelocytes—25 to 45 per cent. shortly before death; eosinophiles—varied between 1 and 2 per cent. No nucleated red cells were seen. The child was not at all rachitic, and there was no suspicion of congenital syphilis. No autopsy was allowed.

The second case was in a boy aged 2½ years, who came under observation in October, 1903. This was a typical case of acute lymphatic leukæmia. There was a history of about two months' indefinite illness, and of getting rapidly worse during the last two weeks. On admission he was very anæmic, with wide-spread small subcutaneous hæmorrhages; none extensive. There was almost universal moderate enlargement of the lymph glands, generally to the size of a small marble. The spleen was slightly enlarged, and extended to about 2in. below the edge of the ribs. The liver was also enlarged. He was not rachitic. There was no membrane on the tonsils, and no ulceration of the gums. The temperature was up all the time he was in the hospital, varying between 99 and 101. He died after eight days in the hospital. The blood condition was as follows:—Red cells—1,760,000 on admission; 1,144,000 the day before death; white cells—43,000 on admission, 80,000 the day before death; hæmoglobin—35 per cent. on admission; lymphocytes—96 per cent., mostly small; polynuclear—3 per cent.; myelocytes and eosinopiles, 5 per cent. each; nucleated red cells—absent. At the *post-mortem* examination the typical appearances of acute lymphatic leukæmia were seen.

The third case is an example of the spleno-medullary type, so familiar in adults but rare in children. This child is a girl 6 years of age admitted quite recently into the Children's Hospital for hæmorrhage after tooth extraction. There is a history of being ill two and a half years with anæmia. On admission she is very anæmic. There is an enormous spleen reaching down

to the iliac crest, but no enlargement of the lymph glands. During her stay (nine days) in the hospital the temperature was irregular, rising to 101.4 twice. She was taken out by her parents. Blood examination :—Red cells—1,520,000; white cells—208,000; hb. value—30 per cent. Differential count :—Polynuclear—50 per cent; myelocytes—30 per cent.; mast cells—9 per cent.; lymphocytes—large, 5 per cent.; small, 2 per cent.; eosinophiles—4 per cent. Two nucleated red cells were seen in counting 300 leucocytes. She is not rachitic.

The fourth case was one of "anæmia infantum pseudo-leukæmica." This child is a girl, 1½ years old on admission to the hospital in November, 1904. She has been bottle-fed since 2 months of age, and has had diarrhœa off and on ever since 12 months of age. She was first noticed to be rachitic about six months ago. On admission it is noted that the child is rather anæmic, has well-marked rickets with bow legs, and a distended abdomen. The spleen is very much enlarged, and extends down almost to the crest of the ilium. There is some diarrhœa, with three or four motions a day. The temperature rises at night to 102 or thereabouts. On December 2nd it is noted that the child is brighter, the temperature is normal, and that the diarrhœa has ceased. The spleen is a little smaller. She takes her food well and is contented. After that she continued to improve rapidly till, on discharge, she was practically well; the spleen being hardly palpable, although the abdominal distension was still present. She was given hydrarg C creta ½gr., *bis die,* till her motions became normal. There were no signs of congenital syphilis.

Date.	Red Count.	White Count.	Hb.	Polynuclear.	Lymphocytes.	
					Large.	Small.
1904.			Per cent	Per cent.	Per cent	Per cent
November 23.....	3,700,000	31,500	60	44	6	31
	(Another differential count the same day)		—	36	40	
December 4	?	20,000	?	42.4	5.2	32
December 7	5,200,000	18,400	?	40.5	7.5	33
December 21	?	12,000	?	38	44	
January 13, 1905..	4,700,000	7,000	?	30.5	65.5	

Myelocytes.	Eosinophile.	Mast Cells.	Remarks.
Per cent.	Per cent.	Per cent.	
14	4.	1.	Three normoblasts seen while counting 250 white cells, a few megaloblasts seen.
19	—	—	—
16	3.6	.8	Six normoblasts and one megaloblast seen in counting 250 cells, red cells normal.
12.5	5.75	.75	Two normoblasts seen in counting 220 cells and two megaloblasts.
15	3.0	?	Two normoblasts seen in counting 100 cells.
2.	2.	—	No nucleated red cells seen.

Date of admission, November 21st, 1904. Date of discharge, February 7th, 1905.

The chief point of interest in this case is the high proportion of myelocytes, nearly 20 per cent. Myelocytes have often been observed before in this complaint, but 10 per cent. has been somewhat arbitrarily fixed as the

limit. It is quite absurd to draw a line at 10 per cent., and say all cases above this are leukæmia and all below are not. The myelocytes were fairly equally divided between the large and small varieties.

Von Jaksch's anæmia must be rare in Australia, as this is only the second case I have seen in six years (I have not included the other case, as the differential count was not satisfactorily done). It seems to be common enough in the large cities of Europe; this is probably due to the infrequency of rickets with us, as this form of anæmia is almost invariably associated with rickets. This fact lends support to the view that the same causes which produce rickets also produce this disease.

A CASE OF SPLENO-MEDULLARY LEUCOCYTHÆMIA TREATED WITH X-RAYS.

By F. J. Clendinnen, M.D., L.R.C.P., &c.

Frederick P., married, aged 38 years, was admitted on 5th of November, 1904, to the Melbourne Hospital, under Dr. Maudesley.

His history previous to admission was briefly as follows:—Suffered from bronchial asthma during 1891, in Southern Queensland; disappeared when living in Thursday Island; suffered from dengue fever during epidemic of 1896 and 1897, in Thursday Island. Contracted enteric fever in 1900, in South Africa. About May, 1902, three months after arriving in Melbourne, became pale with a worn appearance, but otherwise felt in fair health; gradually became more pallid until March, 1903, when he became very weak, legs and ankles swelled at night, suffered from occasional pains at the left side about the stomach, and a feeling of fullness after meals. Sought medical advice and was prescribed arsenic and iron, which he continued for some time without much benefit. Took six weeks' rest from work; took sea trip to Brisbane, returned to Melbourne improved, less pallor; resumed work. Continued to improve until October, 1904, when he noticed his abdomen increasing in size: sought medical advice; was tapped, and large quantity of fluid removed. Had no hæmorrhage of any kind. After admission the fluid accumulated again. He was sent to me on December 1st, 1904, as a case of spleno-medullary leukæmia, to have X-ray treatment *a la* Senn.

On examination, patient looks pale, thin, and weak—distended abdomen; margin of spleen could not be made out, owing to the ascites present. After about a month's treatment the edge of the spleen could be felt about an inch to the right of the umbilicus, and lower edge about one and a half above the left iliac spine. Ten minutes' exposure over the splenic area with a medium tube, distant about 6 inches, with two and a half to three amps in the primary, were given three times a week. After about ten applications he was given a rest, owing to slight dematitis setting in amounting to mere bronzing and exfoliation of the epidermis. The ends of the long bones were also submitted to ten minutes' exposure, chiefly over both knees.

During the time he was under treatment with the X-rays, I stopped all internal medication. His temperature fluctuated between 98 and 100.6.

He was discharged from the hospital on December 9th, 1904, and attended as out-patient; for about the last three months he has only been attending now and again.

Examination of Blood.—Blood examined in Brisbane, May, 1903—actual figures not known to patient: approximately, 3,000,000 red, 200,000 whites.

Melbourne Hospital—November, 1904—Erythrocytes, 4,600,000; leucocytes, 190,000; hæmoglobin, 70 per cent.; characteristic leukæmic film.

December, 1904—Erythrocytes, 4,300,000; leucocytes, 156,000; hæmoglobin, 75 per cent. February, 1905—Erythrocytes, 4,360,000; whites, 129,000. March, 1905—Whites, 132,000. April, 1905—Whites, 125,000. May, 1905—Not known. June, 1905—1,000,000 (?). July, 1905—Whites, 300,000. August, 1905—Red, 2,796,000; whites, 264,800; hæmoglobin, 60 per cent; large number myelocytes, also nucleated red.

Frederick P. started X-ray treatment on November 30th, 1904. December twelve sittings, consisting of ten minutes over splenic area and ten minutes over the terminal ends of the long bones. January had ten sittings likewise. February twelve ditto. March nine, April eight, May thirteen, June ten, July ten. Temperature never been up to 100 since 23rd of April, 1905.

DISCUSSION.

Dr. Springthorpe referred briefly to the light thrown upon the use of radium by the investigations upon cancer, which had been so well brought under their notice by the President of the Section on Pathology. It was generally recognised that from the material side life came in as a new kind of movement in molecules of a certain complexity. This was seen in the mitoses which accompany cell proliferation. In cancer it now seemed as if a second conception, so to speak, was started in the same cell—a sort of molecular twins—in which the second offspring grew asymmetrically and dominated the situation. This seemed to put ordinary germs out of the question, but left it still unsettled why this second birth had invaded the cell. But, in radio-active and similar emanations we had molecular movements of the same fundamental simplicity well calculated to influence these vital vibrations, and possibly presenting the key to a wise interference in abnormal conditions. The therapeutic application of X-rays and of concentrated solar and electric light might have a similar basis.

PHLEBITIS—INTERNAL JUGULAR.

By F. J. Clendinnen, M.D., L.R.C.P., Melbourne.

On December 18th, 1904, I was summoned to attend Mrs. B.; *æt.* 36, married, two children. I found the patient was about four months advanced in pregnancy. She was complaining of great pain in the left leg. On examination I found the leg somewhat swollen, pale, slight œdema, and in the popliteal space there was found a small lump about the size of an almond. She was kept in bed, and leg wrapped in wool, and flannel bandages applied. Apex of heart in normal position; systolic mitral bruit, second aortic sound slightly accentuated.

Previous History.—When a child, had measles. At the age of 19 she suffered from amenorrhœa and anemia, due, as she states, to shock after her sister's death. At the age of 21 was laid up for four months with dropsy of legs. No history of fluid in chest or pericarditis.

Family History.—Father and mother dead. One brother died of diphtheria, and another of stoppage of the bowels; one sister died of consumption, and the other of diphtheria.

Eight years ago I attended her with her first child. She then had about 30 per cent. albumen in urine, and I expected all manner of complications; but she had quite a normal confinement, and made a good recovery without slightest hitch. The second confinement, two and a half years ago, was also normal; still, the urine contained a large quantity of albumen.

On Friday, March 31st, I was summoned about 6 p.m., and found patient suffering from hæmorrhage, which started at 4 p.m. Os was small; plugged vagina and waited awhile. Returned in the evening, removed plugs, douched, and replaced more plugs. About midnight found things not improving: decided to dilate and deliver—fully made up my mind that it was a case of *placenta prævia*. Chloroform was administered, and I proceeded to dilate by digital method. Inserted my hand after dilating up to four fingers, ruptured the membranes and turned: delivered female fœtus, stillborn. Expressed the placenta. No further hæmorrhage: pulse quick.

April 1st and 2nd.—Temperature normal; pulse 88.

Monday, 3rd.—Patient complained of pain in the head, very like neuralgia. Pulse, 76; temperature, 99°; urine, 40 ounces.

Tuesday, 4th.—Pain still in head, with slight throbbing; everything else normal. Ordered phenacetin and caffein cit.

Wednesday, 5th.—Patient complaining of pain in right side of the neck, with a stiff feeling; intense pain on moving the head; the throbbing in head worse.

Thursday, 6th.—Examined neck, and found a small lump, tender, above sternal end of clávicle, over the internal jugular vein; pulsation above this and up side of neck.

Thursday, 6th.—Throbbing in head very much worse, with paroxysms of pain, which started at the lump on the right side of the neck, gradually going through the head with great throbbing and a bursting sensation; gradually dying away, leaving a burning sensation down the spine. She was unable to sleep, owing to the intense pain and throbbing in the head.

Friday, 7th.—All day the pain was very bad; the attacks much more frequent. Had consultation with Dr. Stawell, and we agreed to give her one-sixth of a grain of morphia and one minim trinitrin, two-hourly for a few doses, afterwards four-hourly.

Saturday, 8th.—Slept about three hours after morphia; pain and throbbing slightly better. Had another bad attack of pain in evening.

Sunday, 9th.—Pain and throbbing again troublesome; not able to get any sleep. From midnight very restless.

Monday, 10th, at 8.30 a.m.—Patient collapsed, got very cold and clammy, almost pulseless. After treatment revived, and went on until 9 p.m.: collapsed again; pulse, 140. Revived again under stimulating treatment. Ordered her strychnine, digitalis, and sp. am. aromatic and bromide, to be injected per rectum.

Tuesday, 11th.—Had three or four hours' sleep after the bromide. Pulse varied, 90-100; temperature, 99°.6. Took plenty of milk food; bowels freely moved; urine, 30 to 40 ounces in twenty-four hours. Slowly but steadily improved from that on. Urine specific gravity, 1020; albumen, 30 per cent. Few hyaline and granular cells; no blood casts, epithelial, or fatty casts; bladder epithelial—characteristic of large pale kidney. Blood pressure, 166.

I thought this case sufficiently interesting to bring before the members, as I have been unable to find a similar case yet published. This patient had no eclampsic attacks during any of her confinements. I may state that this is the third case I have had of patients suffering from albuminuria who have suffered during the middle months of pregnancy from phlebitis, and all in the left leg. It can be readily understood why they should occur in the popliteal, probably due to traumatic origin, injury to the intima of the vessel—similar to popliteal aneurisms. But does the toxæmia, not puerpural but uræmic, play an important part in coagulation of the blood in these cases of albuminuria?

THE PRINCIPLES OF CHEMISTRY AND THE STUDY OF MEDICINE.

By C. Reissmann, M.A., M.D., Camb., B.Sc., M.R.C.P., Lond.

Motto—"Ein arzt, der nur die Medizin kennt,
Kennt nicht einmal die Medizin."
—Aphorism of a Spanish Teacher.

Theme.—In a very great measure the advance in our knowledge of the science of medicine is the direct result of chemical research. There are many problems in medicine—problems often of the first importance—which cannot be studied nor understood without a general acquaintance with the principles of chemistry in its several branches.

Recent research in chemistry so frequently, and often so unexpectedly, has an important influence on the progress of medicine that many of us medical men feel that we must be able to keep in touch with its present developments; to watch this sister science, and ever to be ready to avail ourselves of such of her discoveries as may be useful to us in the diagnosis and treatment of disease.

To be able to do this we must possess some foundation of chemical education. This does not mean that we must make an exhaustive study of chemistry, nor that we must study chemistry to the exclusion of other branches of science: we need not even burden our minds with many of the facts now acquired by medical students. For instance, a knowledge of the physical properties of chemical substances, though valuable, is of relatively minor importance. At most we can only hope to know but a few of them—for of the compounds of carbon alone more than one hundred thousand have been described. It is, however, of the very first importance to be familiar and thoroughly conversant with the principles of chemistry—descriptive, analytical, physical, and physiological, which is the alphabet and grammar of a science in which we live.

Think only of the new synthetic preparations which are brought to our notice by the enterprising commercial traveller, or by conspicuous advertisements. We are baffled by their number and the complexity of their formulæ. To understand the chemistry of them requires an amount of technical knowledge which few of us possess. We are then tempted, either to condemn these preparations without consideration, or else to accept wholly all the vendor may paint in his attractive advertisement. In most cases it requires no specialised technical knowledge, but only a good general acquaintance with the principles of chemistry, in order to recognise whether or not the claims made in favor of a drug rest upon the safe foundation of accurate reasoning from observed facts. Thus, if someone should assert that an emulsion of petroleum is a wholesome food for consumptives we should be ready with the reply that petroleum is a hydrocarbon, and hydrocarbons are not food stuffs. If we are told that petroleum emulsion is an efficient germicide, we should ask for the evidence of a competent bacteriologist.

I suppose most of us have to make a chemical examination of urine every day of our lives, but do we always succeed in detecting all the most important pathological ingredients? Why does myelopathic albumosuria count among the rarest of diseases? I suspect that it would not be so extremely rare if it were not that sometimes when examining urine we have not observed what we have seen, or else we have not known what we have observed.

For the proper treatment of a disease such as diabetes, where there is a want of assimilation of an important food, a knowledge of chemistry is all-important. In a patient suffering from diabetes the power to assimilate

dextrose is impaired, or lost, but most patients can nevertheless assimilate a certain amount of lævulose. The lævulose is stored in the liver as glycogen, and it seems probable that it can completely replace dextrose in the diet: therefore it is sometimes prescribed. But, as a rule, diabetic patients do not complain because they are forbidden to take sugar, for they can still sweeten their foods with saccharine or saxin: they do complain, and sometimes very bitterly, because they may take neither bread nor any form of starchy food. Starches are not allowed because they are converted into dextrose, which to them is useless as food, and may even, as some think, act as a poison.

Now if there should be a starch which, on digestion, yields lævulose and nothing else, is there any reason why this starch should not be given? Such a starch, indeed, exists; it is found abundantly in the pure state in dahlia bulbs, and it is known as inulin. The simplest way to prepare inulin is to mince dahlia bulbs, digest them with a small quantity of water, and strain. The water contains nearly the whole of the inulin, a large quantity of which will settle to the bottom of the vessel in twenty-four hours. It may be dried to a white powder. A much larger yield is obtained if the inulin be precipitated with alcohol and then collected, for it is somewhat freely soluble in water.

I have had no difficulty in preparing a large quantity of this starch. It has a somewhat sweet taste, but is otherwise tasteless. If biscuits could be made containing inulin I believe they would be a valuable food for some diabetic patients. The attempts I have myself made in this direction have not been very successful.

I have selected diabetes as an example, but in every disease in which there is faulty digestion or mal-assimilation of food, a knowledge of chemistry is useful, if not indispensable.

Histology.—Consider now Histology and the use of staining reagents. At one time these were used in a haphazard fashion. Does not this remind one of the epoch of empirical methods in chemistry? This period of physiological alchemy is only now passing away. To-day we regard stains as chemical reagents. When we stain an animal tissue with a dye we perform a chemical reaction very much in the same way as a chemist tests for an inorganic base in a test tube. As soon as this principle was properly grasped a great advance was made in physiological and pathological histology. By such chemical means is the intricacy of the central nervous system being unravelled at the present time.

The method of Golgi—a method which Sir Victor Horsley says is revolutionising all our definite knowledge of nerve fibres—what is this method but a chemical reaction?

The staining methods of Marchi or Ramon-y-Cajal, of Nissl, and of Weigert are definite chemical reactions, and until we recognise this all search for further improvements is mere groping in the dark.

Weigert.—The first to demonstrate bacteria by means of chemical dyes was Weigert. It was he who introduced the use of aniline colors in microscopical technique. Weigert's first attempts were empirical, but later they followed a logical sequence.

Most of the methods now used to demonstrate nervous structures are founded on Weigert's researches. These researches led Weigert to the discovery, which I will shortly relate, that the successful staining of nervous tissue depends on the recognition of a definite chemical principle.

It had long been known that nervous tissue has a special affinity for many highly oxidised metallic compounds—as osmic acid and chromic acid. The search for a special method of staining medullated nerves led to important results, not the least of which was the discovery of the tubercle

bacillus: for, in his first communication, Koch demonstrated the tubercle bacillus in sections stained with alkaline methylene blue and counter-stained with Bismarck brown. But this was the method which Weigert was using to penetrate the structure of the central nervous system. Here it failed. He therefore tried methyl violet, and again counter-stained with Bismarck brown, and was now rewarded by seeing the network of Gerlach in the grey substance of the cord, a structure hitherto only demonstrated by gold preparations. For several months he continued his experiments without success, until at length he tried an aqueous solution of acid fuchsine—a dye not hitherto used in histology. Sections of the cord were stained dark red. To remove the excess of stain he used an alkaline fluid, and he chose alkaline alcohol as the sections must subsequently be hardened in alcohol. To his astonishment they were completely decolorised. Thinking them destroyed, he threw them into a dish of water, when, to his joy, they again became red, and now showed the same differentiation of structure as sections stained with methyl violet and Bismarck brown. Gerlach's network was demonstrated, and the white substance appeared dark, the grey substance light.

But the method, successful as it was for sections of medulla and cord, failed to show the nerve fibres in the brain.

Meanwhile, Paul Ehrlich had improved the method of staining tubercle bacilli by using a strong mineral acid in place of Bismarck brown. Weigert adopted the same principle to stain brain sections: he used acid fuchsine and washed with dilute hydrochloric acid, and succeeded in demonstrating the nerve cells and their nuclei. But he still failed to demonstrate the fine nerve fibres of the cerebrum. Ultimately he saw that success was only achieved in sections hardened in chromic acid. This acid acted as a mordant, and subsequent staining depended upon a chemical reaction between the dye and a compound formed by the mordant with some constituent of the nerve structure. Various metallic salts served as mordants—salts of lead, tin, iron, and vanadium; but copper salts gave the best practical results, and hæmatoxylin proved the most successful dye. Whereas, in the method of Golgi, there is an element of chance whether or not a given specimen will be successfully stained, Weigert's method has this to commend it: that every properly prepared specimen will show both the coarse as well as the fine nerve fibres. The bichromate of potash, in acting upon the medullary substance, is partially reduced to an oxide of chromium; it is in this way that the brown coloration is produced. The reduction is stronger in the medullary substance than in the other constituents of nervous tissue: thus the ganglion cells and neuroglia reduces chromium salts in a lesser degree. This produces the difference in color between white and grey matter, and in normal and degenerated parts of the nervous system.

Thus three years' labor resulted in the discovery that a chemical principle must be recognised for the successful demonstration of the fine nerve fibres in the central nervous system, and subsequent investigations in this branch of anatomy have all followed this principle which was discovered by the labors of Weigert.

A careful investigation of Marchi's reaction for the demonstration of tracts of nerve degeneration has resulted in the discovery of a chemical test by which a severe and active organic nervous disorder can be distinguished from a functional nervous disorder: for in the former cholin, a substance which can be readily detected, appears in the blood, while it is absent in the latter. My friend Otto Grünbaum has applied this test. He found that in normal blood cholin is absent, or present only in negligible quantities. In four cases of hysteria, and one case of tobacco-poisoning, the result was negative. He obtained positive results in three cases of disseminated sclerosis.

in two of paralysis agitans, in three of tabes dorsalis, in one of progressive muscular atrophy, and in two of transverse myelitis. This method was introduced and has been successfully employed by Professor Halliburton and Dr. Mott. A full account of it is given in Professor Halliburton's book on "Bio-chemistry of Muscle and Nerve."

The detection of iron in the liver by a simple mirco-chemical re-action is the chief argument upon which Dr. Hunter based a theory of the nature and cause of pernicious anæmia.

Ammonium molybdate, followed by pyrogallol, is a chemical test by which a phosphorus-containing substance can be detected. It has proved to us that phosphorus compounds are confined almost exclusively to the nucleus of a cell.

The chemistry of micro-chemical reactions is a branch of chemical science which promises us a wealth of new facts. This is not the only department of chemical science from which our own science derives advantages.

Low Temperature.—Experiments at low temperatures have been carried out by Cailletet, Olzewski, and Dewar, and a temperature of 257° has been registered. Availing himself of these researches Macfayden has prepared a vaccine against typhoid fever, by crushing typhoid bacilli frozen with liquid air.

Velocity of Chemical Action.—The rapidity with which a chemical re-action is completed depends upon the temperature. A reaction which at ordinary temperatures is accompanied by the evolution of heat and light may, at a very low temperature, be completely arrested. As an illustration I may remind you that, at the temperature of liquid hydrogen, fluorine gas is without action on scilicon, boron, carbon, or mercury, though at ordinary temperatures it combines with each of these with the liberation of much energy. This is an extreme example of the general principle that the reaction velocity of chemical agents is increased by a rise of temperature. The connection between such a purely chemical principle and the practice of medicine may appear to be remote, but it finds an application in the dosage of medicine; for by it we know that chemical reactions will occur more rapidly in a feverish patient than in a normal individual, and, therefore, *cæteris paribus*, the dose of a drug prescribed for the former should be smaller than for the latter.

Do we always remember that the chemical action which takes place when we use antiseptic solutions for the destruction of septic germs requires a certain time for its completion? A septic instrument is not rendered sterile by dropping it for a moment in a one in twenty solution of carbolic acid; much less if that solution is warmed by the addition of a further quantity of hot water.

Mass Action.—The department of chemistry which has thrown most light in some of the darker alleys and by-lanes of medicine is that which itself has been most illumined in recent years, namely, chemical dynamics, or the theory of the progress of chemical reactions and the theory of chemical equilibrium. These two phenomena—the velocity of chemical reaction and chemical equilibrium—are explained by the law of "mass action." Every substance, says Berthollet, which enters into chemical combination reacts by reason of its affinity and of its mass. It will be acknowledged, I think, that this is one of the most important natural laws which has been enunciated within recent times.

If equivalent masses of a weak organic acid like acetic acid, and of a strong mineral acid like hydrochloric acid, are allowed to react on an equivalent quantity of a base, such as soda in aqueous solution, it is found that the acetic acid takes one thirty-fourth of the soda and the hydrochloric acid takes the remaining thirty-three thirty-fourths; but if we use more than one

equivalent of acetic acid, this acid, though relatively weak, fixes more of the soda, and less soda is taken by the hydrochloric acid ; and the greater the amount of acetic acid present the greater is the amount of soda it will fix and the amount of hydrochloric acid which will remain uncombined. The weak acid displaces the stronger because it is more abundantly present.

Put into chemical language, one says, chemical action is proportional to the active mass of the reacting substances. The active mass of a substance is the amount of the same in unit volume.

The law of mass action explains the secretion of hydrochloric acid from alkaline blood. By reason of its mass the carbonic acid in the blood sets free a small quantity of hydrochloric acid from the sodium chloride, which is also present in the blood. This minute quantity of uncombined hydrochloric acid is selected by the oxyntic cells in the cardiac portion of the stomach, and the carbonic acid by its mass influence sets free another small quantity of hydrochloric acid. This is again seized by the oxyntic cells, which in time are able to abstract a considerable amount of acid and deliver it into the stomach.

In the study of medicine I suppose there is no subject of greater complexity than the theory of immunity. If one considers Ehrlich's theory of immunity with his conception of receptors and amboceptors of immune body and complement of toxins and toxones, one is struck with admiration at his extreme ingenuity ; but one dearly longs for simpler exposition. Now Masden and Arrhenius have advanced a perfectly simple explanation of immunity, by applying to the ascertained phenomena the law of mass action. They assert that toxins and antitoxins react by reason of their affinities and of their mass, and the reaction toxins plus antitoxin equals a neutral body [toxin-antitoxin] is analagous to the inter-action of a weak base, as ammonia with an acid of weak avidity, as boric acid.

Electrolytic Dissociation.—The theory of electrolytic dissociation—that in dilute solution inorganic compounds dissociate into their respective ions, which carry electrical charges of opposite sign—frequently has a practical application in the practice of medicine.

The disinfectant property of mercury salts depends on the mercury ion, and not on the inorganic ion with which the mercury is combined. In a given solution of mercuric chloride the disinfectant action is proportional to its degree of dissociation, that is, to the number of mercury ions present.

But the degree of dissociation of a dissolved electrolyte is diminished when an electrolyte containing a common ion is added to its solution.

Therefore, the germicidal action of mercuric chloride is lessened by the addition of sodium chloride or hydrochloric acid to its solution.

Pasteur.—About this time five years ago there passed away one of the greatest chemists the world has known. The grandest illustration of the influence of the principles of chemistry on the study of medicine is to be found in the life of the chemist Pasteur ; and we now remember with sorrow that at every incursion he made on the domain of medicine he was looked upon as one who was poaching on the preserves of others. But he was a chemist to whom the greatest of living surgeons, Lord Lister, wrote as follows :—" Allow me to take this opportunity to tender you my most cordial thanks for having, by your brilliant researches, demonstrated to me the truth of the germ theory of putrefaction, and thus furnish me with the principle upon which the antiseptic system can be carried out."

Studies in crystallography taught Pasteur the use of the microscope. The microscope furnished him with the means of penetrating the mystery of fermentation. An investigation of fermentation led him to see the error of the theory of spontaneous generation, and to the discovery of micro-organisms

as the cause of disease. From this it was but a step for him to preventive inoculation, the cure of anthrax and hydrophobia, and of many other diseases.

I have given you a few instances in support of my assertion that the principles of chemistry—by which I mean the fundamental laws and theories of chemistry—should be mastered by every student of medicine, because he will have frequent opportunities to apply them in his practice. They are almost as useful to him as are pure mathematics to the student of engineering.

Gentlemen, this theme, if time and a fuller knowledge of the subject on my part would permit it, might be elaborated in an extensive treatise. The great influence of the principles of chemistry in the study of medicine is one of the chief practical results which we owe to this age, which has been called the age of natural science.

A physician may no longer be a specialist in one branch of natural knowledge called medicine. I believe that, with fuller knowledge and wider experience, we shall discover that medicine, like chemistry, is but a branch of the great tree of natural science whose stem is physics.

It is often said that an educated man is one who knows something of everything and everything of something. An educated physician should likewise know everything of medicine and something of every other science.

It is an historical fact that great and rapid progress has been made since we began to study the phenomena of physiology and of pathology by the methods of physics and chemistry, and abandoned those void and abstract speculations that were used and misused by the philosophers of the early part of the last century.

Chemistry, like physics, is one of the preliminary subjects of study prescribed in the curriculum of medical education. Some think little of it, and find no more of interest or of value in it than the man who, lacking a sense of harmony, finds in the rehearsal of a great opera.

I have deemed this subject of sufficient importance to bring before this Congress, because most of you are closely associated with medical students, who look to you for guidance and advice; and I ask, if you agree with what I have said, that you will convince them that if in the study of medicine they would be masters they would do well first to master the principles of chemistry.

ERYTHEMA INDURATUM, OR BAZIN'S MALADY

(*Erytheme Indure Scrofuleux*).

By F. J. Clendinnen, M.D., L.R.C.P.,

Mrs. Q., *æt.* 32, married; three children, the last two years ago—all healthy. *Family History.*—All healthy.

Previous to marriage was engaged at business which necessitated standing greater part of the day. Since marriage, always ailing; never has good appetite. Bowels regular. Suffers from cold hands and feet; no chilblains. Lived in Adelaide, but for ten years in Queensland. Whilst in Queensland was treated with specific treatment. In Adelaide was treated with rest and local treatment.

When I saw the patient, who looked anæmic, she was unable to get about, as her legs were in a painful condition with numbers of sloughing ulcers. This condition started two years last June, as red nodules with a broad base. This gradually darkened in color, till it attained a purplish hue. The outer margin of the nodule softened until it eventually broke down and caused a circular

slough, leaving a patch of necrosed tissue in the centre which eventually sloughed off and left ugly indolent ulcers—rather suggestive of gummata. It started in winter (June): no febrile symptoms. The nodules were at first very painful till they discharged, when they became more bearable. The early stage resembles erythema nodusum were it not for the circular hard lump in the centre.

All the authorities agree that complete rest is necessary, and that local treatment should be employed, and the patient's strength supported. I decided to try the ultra-violet rays. I told her to come to me, and she replied "that she could not walk." However, she attended, conveyed to my house in a cab for the first week; afterwards walked to station, and came by train. They all healed up in course of about five weeks, with the exception of a fresh nodule appearing in another part of the leg. She was unable to attend for further treatment owing to approach of her confinement.

The treatment with ultra-violet rays was applied daily for from five to ten minutes during first week; afterwards three times a week. No internal treatment was administered; the only local treatment consisted of boric foments—together with the ultra-violet rays. All the ulcers treated were healed within six weeks. Several indolent ulcers of long standing have been healed up by this method, which consists of alternating current transformed up to about 6,000 volts to an iron electrode lamp, with a condenser in parallel.

I carry out a method entirely my own, in which I use no compression with rock crystal, but, as I term it, an open method. I have adopted this plan simply from experiments I have made in electrical discharges through the air. I have also adopted another mode by painting a celluloid film with a fluorescent substance; placing this over the part and applying the rays, the fluorescence remains for one hour or more, according to the charge.

BACTERIOLOGICAL EXAMINATION OF CASE.

By Dr. Huckell.

A cover slip preparation of the pus stained with eosin and Loeffler's blue showed the usual number of broken down white corpuscles, and cells, and crowds of cocci. There were also present bodies resembling the stellate arrangement of clubs which is found to obtain in actinomycosis. These took up the basic stain, but not very strongly. These appeared to be of the nature either of one of the mould or else one of the pleomorphous bacteria of the streptothrix order.

Agar and blood serum cultures were then made of the pus: a great number of colonies appeared. A pretty exhaustive examination of these, however, revealed only the well-known staphylococci, both the albus and aureus. The presence of the streptothrix could not be confirmed by culture, but it may be that the proper culture medium was unfortunately not hit upon. We know that the actinomycosis presents only a mycelial arrangement and gonidia-like bodies in artificial culture, and not the more familiar stellate growth, and we might therefore expect to get a different appearance in this hypothetical organism, but on culture results were apparently negative—that is, so far as an hitherto undescribed bacterium is concerned.

Differential Diagnosis.—On first sight one would say that the ulcers were of specific origin, but this may be eliminated, seeing there is no specific history; moreover, by its not responding to specific treatment.

From erythema nodusum, its duration, etiology, they suppurate; no history of rheumatism, no joint affections; it is worse in winter; no endocarditis.

THE MEDICAL TREATMENT OF APPENDICITIS.

By F. Lucas Benham, M.D., M.R.C.P., Lond.

It is with much reluctance and hesitation that I venture to speak on this subject. It has been so much discussed of late years that very little that is fresh remains to be said. The question seems, however, not to be exhausted yet, judging from the contributions which still help to fill the medical press. Nearly all such recent contributions are, however, written from a surgical point of view, and assume that the surgical treatment of the disease has largely superseded the medical: at any rate, I think that this tendency is getting a stronger hold. For my own part, I think that this surgical aspect has been carried to excess; and I propose to offer a few remarks which may help in judging what purely medical treatment can accomplish; and whether it is best to rely on such treatment, or to hand over cases to the surgeon at an early stage; and whether surgical operation is required at all in any considerable proportion of instances. Just like those variable stars which suddenly start from insignificance and invisibility to a position of the first magnitude, so has the disease in question come into prominence of late years. It has been asserted that it is a new disease: that it is a disease that has increased in frequency and in severity: and that surgical operation gives better and more sure results than medical treatment. That it is a new disease is quite an untenable proposition. Its identity as a disease arising from lesion of the vermiform appendix has been recognised at least as long ago as 1836, and no doubt its existence could be traced much farther back. In 1836 it was distinctly described by Addison (Bright and Addison's "Practice of Medicine"). Burne (1) also described it in 1837. The doctrine was maintained principally by the Guy's school and was upheld by Wilks and Moxon ("Pathological Anatomy"). Dr. Crisp (2) published a series of thirty-two fatal cases in 1856, and eight more were reported to the Pathological Society between that time and 1870. Lewis, of New York, also published a collection of forty-seven fatal cases in 1852. In 1871, Bristowe (3) wrote a very lucid account of the nature of the disease. He "defined cases of typhlitis and perityphlitis as those in which there is perforative ulceration of the cæcum or vermiform appendix, and in which, therefore, there is either limited suppuration in the neighborhood of those parts, or sudden peritonitis. Perforation, in the great majority of cases, no doubt occurs in the vermiform appendix. It is not improbable that in some cases the perityphlitic abscesses are peritoneal abscesses." Still, for certain reasons—the small fatality and few *post-mortem* examinations—the correct pathology was not generally known or accepted. The cæcum and cellular tissue were generally supposed to be the seat of perityphlitis (*vide* Niemeyer), and an intraperitoneal abscess was hardly thought of. Many cases, too, were imperfectly diagnosticated, and were looked on as "gastric fever," "inflammation of the bowels," "peritonitis," and "intestinal obstruction." Probably many fatal cases, in which the characteristic symptoms of typhlitis were wanting, were so classed. Dr. K. Fowler said, at the Clinical Society in 1901, that eighty-five cases of typhlitis or perityphlitis were admitted to the Middlesex Hospital in ten years; all recovered, and in only two cases were incisions made. But during the same time there were fourteen necropsies with perforated appendix, but the diagnosis had been peritonitis, intestinal obstruction, enteric fever, &c., and in none of these were there the ordinary symptoms of typhlitis.

Nor is it probably any more true that the disease, although a familiar one already, has appeared in greater frequency. At first sight, there seems

a good deal to be said for the hypothesis; it seems almost self-evident. From the large number of cases reported, as well as the much larger number admitted to hospitals, compared with those of former times, and especially the fatal cases, of which we so seldom heard before, and even from the cases within our own experience, which assumed a novel and more serious character when our attention was drawn thereto, it is very difficult to avoid the belief that the disease is present in a new and more serious phase.

Nevertheless, I cannot help thinking that the supposed increase, both in frequency and severity, is a myth—a pure illusion. It is true that opinions are divided. In support of a supposed increase we have the weighty authorities of Sir W. Gairdner and Dr. G. S. Keith. The former (4) says that he saw his first fatal case in 1884; only four others were found in the records of the Edinburgh Infirmary for ten years: he affirms that the share of the appendix had not been overlooked, but the mortality was probably not more than 5 per cent. The latter (5) said that there was nothing more remarkable in the medical world than the extraordinary increase in cases of appendicitis. It was almost unknown, he said, in the middle of the last century. During forty years' practice in Edinburgh, among all classes, he did not remember a single case, and had heard of none for the last twenty-five years among his old patients. He ascribes the supposed increase to full feeding, especially of butchers' meat. Many others too, have written in support of the same view, and have gone the length of assigning a cause—the use of enamelled cooking-pots, frozen meat, vulcanised indiarubber of bottle-stoppers, &c., &c.

On the other hand, Messrs. Treves, Mayo Robson, Dr. West, and very many others doubt or deny the increase. The *Lancet* itself says (6), "There is, in fact, but little if any reason for thinking that a real increase has occurred in the number of cases of inflammation of the vermiform appendix. The apparent increase is probably due to transference of many cases to the surgeon and exploratory laparotomy for obscure diseases." And, again (7), "Improved diagnosis will probably account for some of the apparent increase of cases." Thus it seems to me that at any rate much stronger proof of the alleged increase is required to be convincing. The greater number of cases sent in nowadays to hospitals (where most statistics are compiled), with a view to operation, accounts in great measure for the apparent increase. For instance, at St. Bartholomew's Hospital in 1882-4, 111 cases were admitted, with seventeen deaths; in 1899-1901, 301 cases were admitted, with thirty-eight deaths; and of these cases 217 were operated on. In the Birmingham Hospital the figures correspond. In the Zürich Hospital also, during the twenty years 1880-1900, forty-five cases were admitted during the first fifteen years, 158 cases in the last five years. Moreover, this supposed increased prevalence extends all over the civilised world, for all countries tell the same story.

The personal factor has a good deal to do, I believe, in forming conclusions on the point. As an illustration of this, I think I may instance a statement of Sir F. Treves (8), that the disease is especially prevalent in tropical and sub-tropical climates; because a large proportion of cases in which he had removed the vermiform appendix for recurrent attacks came therefrom. Is it not far more likely that persons suffering from the complaint and living in a distant British provinces resorted to him in particular because of his wide reputation?

Though I cannot admit any proof of the increased frequency or severity of the disease, I can see no *a priori* reasons why it should not be so. We may compare cancer and insanity, whose absolute increase is still discussed and unsettled. Pancreatitis is a new disease in the sense that it has only lately been identified; yet no one would deny that it has always existed,

and as frequently in former times as now. But diphtheria has almost certainly increased; and many other acute specific diseases, dependent on the activity of microbes, vary in prevalence; they have their periods of waxing and waning—a phenomenon noted by Sydenham, and ascribed by him to the epidemic constitution of the year. What is influenza, in all probability, but an extra virulence of the organism which causes common colds? But most of these diseases are contagious, which appendicitis probably is not; nor has the latter been proved to be essentially a microbic disease. It is held by some that influenza has been a powerful predisposing cause of appendicitis; and there might be a good deal to be said for this view, only the supposed alarming increase preceded the outbreak of influenza. The only points about the disease which are really new are—1. The name "Appendicitis," which, by the way, is a barbarism: it was objected to as long ago as 1859 by Mr. Jas. Long, of Liverpool, who rightly preferred "Typhlitis." 2. A far more exact pathology, due to the more frequent explorations in abdominal surgery and the more numerous necropsies in hospitals; and especially to the knowledge of the correct pathology having become diffused and general, and having led to an improved diagnosis. The statistics of the German army show this well. Cases of appendicitis have rapidily increased in number, but those attributed to disease of the stomach, liver, &c., have *pari passu* declined. 3. The advanced surgical treatment of the disease.

The modern serious attention to the matter began in the United States of America; and was, I believe, initiated by a paper by Fitz in 1886, who had collected reports of a number of fatal cases.

With the rise and spread of abdominal surgery, the peritoneum having yielded its secrets to adepts, the surgeons, always anxious to enlarge the sphere of their operations, speedily took the matter up, and went rapidly ahead in America, where they made a practical application of the new pathology in their own way. The wave reached England. Cases of appendicitis had been operated on (*i.e.*, more than merely opening superficial abscesses) by Messrs. Symonds and Godlee (a case apiece) in 1885 (9), and the appendix had been removed many a time in the course of abdominal operations by Spencer Wells and Knowsley Thornton. Public attention was, however, first drawn to the question in London by a paper by Mr. (now Sir F.) Treves on "Relapsing Typhlitis treated by Operation," read before the Medico-Chirurgical Society (10), in which he recommended removal of the appendix in a quiescent interval after repeated attacks.

The application of the appendix theory was soon extended by surgeons to the active state of the disease. A discussion was opened at the Medical Society of London by Dr. Bull, of New York (11), who advocated early operation. Further discussions took place at the annual meeting of the British Medical Association at Leeds in 1889, and at the Clinical Society of London in 1891; and the medical journals have been full of the subject—both facts and opinions—ever since; and meanwhile attempts have been made, with considerable success, to withdraw the disease almost entirely from the domain of medicine to that of surgery. I was, I think, present myself at the discussions at the Medical and Clinical Societies in London, and have since followed the subject in English periodical literature, with curiosity and interest.

In former times surgeons used to operate only when the abscess reached the surface, or was extending dangerously; next, they advised operation as soon as suppuration had taken place, or was suspected to have done so; but, finding this not very successful, the mortality being quite 20 per cent., they are now getting to recommend the American plan of operating, not

merely if there is no improvement within thirty-six hours, but immediately the diagnosis is made; besides immediate laparotomy in cases of general peritonitis, and removal of the appendix after convalescence, if that has not been done already.

Now, my contention is that most of this advanced surgical treatment is uncalled for and even harmful. It is supposed to be based on the improved pathology, but I doubt the justice of the application. The acquired knowledge should be used, not abused. I am reminded of a certain old lithotomist, mentioned by Sir H. Thompson in his lectures, who was accustomed to be wonderfully successful in his operations until he made a special study of the anatomy of the perinæum. After this, in trying to do still better, steering by his anatomical chart, he only did worse than before. Thus, better knowledge does not necessarily involve better treatment; but the fault is in ourselves, and the lesson is how to use our new knowledge aright. It is admitted by most who have devoted attention to the subject that a very large proportion of cases will recover without surgical intervention. It is impossible to be exact, as few statistics of former years are to be had. Dr. W. Russell, in an interesting paper (12), gives a number of statistics from Germany and elsewhere, and calculates that 90 to 95 per cent. of the cases recover without operation; and this accords with the opinion of Treves and others. Of private cases, probably less than 5 per cent. are fatal: of hospital cases, not more than 10 or 15 per cent. If there has been any increased fatality in recent years, it is probably due to increased surgical activity. Numerous opinions have been expressed that medical treatment is sufficient and the best, for most cases at any rate.

Dr. Fagge says (13), "Even when typhlitis presents itself under the guise of intestinal obstruction or of diffused acute peritonitis, it seldom destroys life if judiciously treated."

Dr. Hare (14) said that he had never found any need for surgical interference in his own cases of typhlitis or perityphlitis. He advised the free use of leeches, which were too much neglected now.

Dr. Fowler (15) protested against the idea that most cases could not get well without surgery. He believed that all cases arose from the appendix. Medical treatment was best for early cases, except where general peritonitis was present.

The debate whether the disease should be treated principally medically or principally surgically is not conducted by the whole body of physicians ranged against the surgeons; for many physicians prefer to hand over their most serious cases for operation; and many surgeons are content that a large proportion of cases should be treated medically. Among the latter class of surgeons, Treves is one of the most moderate and consistent. He defines (16) the disease as a "localised peritonitis in the cæcal region." Though he advises surgical interference when suppuration is known or believed to exist, he seldom has recourse to the knife before the fifth day, *i.e.*, unless there are very urgent and emphatic symptoms. He says, "medical treatment is, on the whole, remarkably successful. Any proposal to treat all cases of typhlitis by operation would lead to disaster. In the London Hospital nearly all cases recovered under ordinary medical treatment (there were twenty-five fatal cases during the last few years). Possibly it was better to do too little than too much and to return to the period before removal of the appendix had set in." And again (17), "The very great majority of cases get well spontaneously. Operation during an acute attack is attended with great risk to life—20 per cent. or more. A fair-sized perforation or a fair amount of gangrene could exist without serious symptoms; and he had

found a concretion lying outside the appendix one month after the attack without a single drop of pus, when all symptoms had subsided, though there must have been rupture, perforation, or gangrene."

Mr. Langton also (18) believes that physicians could cure most cases.

Mr. Ballance (19), however, takes the opposite view. He strongly advises operation in all stages—especially within the first forty-eight hours, before rupture or ulceration has occurred; but he admits that many cases run quite a mild course and get well without any surgical intervention at all. His argument is that medical treatment in any particular case is uncertain as to its result, while surgical procedure is certain; and he quotes Leguen—" The prospect of spontaneous recovery and of medical treatment is a decoy, to rely on which is to be led into a snare."

Dr. Bull, of New York, adopts as an axiom that the risk of operation and exploration is less than the risk of the continuance of the disease. He also remarked (20) that he doubted whether patients would ever consent to have the appendix removed in the quiescent period. His doubts have proved quite unjustified; for patients who have suffered at all from appendicitis are only too eager to undergo this internal circumcision, and to submit to an operation of any sort, especially since an illustrious personage suffered from this disease and was operated on, they think it must be the proper thing to do in every case.

The modern surgical treatment is based on the maxim that wherever there is pus it should be evacuated by incision; or else it should be prevented by removal of the focus of disease beforehand. Though this rule is generally true, there are exceptions of course, and this is one of them. We know now that nearly every case of appendicitis or perityphlitis arises from the vermiform appendix: that inflammation of this organ leads to ulceration and perhaps perforation, with inflammation of the adjacent peritoneum: the contents of the vermiform appendix may exude, or a portion may slough away. This process of ulceration and sloughing of course takes some little time; and, meanwhile, adhesions form between the appendix and neighboring parts—abdominal wall, omentum, intestines, &c.—so that when perforation occurs and the contents are discharged that part of the peritoneum is already pretty well shut off, and thus an intraperitoneal abscess cavity forms. Probably an abscess forms in very many cases; but, before it attains to a large size, it bursts into the cæcum, which acts as a natural and safe drain—quite different from the case of an empyema bursting into the lung—and then the abscess gradually closes. Treves says (21), " When a typhlitic abscess is evacuated, it is not uncommon to find that the abscess communicates by a ragged aperture with the cavity of the cæcum. In nearly every instance there is evidence to show that the abscess had commenced outside (in the appendix) and then burst into the caput coli." The exceptions where the abscess discharges through the skin or bursts into the peritoneum are very rare. Sometimes the pus will dry up and become encapsuled or absorbed. Probably this is the course of the 90 or 95 per cent. of cases of perityphlitis which are known to recover; and little attention was paid to the local symptoms when the inflammatory swelling was believed to be extra-peritoneal and non-suppurative. But, when it became known—just as M. Jourdain, in Moliere's play, came to know that he had been talking prose all his life without being aware of it—that the perityphlitic swelling was an intraperitoneal abscess, medical men became alarmed at even simple normal cases; and instead of leaving them alone, to follow their natural course, some of them seem to have lost their heads, distrusted the medical treatment that they had formerly employed with success, and began to deem surgical interference necessary; and the results have been perhaps less fortunate than hitherto.

The vermiform appendix is certainly specially exposed to certain accidents, but it is also specially protected from the results of these mishaps. Its fixed position in a sheltered corner of the abdomen, its small calibre, and the absence of a free current of foreign contents, make its relations quite different from those of the stomach or intestines in the event of perforation. The chances are as much in favor of a localised inflammation and abscess resulting from a perforation of the appendix as they are against it in the case of perforation of the stomach or bowel. Further proof of the abscess discharging into the bowel may be sometimes had by observing pus in the stools: I have noticed it several times, and it should always be looked out for. There is often a remission of the symptoms about the same time. Sometimes, as the abscess enlarges, it may give way a little here and there in other directions than into the cæcum, and such occurrences may be indicated by more or less serious symptoms of sudden collapse. They are not necessarily grave, as fresh adhesions may form and again enclose the pus, as they usually do. Sometimes the pus finds its way between coils of bowels or into the pelvis, causing symptoms of more or less severe peritonitis. Even this is far from necessarily fatal, even if the spread is of considerable extent. Occasionally, when the abscess has attained a considerable size before bursting into the cæcum, its cavity may afterwards become distended with intestinal gas. I remember a case which happened many years ago, where this seemed to be the explanation of the sudden development of a good-sized tympanitic tumor in the abdomen, in the course of an attack of perityphlitis: this case recovered perfectly, and that attack proved to be the last of the disease.

A few cases happen where the abscess spreads to a dangerous extent; and then the help of the surgeon may be required.

Early rupture into the peritoneum, if it should cause diffuse suppurative peritonitis, is so generally fatal—even 70 to 80 per cent. when operated on—that surgical assitance is of little avail. But, as Treves remarks, " A case of death from perforation is very rare and does not warrant the routine early opening, even in the majority of cases."

Treatment.—I have read a quotation from Dr. Osler that there is no medicinal treatment of appendicitis. With the greatest respect for this author, I venture to say, as Sterne does of his text, in one of his sermons, " That, I utterly deny." A very great deal can be done by medical treatment, including the use of drugs, even in grave cases. It must be borne in mind that, though some cases are so light that they recover speedily and almost spontaneously, with rest alone, there is almost always some peritonitis and often an abscess is formed. But neither the peritonitis nor the abscess is necessarily fatal: even extensive peritonitis may be recovered from. I recollect a case in 1889 which interested me a good deal, as the discussion on surgical treatment of the disease was then fresh and active. It was that of a lad who had a severe attack of appendicitis as the result of a fall on the kerb, while running in the city of London. When I saw him, about two days after the onset, there were not only well-marked local symptoms—swelling, etc., in the iliac region, but also more general peritonitis. He soon developed extreme tympanites, obstruction of the bowels and regurgitant vomiting, yet he made a perfect recovery.

I do not remember having seen a fatal case either during life or after death until the present year, when I met with one at last. It occurred in a lad, aged 16, who suffered from long-standing tubercular caries of the spine, and a more recent similar affection of the right hip, so that the lad was a cripple. There were no sinuses, though I suspected (erroneously as it turned out) a psoas abscess. He developed appendicitis, and though the symptoms were comparatively mild at first—he was so used to pain that he did not

make much complaint—I recognised general peritonitis ; but I was in doubt whether the cause was the appendix, tubercular peritonitis, psoas abscess, or the hip joint. He died from rapid collapse on the fourth day ; and a *post mortem* revealed a sloughy appendix containing a concretion and universal suppurative peritonitis without any attempt at localisation by adhesions. I do not suppose that any mode of treatment could have saved his life.

The most important thing of all in every case is complete rest in bed from the outset. There is nothing more liable to bring on an exacerbation of the disease than allowing the patient to rise and walk about after a few days, when an abscess is beginning to form, although there may be a lull in the symptoms, as often happens.

Next in importance is almost complete starvation for a day or two, and very careful and scanty dieting for some time afterwards—especially the avoidance of solid food.

As far as more active treatment is concerned, the application of several leeches gives great relief at the outset : it may cut short a recurrent attack.

The administration of two drugs in particular is essential from the beginning :—1. Opium, in moderate but sufficient doses ; the object being not only to mitigate pain, but to keep the bowels quiet, so that adhesions may form and the area of disease be limited. It is commonly advised by surgeons nowadays to abstain from opium, in order not to " mask the symptoms"—a most dangerous piece of advice, which, if followed, is only too likely to lead to the case falling into, and perhaps through, their hands. For the same reasons, drastic purgatives must be avoided, although it is well to have the bowels cleared out at starting. 2. Mercury. A dose of calomel iij-v gr. or more should be given immediately, partly to evacuate, partly to cleanse and disinfect the bowels. It should be repeated in smaller quantities at intervals during the next few days. During the whole course of the disease, a grain or two of calomel will at any time check any slight tendency to aggravation, as shown by pain, pulse, temperature, &c. But, in addition to the exhibition of calomel internally (with opium), mercury should be freely applied externally, in the form of ung. hydrarg., all over the seat of inflammation. It most distinctly relieves pain (as it does also in cases of female pelvic peritonitis), and it is more efficacious when combined with ext. belladonnæ—ʒ j., ℥ j. to ʒ j.

Fomentations can also be applied over this ointment.

Some physicians have found very great service from saline laxatives from the very first. They empty the bowels and relieve the congestion of the coats of the intestines. I always administer them in a mild form, and I do not think that they oppose the opium. Towards the end of the illness, aperients are still more required—salts or sulphur. I have seen the urine rapidly become pale and clear at this period after giving sulphur : whether *post* or *propter* I am not sure.

After the first day or two, it is nearly always my custom to order pil. plumbi. e opio, taking care that the pills are fresh and soft, so as to be absorbed. The acetate of lead is very beneficial in controlling inflammation ; and it is a useful remedy, as Graves taught, for relieving the distension of tympanites.

Turpentine, a recognised remedy for peritonitis, is often of great use also, especially in the tympanites which may come on at a later period from peritonitis.

Throughout the complaint, the pulse, temperature, tongue, and urine, as well as local symptoms—pain, tenderness, and distension of the abdomen—are all important signs, and should be carefully watched ; but probably the pulse is the most valuable index of all.

After an abscess has formed, care should be taken to look out for pus in the stools; it will not infrequently be present. If the bowels require relief, a simple or glycerine enema is the best means to employ.

It is important to insist on complete rest in bed and strict light diet until all active symptoms have subsided—the pulse remaining normal, the temperature subnormal and steady, the tongue clean, the urine clear and pale, and the local pain, tenderness, and swelling having disappeared. If all these are natural, there need be little fear of a relapse or recurrence.

If any swelling or hardness persists over the cæcum, with tenderness, not due to fæcal accumulation, a blister helps to clear it away.

If care and attention be devoted to every case, it will very seldom be necessary to invoke the aid of the surgeon; and recovery will almost surely follow.

Patience may be required on the patient's part. A difficulty that we often have to encounter nowadays, especially since the King's illness, is the belief or superstition on the part of the public that every case of appendicitis requires an operation, and that to avoid it shows ignorance or want of skill on our part. If by any mischance the case should end badly, we might get blamed for not having had recourse to surgical interference. This may sometimes cause a little extra anxiety, but the termination usually proves it groundless. I have never had to advise an operation yet, and have not regretted not having done so.

Surgery may be required in some cases, of course. It may be justifiably resorted to in those cases of septic peritonitis which arise at once, without time for demarcation of an abscess; but the mortality of an operation in such cases is very great—70 or 80 per cent., hardly less than if left alone. Also if the general condition is serious from retained and spreading abscesses. The most dangerous and most obscure of these are those which spread upwards along the ascending colon behind the cæcum. As Treves says, the longer an operation is delayed the easier it is to reach pus.

I have tried to show in the above review that the resources of medicine are by no means despicable, even entirely the reverse; and that medicine can produce as good and even better results, even in severe cases, than surgery, even at the hands of the most skilful operators.

Were it true, which is very doubtful, that an operation at the very outset would not only remove the disease immediately, but be attended by infinitesimal mortality, it would still be preposterous to subject every patient thereto, considering that so many recover easily without; for an operation is in itself an evil, *i.e.*, so far as the patient is concerned—a fact which our friends the surgeons sometimes seem to overlook.

References.

1. Trans. Med. Chir. Soc.
2. Trans. Path. Soc., vol. x.
3. Reynolds' "System of Medicine," vol. iii.
4. *Lancet*, July 12th, 1902, p. 108.
5. *Lancet*, July 12th, 1902, p. 108.
6. *Lancet*, March 9th, 1901, p. 731.
7. *Lancet*, vol. ii., 1903, p. 47.
8. Cavendish Lecture, *Lancet*, June 20th, 1902.
9. Trans. Clin. Soc., Lond.
10. Trans. Med. Chir. Soc., Feb., 1888.
11. On the Surgical Management of Typhlitis and Perityphlitis, Trans. Med. Soc., Lond., Nov., 1888.
12. *Lancet*, March 13th, 1904, p. 747.
13. Text-Book of Medicine, vol. ii., p. 223.
14. Trans. Med. Chir. Soc., 1888.
15. *Ibid.*
16. Clin. Soc., 1891.
17. Cavendish Lecture, 1902.
18. Clin. Soc., 1891.
19. *Lancet*, October, 1904, p. 1190.
20. *Loc. cit.*
21. *Brit. Med. Journal*, 1889.

THE CHEMICAL EFFECTS OF THE X-RAYS.

By F. J. Clendinnen, M.D., L.R.C.P., Lond.

At the last Congress, I mentioned my views concerning the stimulating effect of the X-rays. It was stated by a member that he had healed an ulcer of the breast by means of the X-rays, and I put forth my theory that the beneficial result was probably produced by an oxidising agent, *e.g.*, ozone or peroxide of nitrogen. We are well acquainted with the effects of peroxide of hydrogen if syringed into a sinus—how it stimulates the healing process.

I wish to bring forward some experiments that I have been carrying out, which show that probably the healing and stimulating effect of the X-rays is partly due to nitrogen compounds, nitrous and nitric acids being formed by the passage of the rays through the air. I may state that what drew my attention to the probability of this was the bronzing or pigmentation of the skin that occurs frequently in cases treated by the X-rays. We all know that if nitric acid comes in contact with any proteid matter that it will turn it yellow: this is due to the formation of xanthroproteic acid. We also are aware that this xanthroproteic acid, when it comes in contact with ammonia, deepens in color.

This is what occurs probably, these nitric and nitrous ions coming in contact with the skin give ries to the discoloration, and the ammonia in the skin darkens the color, and probably a further oxidisation, forming a brown. This theory, I think, also helps to explain the necrotic changes that take place in the tissues under prolonged application of the X-rays. These nitric ions are carried into the tissues, and set up that destructive condition which is so difficult to heal—a condition simulating a deep burn from nitric acid.

I will now explain how I set about my experiments. Firstly, I started an X-ray tube working, the anticathode pointing downwards into a glass vessel containing a very weak solution of phenol-phthalein already acted on by an alkali, *i.e.*, turning it a pale pink—this pink solution makes a very delicate test for acids.

The tube was allowed to work for ten minutes. During this time, the color of the liquid did not change, but, on shaking it up, the color gradually disappeared, showing, evidently, that an acid was present. In order to verify the test, I played the rays into an empty vessel rinsed out with distilled water for the same time as before, then shook up the test solution, which became, as before, colorless. To this colorless solution I added an alkali (soda), and the pink was readily restored, which evidently proves that the discoloration was due to an acid, and not to an oxidising agent, otherwise it would not be restored, except by a deoxidising agent.

Being armed with the assurance that an acid was present, the next thing to ascertain was what acid this was. Most probably it was nitric, the air being composed, roughly speaking, of nitrogen (78 per cent.), and oxygen (21 per cent.). If any compound of nitrogen and oxygen is formed, it is most likely to be NO_2 (nitrogen peroxide), which is the most stable, and this, chemically acted upon by water vapor in the air, with the result that nitric and nitrous acid are formed. *E.g.*, NO_2 = nitrogen peroxide. $2\,NO_2 + H_2O = HNO_2 + HNO_3$ nitrous and nitric acid.

In order to test the presence of this acid, I obtained some diphenylamine and made a 1 per cent solution with chemically pure concentrated sulphuric acid, which forms diphenyl-sulphonic acid; this being a very delicate test for nitric acid. It is said that one part in 3,000,000 can be detected by the above. I placed this standard solution in a burette.

I place beneath the X-ray tube an empty glass jar, previously washed thoroughly with distilled water, and start to energise the tube. At the end of ten minutes, I put in 1 ounce of distilled water in the glass jar, and place

on it a ground glass lid in order to shake it up and let contents stand for a while. I now transfer the distilled water from the jar into a glass measure, and run in from the burette 1 c.c. of the standard solution of diphenyly-sulphonic acid, and at the junction of the two liquids, if nitric acid is present, there will be a blue band.

I. Made a blank test with distilled water and solution of indicator. No reaction.

II. Ran 1½ c.c. of indicator in 1½ ounce of distilled water, through which the rays had passed for ten minutes. A white flocculent substance appeared in the liquid, which soon began to show evidence of crystals forming, which separated in the light, and on standing the whole precipitate crystalised, remaining suspended midway in the solution.

III. Repeated the above experiment, taking care that the sulphuric ran to the bottom of the liquid to be examined, and a decided ring of a faint blue color appeared, with traces of precipitate as in II.

IV. Repeated blank test, and obtained slight opalescence without blue ring.

V. Passed rays through aluminium sheet, covering glass vessel containing 1½ ounce of distilled water for ten minutes. Repeated test with 1½ c.c. of indicator solution. Obtained same result as in III.

I wish to gratefully acknowledge the valuable assistance rendered to me by Professor Masson and Mr. M. P. Hansen.

NOTES ON CASE OF COMPLETE TRANSPOSITION OF ORGANS.

By A. Jarvie Hood, M.B., Glasgow.

Owing to the rarity of this condition, I consider it justifiable to bring the notes of this case before the Congress.

The patient, Toshugo Kubo (a Japanese cook), age 36, was admitted in the J ward of the Sydney Hospital on the 4th of June, 1904, complaining of cough for three weeks. His knowledge of English was so slight that it was impossible to elicit any definite history of his illness from him, but a Japanese interpreter gave the information that for some years he had been subjected to severe exposure, and had gone through the China-Japanese war.

According to the interpreter he had been fairly well up to three weeks ago, when his cough began. This symptom had come on gradually, and. so far as could be ascertained, there had been no definite rigors ; but the expectoration had been very profuse during the whole of the three weeks. When admitted his principal symptoms were a constant cough and dyspnœa, with profuse and frothy expectoration. The urine contained a small quantity of albumen, but no blood or tube casts.

On Examination : Chest.—Defective resonance was marked on the left side, at the vertebral border of scapula, breathing was harsh, with rales all over both bases behind and in front. The heart sounds were particularly well heard over the posterior chest wall.

Heart.—The apex beat was in the sixth inter-space, 2 inches below and to the right of the right nipple. Heart dullness appears all to the right of the left sternal border, and is continuous with the liver dullness. The heart impulse is very diffuse. The sounds are irregular, and the first sound replaced by a systolic murmer most distinct over the fifth costal cartilage on the right side, but also heard along the lower half of the sternum. The second sound sometimes appears fairly distinct, but at other times is replaced by a systolic murmer : all these murmers are heard distinctly all over the chest behind.

COMPLETE TRANSPOSITION OF ORGANS.

Diagnosis during life verified by post mortem.
August, 1904.

Face page 96.

Liver.—The liver and heart dullness are conjoined, and form an area extending from 2 inches above the right nipple downwards and to the left, crossing the costal margin in the left nipple line and extending 2 inches below it; then across, keeping about 2 inches below costal margin.

There is well-marked œdema of both legs; pulse thin, and at times hardly perceptible. Osseous and nervous system apparently normal.

The diagnosis was evidently one of transposition of the organs, with, at least, mitral incompetency and the usual signs of cardiac failure.

Notwithstanding the measures adopted, he steadily got worse; the urine became very scanty, and the signs of dropsy increased. He died on the 9th of August, and the *post mortem* was performed the same afternoon by Dr. J. S. Thomson.

Post Mortem: Nature of Case.—Complete transposition of the organs; cardiac dilatation; mitral and tricuspid regurgitation; chronic venous congestion of organs; fatty and cirrhotic liver.

External Appearance.—Japanese. Surface of the body very œdematous; arms, legs, scrotum, greatly swollen; *rigor mortis* not marked.

Thorax.—(a) Cavities.—Each pleural sac contained a few ounces of clear yellowish fluid. Pericardium healthy. (b) Respiratory System.—Right lung weight, 1 pound 10 ounces; this lung had only two lobes; it was very congested and œdematous. Left lung weight, 1 pound 13 ounces: this lung had three lobes (well marked), congested and œdematous. (c) Circulatory System.—Heart weight, 1 pound 4 ounces. The heart was situated chiefly on the right side of the body with the apex pointing towards the right. The aorta arose from the right ventricle, and the pulmonary artery from the left. The venæ cavæ emptied into the left auricle, the right innominate vein crossing over to form the superior vena cava. The pulmonary veins emptied into the right auricle. The right subclavian and common cartoid arteries arose separately from one another from the arch of the aorta, while the left one came off as the left innominate artery. The aortic arch passed almost directly backwards, and then turned down on the right side of the spine. The right auriculo-ventricular valve (mitral) had two flaps, and the left three: both were widely dilated, also the ventricles and auricles. The heart muscle was fatty.

Abdomen.—Peritoneum healthy.

Alimentary System.—(a) Liver weight, 3 pounds 13½ ounces. The larger portion was situated on the left side, with gall bladder: in fact, there was complete transposition of all the abdominal viscera, including the relation of vena cava to aorta, the former being on the left side of the latter. The cardiac end of the stomach was on the right and pylorus on the left, spleen in the right hypochondrium, appendix and cæcum on the left, ascending colon on the left, descending colon on the right. Rectum entered the pelvis from the right. Liver fatty, cirrhotic, nutmeg. Gall bladder and ducts healthy. (b) Stomach and Intestines.—Catarrhal condition of mucosa. Appendix healthy.

Genito-Urinary System.—Right kidney weight, 6¾ ounces. Capsule stripped readily; organ swollen and congested; no diminution of cortex. Left kidney weight, 6 ounces, being similar to the right.

Bladder and Ureters.—Healthy.

Genitalia.—Œdema of scrotum: otherwise normal.

Spleen.—Weight, 2¾ ounces; firm and dark; glands healthy.

The brain and spinal cord were not examined.

Diagnosis.—Endocarditis and dextro-cardia.

A CASE OF ACUTE (SUPERVENING ON CHRONIC) PANCREATITIS.

By F. Lucas Benham, M.D., M.R.C.P., Lond.

The patient was a young woman, aged 24, engaged in domestic service. She had never had any serious illness; and, in particular, she had never suffered from jaundice or biliary colic. She always had a large appetite, but was sometimes dyspeptic, and was at the time anæmic.

On January 17th she had been eating some unripe grapes. Next day (18th) she had a pain, in the region of the stomach, on rising in the morning, which she ascribed to the above-mentioned indiscretion in diet. She got about and worked during the day, but could not easily hold herself upright, but bent forward to relieve the pain. She went to bed early in the evening. I was sent for to see her, and visited her about 8.15 p.m. I found her lying in bed on her back, with the legs partially drawn up. She was of dark complexion, but thin and anæmic, and was not jaundiced. She did not look dangerously ill. Temperature, 99.6; pulse, 90; tongue moist, slightly furred; thorax, heart, and lungs, normal; abdomen somewhat distended, walls very hard and rigid, especially at the upper part; the lower portion, including the right iliac region, being much less affected. There was great tenderness and she could not bear much pressure, especially over the epigastrium and below the ribs on the left side. No tumor could be felt. Liver dulness was present. Bowels confined. She had already taken 3 grains of calomel, which had caused vomiting. Morphin, $\frac{1}{4}$ grain, with atropine, was injected subcutaneously, and calomel, iij grain, was repeated. The pain was soon much relieved, though not altogether removed. Ordered to take nothing but a little milk with barley-water and arrowroot.

January 19th.—She had had an easy night, with some sleep. A Seidlitz powder given early in the morning had caused the bowels to be moved. I saw her again at 10 a.m. Temperature, normal; pulse rather below 90; Pain was much less, but the tenderness had not quite gone. The abdomen was softer and could bear handling, but not firm and deep pressure. Nothing abnormal was to be felt. The patient said she had never suffered from severe pain after food, from vomiting, or from hæmatemesis. Her mother had arrived and wished to take her home—about three miles distant. As she seemed fit to be moved, I gave my consent to her being driven there. It seemed to me then that the case might be one of intestinal derangement only, though overnight I had suspected peritonitis, and perhaps perforation of a gastric ulcer—most likely into the lesser cavity of the peritoneum. The mother promised to send for me if anything went wrong. I did not see her again during life, and the next that I heard of her was from her father, who called on me two days later (21st) to say that the patient had died rather suddenly early that morning. The account that I received of the further progress of the case was that the girl remained about the same for the rest of the day on which she returned home. Next day (20th) she was sick, and a sister who visited her in the afternoon thought she looked very ill, although the patient herself and her mother did not think it was serious. At 3 a.m. next day the patient became very ill indeed. Her father tried to get a medical man who lived nearer than myself, but he could not come. Death followed, from collapse, at 7 a.m.

An autopsy was made eight hours after death. The body was fairly nourished, but thin and anæmic. Cadaveric lividity was present and well-marked *rigor mortis* in all the limbs. Froth issued from between the lips. The rib cartilages were not calcified. There was very little subcutaneous fat.

On opening the peritoneal cavity, no lymph was seen, and there was no redness or injection of the intestines: there was some clear yellow serum in the pelvis. The omentum contained very little fat: it was mostly a transparent membrane. In several of the small patches of fat were bright yellow specks, or knobs (fat necrosis)—mostly not more than ⅛ inch in diameter. These were scattered about over the omentum, and to a less degree, over the meso-colon.

The stomach appeared rather small. It was not inflamed: there was no evidence of perforation or of the escape of any of the contents. On cutting through the transverse meso-colon, the posterior surface of the stomach was brought into view and found quite healthy also. The organ was removed and opened along the lesser curvature, and was found to contain only slimy, green, bilious, mucous fluid. Mucous membrane healthy, except for an area of extravasation—chiefly along the greater curvature and on one or two folds of the mucous membrane. Behind the stomach, in the region of the pancreas, there was seen a large nodular swelling, about as large as half an orange, very red in patches, with smaller yellow patches: it was hard and immovable.

Liver.—Large, pale, and fatty. Gall-bladder full of calculi, which were white and facetted. Three of them were as large as marbles, the rest were much smaller.

Spleen.—Rather small, normal in structure.

Kidneys.—Of full size. Capsule stripped readily. Cortex smooth and of good thickness. There was a superficial cicatrix in the upper part of the right kidney.

Pancreas.—The tumor above referred to was evidently the pancreas. It was removed with great difficulty, as it was bound down firmly to the spine and mesentery by dense fibrous tissue, and was tightly adherent to the duodenum and adjacent bowels, so that it was not separated without lacerating them. Below it was an enlarged and partially calcified lymphatic gland. On removing the organ it appeared to have a fibrous capsule—very red and injected with ecchymoses. Patches of the surface were green, others yellow. The organ was firm, but not indurated. On section, the lobules were distinct and for the most part normal to the naked eye. The fibrous septa were very red. Here and there, especially on the surface, groups of lobules were green, amorphous, and gelatinous—almost diffluent. Others had a yellow color. The same was seen, to a less extent, in the interior of the gland. The whole pancreas was considerably larger than normal.

Heart.—Pale and flaccid. No epicardial fat. Valves all quite sound. Right ventricle contained some loose clot as well as some firm and tenacious coagula—some black, some pale and gelatinous—entangled in the *columnæ carneæ*. The walls were pale and soft. The left ventricle nearly empty.

Lungs.—Normal: a few slight thin bands of adhesion on the left side.

Intestines.—Normal (externally). No injection or lymph.

Uterus and bladder.—Normal.

Remarks.—Though a considerable number of cases of acute pancreatitis have now been published, all that occur are of sufficient interest to be recorded. The points of interest about the above case are:—

1. Taking all the symptoms during the course of the illness from beginning to end, they presented a fairly typical example of acute pancreatitis. At the beginning, and when the case passed from my notice, I was quite unable to recognise the disease. I suspected that there was local peritonitis—possibly from perforation of the posterior wall of the stomach; but as the symptoms had improved so much during the first night, I could not be sure. I had only met with one fatal case of the disease before: it was in London, several years ago. I saw it in consultation, and thought it was one of local

peritonitis; my colleague took it to be one of intestinal obstruction, or *vice versa*. The patient was sent into the hospital; laparotomy was performed, and death ensued. The cause of death was discovered; and that was the first I ever heard of the disease, which had then only recently been described.

2. The age of the patient is unusual. It is very uncommon, I believe, to find the disease in a young woman of 24. It is also uncommon to meet with gallstones at that age. The co-existence of two such rarities suggests a causal connection between them.

3. The association of a quantity of gallstones with pancreatic mischief. This is almost constant in pancreatitis, both acute and chronic, though the exact nature of the relation is doubtful. It has been supposed that a gall-stone blocks the outlet of the two ducts and thus causes regurgitation of bile into the substance of the pancreas. In some cases this explanation is quite possible; but in others, as in the present, where there has never been any jaundice, it is scarcely tenable. Also, jaundice is very often present without causing obvious damage to the pancreas. I am sorry that the dense fibrous tissue surrounding the parts prevented me from making a dissection of the ducts. It is very probable, however, that the presence of gallstones in the gall-bladder in some way causes irritation of the pancreatic ducts and glandular substance, which may set up inflammation and destruction of the gland.

4. The dense fibrous issue which in this case enveloped the pancreas, especially at the head, and caused firm adhesion to the duodenum, &c., afforded indubitable proof of previous attacks, or of chronic disease; yet, from the history of the case that was obtained, there was no evidence of any serious illness of the sort; which shows how mild, or latent, it may be.

The matter is of considerable importance; and it is much to be desired that there should be means of readily identifying attacks of pancreatitis, even though slight. If this can be done, much can often be effected by appropriate treatment. In some cases, where chronic pancreatitis has been recognised, a cure has been effected by surgical operation directed towards the gall-bladder, draining it and removing any calculi present. In most of these cases there has been jaundice, which has served to direct attention to the primary seat of the disorder. In a few cases, I believe, a simple laparotomy has led to recovery. Still, there is no need to resort to surgical interference unless the attacks are frequent and severe, or of long standing. Milder ones can probably be overcome by simple medical treatment. It would be more satisfactory, however, to be able to distinguish them with certainty, and to know whether they are invariably dependent on the presence of gall-stones, &c.

TUBERCULIN IN DIAGNOSIS AND TREATMENT.

By W. Camac Wilkinson, B.A., M.D., Lond., F.R.C.P.

Infectious diseases of specific origin are the result of an affinity existing between the specific cause or its product, or both, and the tissue elements of the man or animal. The result of the disease depends on the action and reaction of these two essential factors. If the specific cause is very virulent or very abundant, it overpowers the vitality of the invaded tissues, and destroys the organism. On the other hand, the resisting energy of the tissues may overpower the agents of the disease, and either destroy them or neutralise their dangerous products. Tubercle bacilli, the specific cause of all forms of tuberculosis, manifest a remarkable affinity for certain parts of the human

body, notably the lungs, and the result of tuberculous disease of the lungs depends on the virulence and number of tubercle bacilli and the resisting energy of the invaded tissues.

In the treatment of such an infectious disease there are two rational methods, one directed against the specific cause, and therefore called specific, the other aiming rather at strengthening the resisting energy of the invaded tissues. These two distinct methods of specific and general treatment should be combined if we wish to get the best results.

The specific method of treating pulmonary tuberculosis consists essentially in the use of tuberculin in its various forms, which helps to destroy the bacillus and to neutralise its toxic effects. This method is in fact a process of active immunisation, entirely different to the antitoxic treatment of diphtheria, in which the antitoxin is supplied ready-made. In principle there is no difference between the tuberculin treatment for the purpose of curing tuberculosis and the use of Haffkin's fluid, or similar agents, for the purpose of preventing for a short time such diseases as plague, cholera, or typhoid fever. If, then, the method is advocated in order to protect a healthy person against a disease—which, in a mathematical sense, he has a very small chance of contracting—it is not unreasonable to urge the use of the method if it offers even a small chance of curing one who has already fallen a victim to the disease. Those who have used and advocated the use of specific prophylactic fluids containing germ products for the prevention of plague cannot, with any show of consistency, oppose the use of tuberculin, which is, after all, merely a vegetable product of the living tubercle bacillus. Many are, in their ignorance, guilty of this strange inconsistency. On the other hand, I may disarm some opposition by saying at the outset that the extraneous use of tuberculin is not the one only indispensable means of curing tuberculosis—even in an anatomical sense. We know of no disease in which death is inevitable. Always some individuals are ordained by nature, or are strong enough, to survive an attack of the most virulent disease. Plague does not kill all its victims; nor yet tetanus or anthrax. This power to resist lies in the tissues. Let us go a little further. This power to resist depends upon the ability of the tissues to produce a sufficiency of antibodies which either kill the specific agency or render their products harmless. It is too often forgotten that the very and only cause of the production of these antibodies in the tissues is the specific cause of the disease. *The disease works out its own cure by means of its own cause.* Not only the disease, but even the mechanism of immunity that cures it, are both the results of the living germ through its specific products. Except in this way, no infectious disease cures itself or protects against itself. Even when we seek to strengthen the energy of tissue, so that the disease may be resisted, we are blindly assisting nature to work a cure in nature's own way. In pulmonary tuberculosis sanatorium treatment, open-air treatment, the rest cure, and other methods act by stimulating the system to evolve the mechanism of immunity in response to the action of the products of the tubercle bacilli in the tissues, the most important of which is the tuberculin. It may be a shock to some of the advocates of sanatorium treatment to hear that they themselves, without knowing it, have been innocently vaunting the efficacy of tuberculin in the treatment of tuberculosis. On the other hand, those who advocate the artificial supplementing of nature's own direct weapon of defence are quite consistent in upholding the value of other methods, especially sanatorium treatment. Daily experience tells us that pulmonary tuberculosis is a disease that too often fails to work out its own cure. Some fault in the mechanism of immunity allows the germ to continue its work of destruction, or, maybe, to lie in ambush for months and years. The whole purpose of treatment is to stimulate the tissues, to build up and

finish the mechanism of immunity, so that the tissues shall no longer be at the mercy of the tubercle bacillus. *A priori*, it seems logical to stimulate the mechanism by means of the specific products called tuberculin : *a posteriori*, other methods evolved through empiricism are vaunted as the *ne plus ultra*, and specific methods are held to be superfluous.

Unfortunately there is no royal road to an accurate estimate of the value of treatment in a disease so irregular in its onset, so complicated in its nature, and so variable in its course as pulmonary tuberculosis, and the best of us may hardly avoid the pitfalls of inconsequent and circuitous reasoning. Least of all can we judge of the effects of treatment if we report and exhibit our immediate successes. Those who have had a large field for observation are at times staggered at the vagaries of the disease. The science of recent years, especially through the labors of Vagedes and Kossel, working under the inspiration of Koch, tell us in unmistakable language that one of the chief causes of variation has been largely ignored—the variable virulence of the essential factor, the tubercle bacillus. In the old days the natural explanation of the variable course of pulmonary tuberculosis was found in the over-rated element of predisposition. The tendency of the old school is still to labor the question of predisposition. The tendency of the new school of heretics is rather to give due weight to the definite, tangible, and measurable actor—the infective agent in all its variations and vicissitudes. Naegeli's statistics would seem to show that we are all predisposed : *Tedermann ist ein bisschen tuberculos.* The strongest athlete, in the heyday of health and youth, may be just as little immune as the slender weakly factory girl, with the flat chest and so-called phthisical habit. The former may succumb to rapid and acute phthisis ; the latter may show only an inert localised lesion. Predisposition at least is a quality that cannot be estimated, nor yet eliminated.

It is difficult beyond ordinary comprehension to give a right value to the effect of any method of treatment in a disease that runs such a variable course as pulmonary tuberculosis. The disease may exist for months and years without arresting the attention of patient or physician. It may become quiescent at any time ; it may relapse after months or years ; it may reach a serious stage involving the greater part of a lobe, and then end in perfect restoration of health. In spite of treatment of any and every kind, the disease may never relent ; the victim knows no respite till death ; without any treatment, the disease may come to a standstill, and never again disturb the health. The most affectionate regard for logic may not restrain us from attributing success to our own small efforts and failures to the vagaries of the disease. Our memories seize fast hold of successes ; we leave others to record our failures, or leave them unrecorded. Let us adopt what measures we may, more or less failure is the rule, and yet we place a high value on many of our methods. In no other disease is it so difficult to estimate fairly the effect of treatment upon the course of the disease as it is in pulmonary tuberculosis. One sees cases of pulmonary tuberculosis, even complicated with tuberculous ulceration of the larynx or epiglottis, get well of themselves without any special treatment, without sanatorium treatment, without injections of any sort, without local applications. The best results of sanatorium treatment do not present such a satisfactory picture of success that we should rest satisfied. Analyse the statistics of Weicker's Sanatorium, or Englemann's more extensive analysis of the results in German sanatoria. We shall have to travel far before we can be satisfied. To paraphrase Lord Rosebery's epigram, the physician who is satisfied with sanatorium treatment is lost. The rational method of Brehmer was sound as far as it went, though often laborious and tedious in practice, but no other method was possible in his day. Sanatorium treatment holds out much hope of success, especially

in the early stages of the disease; but surely any method that fails in at least 50 per cent. of the cases does not satisfy the conditions of an ideal method. The plague, dreadful as it is, is more merciful—it kills quickly, but it hardly kills 50 per cent. of its victims. The results of sanatorium treatment disclosed in Weicker's careful tables shows that there is plenty of room for improvement.

In the first place, early diagnosis is the key to successful treatment. In the early stages the lesion of active or latent tuberculosis may produce physical signs like those of obsolete foci, and tubercle bacilli may not yet have escaped from the tissues; the lesions are not tuberculous, but part tuberculous. How can the physician attempt to unravel the mystery of these various lesions? If the tubercle bacilli have disappeared, there can be no tuberculosis; and if there is no active tuberculosis, there is no reaction to tuberculin. That is a vital point. Tuberculin alone gives the clue, whether the lesion is active or inert. If the focus is inert, it may harbor tubercle bacilli that will become active at some future time. Yet, if the tuberculin reaction fails, it is likely that the tubercle bacilli are so surrounded that the tissues can themselves destroy the bacilli. On the other hand, a reaction to tuberculin proves that the lesion is not inert. This reaction is a danger signal of the first importance. It warns the physician of a danger hidden from sight, like the fog signal warns the sailor of a danger in a mist which the eye cannot penetrate. By the systematic use of tuberculin we may learn the true nature of many of those lesions which Naegeli too readily assumed to be tuberculous. Many of these lesions may be merely passive, quiescent, but still ready to be kindled into activity by inflammations or infections. Sad to say, these closed lesions thus neglected may become the source of the acute and rapidly progressing forms that develop in early adolescence. Now, if these terrible disasters may happen, there has been no cure, even though the lesion is for a time closed and inert. By no other way than by the systematic use of tuberculin can we distinguish these latent cases from absolute cures; and if tuberculin is not used, no one has a right to use the term *cure*. If the tuberculin test is negative, and after an interval of three months or six months again negative, there is some certainty that a cure has been accomplished. When the prejudice against tuberculin has died its natural and inevitable death, one of the most valuable uses of tuberculin will be found in determining the effect of treatment of any and every sort. Meanwhile, except by those who use the test, the term *cure* must be avoided. It is satisfactory to note that the Tuberculosis Congress in London, 1901, deliberately adopted the opinion that tuberculin was an invaluable agent in diagnosis. The education of English opinion needed ten years before the truth of Koch's original statement was recognised.

Sanatoria cannot dispense with tuberculin as a diagnostic agent, and gradually tuberculin is taking its right place in all the sanatoria of Germany. If tuberculin is not used, mistakes must occur. True cases of tuberculosis are overlooked; and, on the other hand, there is the risk of condemning a man to an expensive and irksome course of treatment for a disease which he has not got. In one year Weicker rejected forty-eight spurious cases already diagnosed by good examiners as early cases. In the eager chase after early cases, otherwise trustworthy examiners may condemn to sanatorium treatment many cases in which the diagnosis rests not upon trustworthy evidence—such as tubercle bacilli in the sputum, or a definite reaction to tuberculin—but rather upon the subjective impressions of the observer as to the character of respiratory movement or auscultatory signs. The tuberculin test alone can save the profession from the yes-no diagnosis. But for the tuberculin test, Weicker might have greatly improved his statistics by including the

rejected cases, and ultimately *recorded them as cases of cured tuberculosis!* Christian science can do as much for these cases as the best sanatoria. Let us take care we give no such chances to the Christian scientists to show their skill. It is an easy off-hand criticism to offer that tuberculin in diagnosis may lead to the inclusion of cases of latent tuberculosis that may never further develop. The same may be said of many of the cases upon which the statistics of sanatoria are based. Who will dare to say that it will not develop, if a reaction to tuberculin occurs? On the other hand, I have saved many persons the expense and inconvenience of exile to the country, to which they had been sentenced by various medical men. Further, when one uses tuberculin for treatment also, if perchance tuberculin has given a doubtful or false indication, the mistake is soon remedied. Thus, if the reaction be no greater than 99°.5-100° F., and tuberculin is given for treatment, a constant absence of reactions with larger doses rapidly increased arouses suspicion. In my experience with proper doses a temperature of 100° is positive. A rise of temperature to 99°.5-100° is doubtful. *Tuberculin is quite harmless if there is no tuberculosis.* Why, then, should we fear to use it in the cause of sanatorium or other methods of treatment, when it surely reveals the presence of tuberculosis lesions? I have no hesitation in saying that tuberculin is an invaluable agent in the selection of suitable cases for sanatoria, and also a ready means of gauging the success or failure of treatment. A more open question is the value of tuberculin as a curative agent. It is extremely difficult to measure the value of different methods of treatment in a disease that runs such a variable course. For any and every method of treatment successes will be claimed. The only test is the *relation of successes to failures.* If we investigate the statistics of sanatoria, we find that the successes of to-day are the failures of to-morrow. Every medical man has had successes, but far more failures. Records are practically worthless *unless they show that after an interval of time—say two years—the patient is the better for treatment.* It is the tradition of sanatoria to publish results as soon as patients leave the institution. This pernicious practice must give rise to misconceptions and disappointments. Dr. Sinclair is an expert in sanatorium methods, and I fully recognise the humane and unostentatious labor of the last two years of his busy life. He knows too well the vagaries of the disease he has to deal with to raise hopes that may not be realised. He knows as well as I do that the immediate results of sanatorium methods, as of other methods, are satisfactory in every point but the most important—*they do not last.* The sequel to the chapter of improvements that stand recorded when the patient leaves the sanatorium must be stated with equal truth, and, sad to say, is a long wearisome chapter of relapses terminating in death after one or several years in the majority of cases—even though at the outset special care is exercised in selecting cases favorable for treatment.

Anyone who has studied the analysis of 30,000 cases, or thereabouts, treated in German sanatoria, will hardly find occasion to boast of brilliant success. The fruit of the English sanatoria and of our own sanatorium in New South Wales is not yet ripe for analysis—I said so in 1903, Ransom says the same in 1905. What is true in 1905 was doubly true in 1903, though I was laughed to scorn when I made the statement. Engelmann's statistics show that of the cases in Stage I. 7.6 per cent. were disabled or dead in one to one and a half years; 19.7 per cent. in two to two and a half years; 33.3 per cent. in three to three and a half years; 55.6 per cent. were disabled or dead in four years. In all stages the records were infinitely worse: in one to one and a half years 25 per cent. were dead; in two to two and a half years 38.4 per cent.; in three to three and a half years, 55 per cent.; in four years the deaths amounted to 66.7 per cent.

Moreover, the failure of present methods is written large in the black letters of the statistics of every country. The after history of 20,878 cases treated in 1897 showed the following permanent results (Engelmann). In 1898, 66 per cent. permanent results ; in 1899, 48 per cent. permanent results ; in 1900, 39 per cent. permanent results ; in 1901, 30 per cent. permanent results. In Weicker's statistics—2,469 cases. In following year, 55 per cent. permanent results ; in 2nd year, 50.7 per cent. permanent results ; in 3rd year, 41.4 per cent. permanent results ; in 4th year, 35 per cent. permanent results.

Two years after treatment 25 *per cent. are dead* ; *four years after treatment* 50 *per cent.* ; *and six years after treatment* 63 *per cent. are dead.*

Even Dr. Sinclair has been premature with his records. When I published my results in 1902, my work extended back five years and more. Dr. Sinclair's extend back hardly more than two years, and most of his successes have been reported within a year of treatment. The condition of time has not been fulfilled, and his records can at most form a starting-point for further observations. Many of the apparent successes of to-day may prove the certain failures of to-morrow. Still, the after history of these recorded cases year by year will show the truth about the results of sanatorium treatment.

We learn already from Dr. Sinclair, as I insisted years ago, that a three months' residence in a sanatorium may be well enough from an insurance point of view, since it may restore the industrial competency of a workman for one or more years ; but in three months one cannot expect to arrest the disease, and it is arrest of the disease which the workman is led to expect and desires. The workman wishes not only to be able to work, but to recover his health. In most cases the work of sanatoria is patchy and unfinished. Patients are discharged—improved, no doubt—but with the disease still potential, if not active, and merely waiting a favorable opportunity to reassert itself. My purpose at this Congress is to bring forward reasons for advocating the use of tuberculin as a remedy, so as to bring the work of the sanatorium to a higher level of efficiency and completeness, for the benefit of the unfortunate victim of pulmonary tuberculosis—and even of the whole community. The better the work of the sanatorium, the better for the community ; and at the present time the inmates of our sanatorium are denied the advantages of tuberculin treatment that are enjoyed by patients in some of the best German sanatoria. The ideal treatment is specific treatment combined with those general methods adopted in the sanatorium that are of undoubted value in increasing that tissue energy upon which the success of tuberculin treatment depends. Weicker lost hope after a careful experience of the ordinary and best sanatorium methods of Breehmer ; he weighed sanatorium treatment in the scales, and found it wanting. He tried tuberculin, and, combining both methods, he has taken heart again. Another very great authority, Mœller, has become a convert to our side, but he states that he uses tuberculin especially when sanatorium methods absolutely fail (see Zeitschrift, für Tuberulose) ; and in such cases he has been astounded at the splendid effects of tuberculin. My simple criticism of this somewhat half-hearted advocacy is that surely, if tuberculin has been proved by him to be of enormous value as a curative agent, when *sanatorium methods have failed*, there is no *a priori* reason why it should be far more successful in those cases in which sanatorium methods do good.

Years ago, when I was making my early observations with tuberculin, I studiously avoided the introduction of factors that might vitiate my conclusions. Accordingly I instructed patients to live under the same conditions to which they had been accustomed. Having thus proved the uniform im-

provement could be only due to the new factor—the tuberculin—I lost no time in adopting all measures that might reasonably be supposed to help in the arrest of the disease and the cure of the patient.

Strange as it may seem, I am the only specialist in diseases of the lungs in Sydney who has gone to the trouble of providing sanatorium treatment for private patients—even though I consider the sanatorium methods of subordinate importance. Other medical men, who openly proclaim that sanatorium treatment is of primary importance, have taken no steps to secure for their private patients that method of treatment which they tell the world is the best, and, indeed, the only treatment. It is idle to pretend that the haphazard exile of patients to the country constitutes sanatorium treatment. I leave it to the over-zealous advocates of sanatorium treatment for the poor to explain why it should not be equally successful in their well-to-do and rich patients. Let those who wish to be enlightened upon the great virtues of sanatorium methods read Mr. Ransom's recent article in the *British Medical Journal.* He quotes Walther, of Nordrach, to this effect, that if by cure we mean that all subjective and objective signs of disease have disappeared, no more than 11.20 per cent. can be so classed, although improvement occurs in 70.80 per cent. of the cases. Ransom writes himself, "I think it may fairly be said, as the result of a large mass of continental statistics, that only about 30 per cent. of patients discharged from sanatoria for the poor maintain the capacity for work more than four years." He also writes, "In England sanatoria for the poor have existed too short a time (writing himself in 1905) to allow of statistics of much value being collected from them." I said the very same thing quite two years back—in 1903. Later Ransom says, "Exactly half the patients discharged have maintained the improvement gained for over a year, while half have got worse or are dead."

Professor Mœller, of the Belzig Sanatorium, near Berlin, and a recognised authority on tuberculosis, six years ago an utter sceptic upon tuberculin treatment, is now one of its most earnest advocates. He tells us that, in cases in which sanatorium methods have entirely failed, tuberculin has proved to possess undoubted curative properties, and that while *sanatorium treatment never cures in three months*, permanent cures are often obtained with tuberculin. In his first record Mœller showed gross results :—Sanatorium methods alone, 10.9 per cent. cured ; sanatorium methods and tuberculin, 36.3 per cent. cured. In the different stages :—Stage I. Sanatorium methods alone, 31.8 per cent. cured ; sanatorium methods and tuberculin, 75 per cent. cured. Stage II. Sanatorium methods alone, 1.9 per cent. cured ; sanatorium methods and tuberculin, 20 per cent. cured. Stage III. No cures possible, except as curiosities.

According to Mœller's later experiences sanatorium treatment may cure pulmonary tuberculosis in the first stage (Berlin Board of Health system) in 25.30 per cent. of the cases, while tuberculin in his hands has secured the same result in 84.6 of the cases. I venture to say that when Mœller uses larger doses, or repeats the course, he will find that tuberculin will cure almost 100 per cent. of the cases. Thus, I hold with our great teacher, Professor Koch, that *tuberculosis of the lungs in the first stage can be cured with certainty by tuberculin.* In the second stage, which Mœller maintains is never cured by sanatorium treatment alone, tuberculin gives *permanent results in from 40 per cent. to 60 per cent. of the cases.* Even in the third stage tuberculin sometimes yields remarkable results that cannot be approached by sanatorium methods. When these splendid results are claimed for tuberculin by those who have had an extensive and prolonged experience with it, are we

going to be so foolish as to pay any heed to the carping surreptitious criticism of those sterile authorities who have the hardihood to express adverse opinions on a matter concerning which they have no personal knowledge ?

In medicine, as in surgery, pioneers have often had to bear much unfair criticism. Experience is our great teacher. I do not know any great authority who has used tuberculin, with the conditions and limitations laid down by Koch, and found tuberculin wanting. On the other hand, I have seen no carefully compiled records that prove tuberculin to be either harmful or useless. We do not condemn operations because novices and those who have not learned their art make mistakes and fail. There have been terrible tragedies arising from various operations in their early history, but the operations have survived. The failures and tragedies associated with tuberculin are not the fault of tuberculin, but the fault of those "fools who rush in where angels fear to tread." Failures may be the stepping-stones to high success, and the failures of to-day are converted by increasing knowledge and experience into the successes of to-morrow.

SECTION OF SURGERY.

ABDOMINAL SYPHILIS.

By Frederick Dougan Bird, M.B., Ch.M., M.R.C.S., Eng., Melbourne.,

President of Section.

While appreciating to the limits of my capacity the honor which allows of my presenting myself to you to-day as President of the Section of Surgery, I am deeply diffident of my ability to interest you in a presidential address, and I can only beg you " gently to hear and kindly to judge " my attempt. On account of the size of the subject, and the fact that the various professional annuals do the thing so much better than I can, I have avoided attempting a review of the present surgical position, and have chosen a special subject which has attracted my attention for a good many years. In the abdomen, that portion of the body which shows the surgeon the greatest help in his work, and also possibly his greatest difficulties, and which has a perennial interest of the highest order, I have found the conditions about which I wish to speak to-day.

While syphilis has been made responsible by various authors for most things, from the production of cleft palate and club-foot to the filling of our lunatic asylums, but little has been written in comparison about the manifestations of this disease in the abdomen. Mistakes are so easily made in connection with syphilitic lesions in the abdomen, and these mistakes so generally mean greatly increased risk to the patient from operation, that I need not ask your pardon for endeavoring to elucidate to some extent these conditions from the thin thread of my personal experience. A somewhat systematic method of considering these lesions is the best way of detailing to you what I know from the study of a number of cases.

In returning to the ancient saintly habit of contemplating the navel, I must admit that while I have seen epitheliomatous and tubercular diseases, fæcal and urinary fistulæ, and dermoid developments here, I have not encountered anything that I took to be syphilitic at the umbilicus itself. However, round about the navel I have met with true subcutaneous gummata, the size of small marbles, fairly defined movable growths, slightly tender, which simulated multiple sarcomata closely. They appear just about the level of the waist, and may be due to the dragging of the clothes in the female, which, acting so continuously, amounts to traumatism. Similar, but smaller, subcutaneous gummatous nodules are well known about the female knee. and their position is similarly determined, I believe, by the irritation of the garter. When the breast is voluminous, and weighs much upon the upper limit of the corset, subcutaneous gummata may form in like manner along the line of pressure.

Increased experience makes me allow increasing importance in my mind to the influence of traumatism as a localising factor in the production of syphilitic manifestations. These strictly subcutaneous gummata are mainly important from their showing that their owner has the specific virus still in

his or her person. A single specimen may be removed to make quite sure by microscopical examination that it is neither one nodule of a sarcomatosis nor an outlying metastasis of an unobstrusive gastric or mammary cancer.

Considering next the parietal wall itself, I have had the good fortune to see a number of tumors (it is hard to call them otherwise) in the parietes due to syphilomatous growth. These could have been divided into cases where the parietes alone was implicated, and cases where underlying structures were infiltrated and by being fixed to the wall where the syphiloma existed formed part of the swelling. The first-named are so like sarcomata of the abdominal wall as to surely cause mistake on the part of the surgeon if he is not cognisant of the syphilitic possibilities of the case. And a mistake of the first order it will be if he operates for the removal of the supposed sarcoma, for if the swelling is of any but small size, and it generally has considerable extension, the patient's abdominal wall is grossly injured past repair by the cutting necessary for its removal.

It is very hard to believe that this discoid well-defined lump can possibly be anything but a circumscribed tumor; and unless the knowledge of the patient's past tells us, or the finding of undoubted traces of syphilis elsewhere puts us on the right diagnostic track, no certain view can be taken, and an incision into the growth is necessary for the removal of a portion of the tissue for the microscope. This insuperable difficulty in clinical diagnosis certainly exists in the hard example of parietal gumma, which takes such a time as a sarcoma might take to grow, though of a shorter existence than a fibroma; but all the cases I have seen were not so dense and fibroid. These softer ones may grow more rapidly than any sarcoma, and may disappear as rapidly. In one case in particular was I much astonished at the marvellous disappearance of these masses. It was that of a married woman, *æt.* 46, who had several apparently healthy children, had had one miscarriage, and presented no signs of syphilis elsewhere. She came to me slightly jaundiced, with a tender swelling in the gall-bladder region. This swelling was slowly travelling in the direction of the umbilicus in the same manner as an enlarging gall-bladder would. However, the swelling implicated the parietes, and my diagnosis was cholecystitis, with cholangitis and the probable presence of gallstones. Operation showed me infiltration of the parietes, to which were welded on the gall-bladder and a portion of colon. The mistake was easily made by me, as I have seen cases very similar in which gallstones were trying to extrude themselves at the umbilicus.

Though I was now used to recognise these syphilomatous growths, I might have easily failed in diagnosis at operation if it had not been for the peculiar character of the infiltrated rectus muscle, which gave a gritty feel on section. This is a marked characteristic of muscle tissue embarrassed by syphilomatous change. The sensation given to the hand through the knife on section of such a muscle is highly reminiscent of the cutting of beef which has been badly corned, and the appearance, which shows much increase in the stroma of the muscle, suggests, through another sense, the likeness to corned beef. This similarity revealed to me the right diagnosis, which was confirmed by D. Mollison's microscopic examination of a portion removed.

Though I did not open into the gall-bladder in this case, bile came through the wound in a few days' time, and continued to flow for several weeks. Now this lady during her convalescence developed rounded masses in various parts of her abdomen, which masses undoubtedly infiltrated the parietes and deserved the name of phantom tumors, because of their fitful appearance and equally capricious disappearance; they were much softer than the original tumor, and caused me much exercise of mind at first. I would mark one down on Monday, and on Wednesday it would have vanished, a similar tumor

developing elsewhere in the abdominal wall. My concern at the first was that I should incise these lumps, but, fortunately, they were too evanescent. All this time the patient was being energetically treated with iodide of potassium and mercurial inunction. This lady regained her health completely in about four months, and, as seems to be the rule in these unusual manifestations of syphilis, no other undoubted evidence of the disease could be obtained.

Another illustrative case was that of a young married woman, who developed a considerable tumor in the gall-bladder region. This was operated on in a country hospital, where the surgeon, a very excellent practitioner, found such a jungle of adhesions that, after some exploration, which did not discover gallstones, he closed the abdomen. The patient recovered wonderfully, the tumor disappearing, all but some thickening beneath the scar. In a few months' time, however, her health failed again, and the tumor returning her doctor sent her down to me. The history of the case and the parietal infiltration constrained me to the right diagnosis, which was confirmed by the removal of a small portion for the microscope. Complete permanent restoration followed drug treatment. The power of exploratory operation by itself to almost effect dispersion of the syphilomatous condition was of great interest to me in this case.

The existence of the syphilomatous process in the tissues does not necessarily interfere with the production of a good scar made for diagnostic or exploratory purposes, as in only one case that I have seen has there been a tendency to weakness of the cicatrix.

It is equally embarrassing when such an agglomeration of syphilomatous material is discovered in the appendical region as occurred in a case under my care some years ago. The patient, a previously healthy young fellow, had a deranged alimentary canal, and was evidently ill. He drew my attention to pain and tenderness in the right iliac fossa, where I found a swelling such as is often seen in appendicitis, with a good barrier formation. The diagnosis was made, and as the swelling gave no signs of disappearing, at the end of a few days an incision was made into it, but I did not open the peritoneal cavity, as the muscle was in a condition which immediately turned my thoughts to syphilis. The microscope proved that their direction was right, and the patient regained his health, with the evanescence of the tumor under liberal anti-syphilitic medication. In this case the central portion of the tumor was hard and immovable, but the peripheral parts changed in a most perplexing manner. Here, again, no history or vestige of syphilis was to be found. Both these cases suffered much in health with the formation of these strange tumors, while previously to the appearance of these swellings their health had been excellent, although they must have been syphilitic for some considerable time before the evolution of syphilomatous masses. A deep etiological interest attaches itself to a further consideration of these cases. In a person admittedly syphilitic, though in apparent good health, does some accidental lowering of health cause the formation of these growths ? Or is there some localising factor, probably an unregarded traumatism, which determines the agglomeration of small round cells, that in their turn, by their morbid metabolism, poison the patient, and thus reduce his health ? The latter, I believe, is the true record of the loss of health and strength which is so noticeable in these cases. It is only likely that the vast collection of small infiltrating cells will give rise to strange metabolic results, which are of the nature of a poison, especially when the tumors formed by them may be so wanting in stability in all or part of their diffusion.

These parietal tumors occur not only in adults, but also in infants and children as developments of inherited syphilis. In one case during the time

I was treating the father for tertiary syphilis (stenosing the right bronchus), the mother gave birth to a child apparently healthy. In about a fortnight the infant became sickly, and a lump came into existence to the right of its umbilicus. This lump grew rapidly to a large size when compared with the abdomen, and incision with removal of a portion demonstrated the syphilitic nature of the swelling.

Under mercurial treatment the child flourished ; but the father died of his disease. Fortunately here, with the undoubted syphilis of the father before me, I avoided the danger of performing an extensive mutilating and unnecessary operation on the one hand, and the hopelessness of allowing the child to die of what I might have considered rapid sarcoma on the other. The mistake of resorting to a large operation which is not needed is very likely to occur in cases where the tumor is hard and confined to the parietes. Moderate incision into them with the giving of mercury and iodide is the best treatment for these hard parietal tumors, and the cutting for the removal of a portion for the microscope gives a start for the absorption of the tumor. Some of the less stable forms are dispersed by mere incision, and it is my opinion that all are more readily disposed of by the performance of an initial incision, which has the added advantage of settling the diagnosis.

The worst treatment is total removal of these tumors, leaving an irremediable gap in the abdominal parietes. Among the many wonderful things we witness in our work, not one, I think, is more startling than the disappearance of these syphilomatous masses of large extent and dense consistence under proper treatment. The likeness to tumor is still more marked when we realise, as I have been able to do, that the central softening so characteristic of syphilis formations is by no means a necessity in these masses. That a single surgeon should meet with the number of cases which have come my way goes to show that these strange syphilomatous swellings are not so uncommon that we can neglect the chance of their being present. From what information I have been able to gather of their frequency, they are more common than sarcoma or fibroma of the abdominal parietes.

There is yet another tumor with which they may be confounded. Myxœdematous deposit, when it occurs in the abdominal wall, is usually spread out impartially in the subcutaneous tissue, but sometimes it may be consolidated into a discrete lump which forms an undoubted tumor, and then the likeness to syphilomatous swelling is great.

Very small indeed is the amount written about gummata of the abdominal wall, but great is the literature concerning syphilis of the liver—that municipal destructor of the body. That syphilis finds a permanent home in the liver is a very old tale, and although we do not credit it with being the primary seat of syphilis, whence ulcers radiate even to the genitals, as Fallopius did, we have large, if vague, ideas of the importance of syphilitic hepatic disease.

These ideas mainly cluster round the congenital forms, and rightly so, as these largely predominate numerically. It is of the acquired form mainly that I wish to speak. Its fissures and scarrings, its lobulations and deformities are well known, and when seen at operation are of obvious origin, as are the cicatrices left by gummata ; but gummata, which have not been absorbed when small, as usually obtains, and have attained large size, will give signs before and during operation which are very confusing. The following is a good instance of how mistaken a surgeon may be. A gentleman of the age of 41 (a likely age for gumma of the liver), leading a healthy country life in a district where hydatids are common, consulted me about marked loss of health, strength, and weight, and a want of symmetry in his ribs. He had noticed for some months that his right side was decidedly and increasingly more prominent than his left, and that exertion was apt to cause pain over this enlarged region. The

rounded smooth swelling to be easily felt below the ribs, the increase of the liver dulness upwards, and its continuity with the evident tumor, decided me to tell the patient that he was suffering from hydatid of the liver. The wasting and the pain on exercise were factors that did not fall well into line in the diagnosis; but I certainly have seen cases of hydatid disease with as much and more pain, and some with wasting, though these last are more likely to be cases of multiple omental cysts. Operation was decided on—much more, I admit, to remove the supposed hydatid than the lingering doubts which existed at the back of my mind. Incision showed a large yellowish-white shiny mass, which even then might be taken for hydatid. However, closer inspection showed that small radiating fibres passed to the liver in many directions from the main mass. The tumor felt solidity itself, and was not quite as well defined as a hydatid adventitia is. A trocar inserted in several places discovered no fluid, and gave no impression of a cavity. The hole made by its insertion did not alter on its withdrawal, and the conclusion was soon come to that it was a very large gumma.

Gummata, like hydatids, tend to seek the surface of the liver; and very large ones must, perforce, abut on the periphery. Both prefer the anterior surface rather than the region of the porta. As the gumma reaches the surface a perihepatitis occurs, and this, of course, means pain, which may radiate, as in other hepatic conditions, to the shoulder tip, by way of the phrenic, or through to the back by the splanchnic route, but which is most likely to be present in the diseased area and its immediate surroundings. There is no doubt that pain is one of the main symptoms in late syphilitic disease of the liver.

Gummata of small and moderate size seem generally to be absorbed even without the influence of drugs, leaving only a scar at their former site; but one of the size obtaining in the present case acts as a malignant tumor: its morbid metabolism poisons by its products, and it is my opinion that this patient would have died in a very few months from absorption of the juices of this vast collection of loosely organised cells. As it was, iodide and mercury were given with the happiest results, local and general, and during his convalescence the patient remembered what he had previously denied—the existence of chancre, acquired some five years before. It may be noted that a longer period than this, as a rule, elapses before the occurrence of gumma in the liver.

Really large gummata—such as the one present in this case—must be rare conditions; but small gummata which cause no symptoms, or only slight perihepatitic pains may be, and probably are, common, and we know of their existence only by cicatrices found *post-mortem*, or, peradventure, at an operation. Part of the surgeon's *metier* in an exploratory operation in the vicinity of the liver is undoubtedly to feel the space between the liver and diaphragm for adhesions or anything that may break the smooth hepatic contour; but he should use more than his sense of touch: he should inspect with his eyes the surface of the liver, searching for any scarring or markings which may not be raised above or depressed below the surface, but which, nevertheless, are highly suggestive of syphilis. These cicatrices, clefts, fissures, and furrows, markings and lines, are well known as the results of liver syphilis; but I think that attention has not been sufficiently directed to striæ much more delicate than these. They are of a pale, transparent, bluish tint, looking very much as if the liver surface had been traced over with a small paintbrush dipped in weak nitrate of silver solution. They are not dense enough to cause any inequality of the surface, either as a ridge or a contrasting depression, and they meander towards the liver edge as rivers in a map to the land's edge. For some years now I always look for these markings in operations, and I have

learnt to associate them with syphilis. In cases where syphilis was suspected I have even added an inch or so to my incision (chary as I am of unnecessary length in it) in order to be able to inspect the liver for these markings. They have to be looked for; but I believe, when found, they are good criteria that the patient is a syphilitic. Again, in syphilitic cases I have several times seen small calcareous masses in the centre of starred cicatrices. The proportion of importance allowable to syphilis as regards the formation of adhesions between the liver and the diaphragm is difficult to allocate, and involves a consideration of the causal agencies of perihepatitis; but my mind runs syphiliswards when I find adhesions without obvious cause in the dome of the diaphragm connecting it with the liver.

In the case just related there was no question of the tumor being a dilated gall-bladder; but a gumma has been mistaken for it, and of course the likeness to secondary malignant disease of the liver is considerable. Primary tumors of the liver are rare, rarer than gummata, so that the possibility of a tumor being a gumma, which is easily removed by medicine, must be taken into earnest consideration before a large and risky operation is attempted in order to remove the growth.

The presence of irritation as localising the deposition of gummatous material is interestingly shown in cases where a growing hydatid adventitia is surrounded by an atmosphere of syphilomatous tissue, and is itself similarly infiltrated. The rubbing of the anterior surface of the liver against the ribs may, when the surface is once roughened, constitute by its continuity a traumatism, and account for the preference gummata have for the anterior aspect. Syphilis is apt to exhibit itself in the terminal portions of an artery, a habit well seen in the branching seaweed-like markings on the inner aspect of the cranium, and this terminal endarteritis may furnish in the hepatic artery the initial roughness which is rubbed into actual gumma by the ribs.

Of those strange cases where the liver is covered by a thick and dense envelope of glistening white fibrous nature, I cannot speak with any definite knowledge. One would think these cases, whether only part of a general chronic proliferative peritonitis, or poliorromenitis, or separate hepatic conditions, were likely to be of a syphilitic nature; but I cannot say whether syphilis is the cause, or even one of the causes, of them, or of the somewhat similar condition in the thorax, viz., chronic indurative mediastinitis, or of a combination of these conditions, which is dignified by the name of chronic multiple serositis. There are, however, two other strange conditions—one involving both thorax and abdomen, undoubtedly syphilitic of the acquired form; the other purely abdominal and, I believe, syphilitic of the congenital form.

Taking the former, we find present a very large area which is blended together into an apparently solid mass, dull on percussion, callous in nature, largely destitute of function, and especially wanting in the movement naturally associated with the parts. A heavy dense cloud of syphilitic tissue has settled down over the parts, entirely and impartially obliterating the salience of the anatomy. Thus the lower half of the lung, the diaphragm, the intercostal muscles, the spleen, the parietes of the abdomen, and any organs near may be obscured and enveloped in this mass. Similar cases of less extent have been operated on as sarcoma, Paquelin's cautery taking the place of the knife, or a hopeless prognosis has been given, and the paralysing fog of syphilis allowed to spread. Here is a case, the recital of which is typical. A. B., a previously strong man, *æt.* 39, complained of severe pains over most of his left side, with much shortness of breath on even slight exertion; but his cough was what bothered him most, for it racked him continuously. There was no accompaniment of expectoration. He was much wasted, very anæmic, and looked

ill. The signs of disease were vested in the painful region, which was all very solid on percussion. Air did not enter the lower half of the left lung. There were no adventitious sounds. The fluorescent screen gave a most interesting, as well as a most unusual, picture. The transradiant area below the diaphragm on the left side was replaced by dense shadow, which extended upwards through the diaphragm far into the lung. While the centre of this was very dark, the edge of the shadow was not sharply defiend, for dark streamers radiated in all directions. The diagnosis lay between huge syphilitic gumma and malignant disease. The exploring needle helped to decide, for its passage in several directions through the part was accomplished without the loss of even a drop of blood, which was very much against the growth being sarcoma, and a beautiful exemplification of the bloodless nature of the central parts of a gumma. The administration of full doses of iodide, in addition to mercurial inunction, rapidly restored the man to health, while the diminishing darkness viewed on the florescent screen afforded a most instructive spectacle, and as this syphilitic fog cleared off, the movements of the left diaphragm could again be seen.

In the second of the two conditions referred to, the syphilitic result had been too long present for drug medication to have much effect, and death ensued in the two cases I have seen. They were wonderfully alike, both young women under 25 ; in both ascites was present in large amount, and in both the superficial abdominal veins and the veins in the flank were much enlarged. One had jaundice. Both were tapped for ascites several times, and on the abdomen filling up again I was asked to explore. This exploration resulted the strangest palpation I have experienced. There was a large amount of fluid, and in this the exploring fingers felt the liver and its pedicle and the spleen as if they were plaster models, so firm and unyielding were they. The edge of the liver was rounded, the gall-bladder partook of the general hardness, and the liver was of a dark purple color, with lighter mottlings and streaks. There was evidently nothing to be done in an operative way ; and a *post-mortem* examination some two months after showed in both cases much narrowing of the inferior vena cava, the walls of the portal vein were densely thickened and its lumen partially occluded by old blood-clot. It was decided by the pathologist that syphilis was responsible for the condition in either case. The disease had been so long in existence, and the damage done was so irreparable, that iodide and mercury administration could do no good. Wonderful as are the effects of these drugs in promoting the dispersal of syphilomata and gummatous infiltrations, a time in the history of these deposits must come when, if they have not softened, a degree of fibrosis is attained upon which iodide and mercury have but little power.

It is pleasing now to turn to a class of cases in which a correct diagnosis, leading to a correct treatment, will ensure a speedy remission of symptoms. While syphilis of the liver has been exhaustively treated of by many writers, the manifestations of the disease in the gall-bladder and gall-ducts have had little said of them. It is many years since I became cognisant of what syphilis might do to a gall-bladder. The first revelation was in the case of a previously healthy young woman, now wasted, jaundiced, with pain over the gall-bladder region, and increased resistance there ; the history extending over many months. The gall-bladder at operation was seen to be studded all over with what were apparently carcinomatous nodules, the cystic and common ducts were enlarged and hardened, and there were many adhesions in the portal fissure. Drainage of the gall-bladder was effected. In doing this I took the occasion to remove one of these raised hardened excrescences with a gristly feeling, and Dr. Mollison informed me that it was a gummatous mass. The gall-bladder shows beautifully how tissues along a definitely marked arterial

tract may be picked out by syphilis, which leaves the neighborhood free at all events of macroscopical disease. Thus evidences of syphilis may be obviously scattered along the cystic artery and its branches, while adjoining tissues owning another arterial supply show no manifestations at all. Here is quite another form of syphilis of the gall-bladder, in which other parts were implicated. A lady, *æt.* 39, began to feel epigastric pain and discomfort about a year before I was asked to operate on her. Her illness gradually evolved into one simulating very closely gall-stone in the common duct. The attacks of pain were typical; her jaundice, which was after a time always present, became much deeper during the later stages of the attack, which lasted several days. She would then make a partial recovery, which was again terminated by an attack. The pain was very great, and was relieved to some extent by vomiting. During the period of comparative freedom from pain she had tenderness in the epigastrium and over the gall-bladder, and once her medical attendant believed that he could feel a distended gall-bladder. She had lost much weight, and was evidently losing ground after each attack. On opening the abdomen I found a yellowish-white, much thickened gall-bladder fundus uncovered by the liver edge. It was unusually firmly connected with the liver. The thickening extended to the cystic duct and the common duct, in which, however, no stone could be felt. There were a good many adhesions about the portal fissure. The ducts were hard and dense to the feel, like a lead pencil. Cholecystotomy was performed, and a small portion of gall-bladder wall was removed for the mircoscope, by means of which Dr. Mollison found marked evidences of endarteritis. Bile percolated slowly through the wound for a fortnight. I could not help remarking that the changes in the gall-bladder were mainly in the outer and middle coats and not in the mucous membrane. The gall-bladder was not dirty in its inside, as at first inspection would have made one expect. There were no stones or débris at all. During the later stages of the operation I came to the expressed conclusion that the case was syphilitic. She improved much with mercurial inunction and iodide, getting fat and well, although her jaundice took a long time to pass off. Now comes the interesting part of her history. On leaving the care of her own medical man she went to a distant part of the State, and gave up her treatment after about six weeks. Gradually the jaundice came back, the attacks of pain reinstated themselves with much regularity every three weeks, and in three months' time she was nearly as bad as ever. She then came under my care in Melbourne, and a vigorous rubbing in of ung. hydrargyri twice a day with large doses of pot. iod. by the rectum have restored her to apparent health. I have spoken about the duct feeling like a portion of lead pencil; it is in my mind that this is a distinctive syphilitic phenomenon. I have met with it in several cases which I believe to be syphilitic, and which were improved, and seemingly cured, by antisyphilitic treatment; but as I had no microscopical confirmation of their nature, I do not wish to press their acceptance. It is difficult to be sure how jaundice is brought about in such a case as I describe; it may be due to one or more of the following conditions—gumma pressing on and involving the common duct, adhesions kinking the common duct, syphilitic infiltration and hardening of the head of the pancreas, syphilitic infiltration or ulceration and contraction inside the common ducts, similar conditions in the intra-hepatic ducts; cicatricial deformity of the liver may produce such dragging on the duct that its lumen may be insufficient for the passage of bile; and, lastly, syphilitic adenitis in the portal fissure may furnish the requisite pressure on the duct. The jaundice of the secondary stage of syphilis, which, I take it, is commoner than this tertiary jaundice, and which is seen concomitantly with the roseolous skin, is also difficult of explanation, though, we can here add, a specific cholangitis and a condyloma at the papilla to the

other possibilities of production. I have also seen jaundice in the cases of enveloping gumma, where many structures are involved, and here pressure of the gummatous material outside the ducts, intra and extra hepatic, probably accounts for the condition. These latter cases seem commoner from my own experience, and what very little of literature there is, than syphilitic manifestations confined to the gall-bladder, ducts, and portal fissure. Jaundice and syphilis are not linked ideas in one's mind as jaundice and cholangitis, or jaundice and stone, jaundice and cancer of the head of the pancreas; but certainly a due association should exist, not because jaundice in tertiary syphilis is common, but because if the cause is not recognised and treated the patient will die; whereas the exhibition of mercury and iodide will certainly relieve and apparently cure, if used before the gummata have formed too dense a fibrous capsule and the infiltrations have resulted in too permanent a fibrosis: and I believe both these pathological terminations are reached only after a long period. Even the jaundice of secondary syphilis has dire danger if undiagnosed, as its tendency is to persist for a long time, and a very long continuance in some cases is associated with acute atrophy of the liver.

I have left in Melbourne a patient exhibiting yet another variety of gall-bladder syphilis. She is a middle-aged lady, who has been wasting much and departing from health for many months. Four weeks ago she noticed a swelling in the right lower abdomen. It was not very painful or tender, but steadily enlarged. When I saw it, carcinoma of the cæcum seemed the most likely diagnosis. The lump was hard, nodular, and very movable, laterally and upwards. She passed a good deal of blood and mucus at times, and had much constipation. Incision showed a deformed liver of the saddle-flap variety, with, in addition, a large flattened Reidel's lobe, at the extremity of which was a much distended yellowish-white gall-bladder. This contained more than 2 ounces of clear mucus; its walls were fleshy and very thick, even to half an inch or more, the mucous membrane participating but very slightly in the thickening. The wall was of the same yellowish whiteness as the surface all through. Encysted in the neck was a large gall-stone. The liver being well dislocated, I could satisfy myself that the ducts were macroscopically healthy and contained no other gall-stones. The deformity of the liver was largely accounted for by the presence of fibrous trabeculæ meandering through it in many directions, giving the appearance of an early stage of botryoid liver. The gall-bladder was removed, and made an interesting specimen, which left doubts in one's mind as to whether the great mural thickening was due to ordinary chronic inflammation, carcinoma, or syphilis. It may be remarked that the greatest depth of wall was not where the gall-stone reposed. Dr. Mollison is certain of the syphilitic nature of the specimen, and the patient is doing well on the usual medication. There can be no doubt, I think, of the propriety of classing syphilis as one of the causes of gall-bladder enlargement.

It may be permitted me to interpolate here the mention of an excellent example of the well-known "botryoid" liver, which is the untreated outcome of gumma and gummatous infiltration of the liver, which, when affecting the left lobe, may give rise to very remarkable epigastric tumor or tumors, and if the right lobe is mainly implicated, masses simulating a gall-bladder stuffed with stones or cancerous nodes. The patient, a sea captain, *æt.* 39, with a syphilitic history, complained of severe pain in the epigastrium and vomiting. He was not jaundiced, but of a remarkable earthy color. A rough irregular swelling could be felt below the ensiform cartilage, and the palpable edge of the right lobe was of a similar character. Any indiscretion in diet, especially alcoholic excess, brought him nearly to death's door with hæmatemesis and prostration. In his fatal attack the question of opening his stomach and

getting at the ulcer, which Dr. Maudsley and I predicated as the most likely source of the hæmorrhage, obtruded itself. However, he sank so rapidly that operation was not permissible, and an autopsy failed to show any ulcer in the stomach, but the gastric and œsophageal veins from which the fatal bleeding had come were very large indeed. The liver was much deformed all over, and its advanced pathology seemed hardly compatible with the periods of fairly good health which the patient enjoyed when he took iodide and care of his diet.

In trying to pursue the somewhat systematic scheme which I suggested at starting, we may now consider stomach syphilis; and some of its manifestations are truly marvellous. If a man who knew the appearances of ordinary syphilis in other parts of the body were to imagine for himself the most remarkable forms he could picture for gastric syphilis, "the brightest heaven of his invention" would probably contain no more extraordinary tumor than that which occurs in the gastric wall in some rare cases of syphilis. I do not refer to those cases where parietes, gall-bladder, pylorus, and other structures are enveloped in a cloud of gummatous material; such a case I have seen, and they fall into line with cases I have already described, differing only in taking in the stomach as well as other parts. In the instances I wish to dilate upon a large definite solid new growth occurs in the stomach wall; it does not lose in density from its centre to its periphery; it does not gradually fade away into normal tissue, but it has its solid bosses and firm partially detached masses, with as clear-cut edges as the carcinoma which it so strongly resembles. It is a separate, distinct tumor, which can be picked up between the fingers—a performance impossible in connection with the widely diffused gummatous cloud wrapping together several separate tissues. It is very like cancer or sarcoma to look at, and, with the exploring finger only, I think a differentiation cannot be made. It is this strong physical likeness to malignant disease, quite apart from similitude in symptoms, which renders the surgeon cautious to an advanced degree. The following case is a handsome example:—

Mrs. W., *æt.* 36, had enjoyed good health until two years before she consulted me. At this time, gradually, began dyspeptic symptoms and, concomitantly, she lost weight, strength, and her robust appearance. There was no obvious cause for all this, for she was and had been leading a healthy country life. This downward course was slow, until about three months before I saw her, when she began to have pain and much tenderness in the region just below the eighth costal cartilage, on the left side, and here a lump was soon to be felt. Her health now deteriorated rapidly; she became very anæmic and distressed indeed; she could hardly walk. Her pain was continuous, though worse at night when she lay down. She could scarcely bear her clothes over the tumor, so tender was it. A hurried examination showed that the abdominal wall moved fairly well, except in the region where the pain was. As she was very wasted the rib margins stood out in strong relief, and below the left one a distinct tumor was visible and palpable. It was irregular in shape and gave somewhat the sensation of an inflammatory swelling, but no fluctuation could be felt in it. Respiration did not affect its position; it was not movable within the abdomen, and attempts at moving it caused intense pain. No glandular enlargement was noticeable anywhere. The patient's temperature was raised to 101° Fah., but she had had no rigors, and there was no redness of the skin over the tumor. She had no vomiting, and it seemed quite inadmissible to obtain stomach contents by tube. It was evident that operation was necessary at once, the working hypothesis being that the symptoms were due to a perigastric abscess with very thick walls. Operation was begun with an incision cautiously made over the

swelling; as no pus was met with, the cut was enlarged, and the peritoneum opened to a small extent with some difficulty, as it was adherent to an underlying mass, which was explored with a hypodermic syringe without striking pus or a cavity. The sensation derived from the insertion of the needle was not suggestive of carcinoma, but rather of inflammatory tissue. It felt very much as if the needle had been pushed into a hard apple, and, to continue the simile, the hole left was such as would remain in an apple. The peritoneal cavity was now demonstrated above and below the adhesion, and it was noticed that the parietal membrane was altered, being thickened and injected. The investigating finger now met with a large mass evidently situated in the wall of the stomach. The tumor was as big as the palm of the hand, with processes issuing in several directions, but there was no attempt on the part of the growth to travel along the lesser curvature. The circumferential lymphatic glands were enlarged, reddish, and fleshy. With the eyes shut, the growth, which could now be moved about, felt very much like a carcinoma indeed, but it did not look altogether like one, especially about the part which had been adherent. It seemed too fleshy, not cicatricial enough, for chronic cancer; there was not the dull grey opaque white of carcinoma, but a semi-translucent, almost opalescent, look about it. For all this, the idea of carcinoma would come first to one's mind on seeing the tumor as well as feeling it. The condition of the posterior wall of the stomach was now investigated, and while opening the lesser cavity a clue to the nature of the case was found in the presence of white shining glistening bands of fibrous adhesions scattered in the great omentum quite irregularly and without obvious causation, there being no adherence of the omentum to the parietes or the stomach. One of these bands was circular and surrounded a hiatus in the omentum, having streamers issuing from it in several directions. There were also many linear strands of dense fibrous tissue mingled in the omentum which evidently had no use in limiting inflammation or extravasation from the stomach, and which appeared of greater age than the tumor itself.

Nothing unusual was found on the posterior surface of the stomach. These omental appearances decided me against any operation, so the abdomen was closed after the removal of a small piece of the tumor for Dr. Mollison, who assured me he had never seen endarteritic changes more easily guaranteed as syphilitic than those in the specimen. In five weeks the patient, under appropriate treatment, had gained health, strength, and good looks, and lost her tumor as far as external palpation could recognise. This gives us examples of two forms of syphilis in the stomach; a third is well exemplified in the following cases :—1. G.C., male, *æt.* 37, had been ill more or less for two years with some gastric trouble, diagnosed as gastric or duodenal ulcer. The prominent symptom when the case first came under my notice was persistent vomiting, which allowed of no food being retained. Food caused immediate pain in the epigastrium, which was speedily relieved by vomiting. Hæmatemesis had occurred twice in the last few days, once amounting, according to the patient, to the vomiting of a pint of blood. An examination of the patient showed him to be of a peculiar blanched complexion. His abdomen showed fulness in its upper half, and peristaltic waves could be seen passing from left to right across his dilated stomach. No tumor could be felt anywhere, and no superficial enlarged glands were present. There was a tender spot in the epigastrium to the right of the middle line, and firm pressure here caused great pain. Operation was decided on, and revealed a lump formed by a much thickened and stenosed pylorus and marked gastric dilatation. A gastrectomy was almost decided on, but an examination of the lymphatic glands showed them to be large, fleshy, reddened, and succulent, and I contented myself with removing some of these for examination, having suspicions

of syphilis. The glands showed no signs of malignant structure, only inflammatory changes. The arteries in many places had very thick walls, and in some instances had become thrombosed. Here, again, the tumor had a semi-translucency almost like that of the pudding known as blanc-mange. A course of mercury and iodide improved the patient very much ; he gained weight, lost his pain and no more blood, but his gastric dilatation did not go down, and three months after I again opened his abdomen, finding the tumor very decidedly smaller and the glands more normal. Instead of performing gastro-jejunostomy, I chose pyloroplasty. The former would, I think now, have been the better surgery. The pylorus proved to be very thick, and there was the scar of an ulcer inside. The operation was difficult, but a good-sized lumen eventuated. The patient at present, three years after the operation, is in very fair health, but has to take great care as to what he eats, and is always benefited by an antisyphilitic course of medicine. I never see him but I am thankful I did not perform an unnecessary gastrectomy, and wish I had done the more appropriate gastro-jejunostomy.

Here, again, no trial of the acidity of the stomach contents seemed justified. The amount of blood lost was remarkable from an ulcer undoubtedly syphlitic.

The second and very similar case is not so pleasant for me to relate. It was that of a man *æt.* 56, whom I saw in February of this year. He complained principally of wasting and vomiting. Symptoms began about six months ago, in which period he has lost three stones in weight. No severe pain at any time has attacked him, but he has a hot sensation after food, and there is some tenderness in the epigastrium, where a sense of resistance is present but no palpable tumor. His stomach is much dilated, as proved by CO_2 distension, and by the fact that he vomits more than he takes. and observes in the vomit food taken several days before. Chemical analysis of the vomit fails to show the presence of free HCl. Operation disclosed a pyloric growth apparently carcinomatous and suitable for removal. The anæsthetist was anxious that no time should be wasted in deciding as to procedure, and I performed a partial gastrectomy. The specimen proved to be a syphiloma with much fibrosis and cicatricial contraction. Five days after the patient died of pneumonia, much to my regret, my solace being that there was no leakage either at the closed end of the stomach or at the junction on its posterior surface.

It seems that a large syphilitic infiltration mass may appear at the pylorus ; this may correspond closely with the ulcer inside ; or the infiltration may overlap the ulcer much in all directions ; or there may be a syphilomatous tumor and no ulceration ; or an ulcer presumably syphilitic may occur with a minimum of infiltration. The difficulties surrounding the diagnosis of syphilis show up here very strongly. A neighboring gland or a piece of the actual tumor may show microscopical changes in the shape of thickened arteries, endarteritic alterations, endothelial irritation, and thrombosis, that may safely be ascribed to syphilis, but with the ulcer these signs cannot be laid at the door of syphilis alone. A non-specific ulcer situated in a place much irritated by stomach contents and muscular spasm will show some of these same signs in parts at all events of surface, so that no certain judgment can be given. However, arteries in other parts of the body which evidence no obvious macroscopical disease may be thickened and exhibit the phenomena of endarteritis, and this would go a long way to prove the pyloric ulcer syphilitic ; but we cannot very well in the living subject cut out a piece of liver in order that the microscope should decide whether the ulcer is syphilitic or not. Then, again, can a person who is syphilitic have an ordinary chronic ulcer which is not syphilitic ?

It is these things which make the discrepancies so great in the opinions of authorities on the subject. The analogy of ulceration of the leg helps us a little ; a syphilitic is exposed to the same causes producing ulceration of the leg as a non-syphilitic, but in the former the syphilitic taint is an additional factor tending to lessen the resistance and perpetuate the ulceration. We all know how syphilis shows itself in waves of activity in the organism generally; how its natural history is illustrated by periods of obvious disease sandwiched between periods of apparently good health ; how in tertiary syphilis, even, a man may have a good year and then a bad year, a good six months and then a bad six months ; now these waves of disease manifestations have their counterpart in gastric and duodenal ulcerations due to syphilis. A strong opinion of the syphilitic nature of an ulcer at or near the pylorus can be given if evident healings have taken place in several areas, if patches of active cell growth can be seen in old fibroses ; but to judge of this in the living subject during the course of an operation is very difficult, and however valuable these evidences of repair intermixed with active disease may be in the museum, they do not aid the surgeon much in the few minutes he has at his disposal.

High authority here helps us, as Professor Allen tells me that he is fairly certain that non-cancerous ulcers actually at the pylorus or immediately under cover of its valve on the duodenal side are, in his matured opinion, very generally syphilitic.

The therapeutic test is the best means of diagnosis, and this, too, is open to misconstruction on several counts. Syphilomata are more easily recognised as not being cancer the younger they are, *i.e.*, when they are fleshy, succulent, and inclined to show the opalescence I referred to. This opalescence is very different, I may remark, from the angry purplish appearance seen in a quick growing malignant adenoma. The more fibrotic they become, the more they are liable to be mistaken for carcinoma, and the similarity is very close in cases. The lymphatic glands differ in the two diseases, being softer, redder, fleshier in syphilis than in carcinoma, but it is only in well-defined varieties that a diagnosis could safely be made from glands without the microscope. However, what is almost as important is that a suspicion of syphilis may arise in the surgeon's mind from an inspection of the glands.

These pyloric syphilomata cause contraction without the presence of an ulcer, so that the dilatation of the stomach is no help to us. The surgeon has to make up his mind quickly. In cases where the signs make him suspicious even slightly of syphilis I feel sure he ought to restrain " the hands which have made him wise," and content himself with removing a gland close to the disease and perhaps a small piece of the tumor, closing the abdomen after a somewhat ignominious retreat.

If the pyloric lumen is evidently very small from cicatrisation, when felt by the invaginating little finger, a gastro-jejunostomy will be the proper procedure if the case is syphilitic, but the risk of gastrectomy when medicine or the much less severe operation of anastomosis will restore the patient to comfort is an awkward one to face.

Now is the time for the surgeon to remember how associated are the organs in this region ; how the stomach duct, which the duodenum may be called, the liver duct and the pancreatic duct all meet at one place ; how these organs and their appendages are supplied by a common trunk, the cœliac axis ; and how inflammatory processes in one organ can easily spread to the others, the subsequent inflammation giving rise to much greater effects than in the original organ. Reminiscent of these things, a rapid investigation by the surgeon of the surrounding parts may afford him much help in coming to a decision.

Markings of various sorts on the liver, patches of purposeless adhesions and stray fibrifications in the omentum, and possibly general fibrosis of the pancreas may put him on the right diagnostic path. However, in spite of every help—the history of the case, the look of the tumor, a general survey at operation, and microscopical assistance afterwards—it may be difficult to determine unmistakably. Such a case is at present under my care. I could not make up my mind at operation that the case was cancerous : Dr. Mollison found some endarteritis in the glands I removed, and no carcinomatous elements, but could not give a definite opinion. Fortunately, because of the great contraction of the pylorus, I had performed a gastro-jejunostomy, and a few days after started the patient on an antisyphilitic course of mercurial inunction and iodide by the rectum, with the happiest results. Even now, however, I cannot be quite sure of the syphilitic nature of the case, but I can be, and am, very thankful that I did not perform gastrectomy, which, judging from the shock present after the less severe operation, would have probably proved fatal. It is likely that a good percentage of supposed cancer cases, which live a long time after gastro-jejunostomy, are really syphilitic, the diversion of the gastric current and the mere operation itself having a very beneficial effect. In this last relation, what a number of cases have been reported where the abdomen has been opened to find a chaotic mass of adhesions too intricate to unravel, the surgeon has reluctantly closed the abdomen, and the patient has promptly lost his symptoms. All these, of course, cannot be claimed as due to syphilis, but a percentage, I feel pretty sure, will fall under the first class of intra-abdominal syphilis of which I spoke.

There is yet another connection of syphilis to be spoken of in gastric pathology. Carcinoma may be engrafted on a syphilitic base. This occurs at the entry and exit of the alimentary canal, and may not be uncommon in the stomach. The University Museum in Melbourne has more than one specimen, which, on the weighty dictum of Professor Allen, is of this nature. At the present time I have under my care a lady whose gastric tumor, on the assertion of Dr. Mollison's microscopy, participates in both diseases.

Sarcoma may enter the lists, and is to be recognised generally because of its discoid rather than spheroidal tumor, because of its tendency to avoid the lesser gastric curvature and to disregard the integrity of the duodenum ; but while these characteristics serve to differentiate it from carcinoma, it may be hard to exclude syphilis when sarcoma is present. It is probably rarer than either carcinoma or syphilis, and here again we see how malignant disease may choose a syphilitic site upon which to develop, for we have in the Melbourne Museum a specimen of gastric sarcoma, with the major portion of the stomach, which I removed some two and a half years ago (the man being still alive and in good health), and which is declared by Professor Allen and Dr. Mollison to be sarcoma growing from a syphilitic foundation.

Of chronic hypertrophic gastritis due to syphilis it is difficult to speak clinically, though pathologically it is a definite entity.

These considerations enhance the already great responsibility laid upon the surgeon in gastric surgery. The indurations he has to differentiate are those of simple chronic ulcer, and simple fibrosis, of carcinoma, of sarcoma, of syphilis, or of combinations of these last. Is it any wonder that he sometimes makes a mistake in his choice of operation, when we consider the complexity of the evidence, and the short time in which he must arrive at his decision ?

Much as I dislike reopening the abdomen after a short interval, I believe the best surgery will more often than in the past lead to such a course, fortified as it will be the second time by microscopical evidence.

In spite of gastric ulcerations, fibroses, contractions, infiltrations, and actual tumor masses of gastric growth, we have not exhausted, I believe, the roll of macroscopic syphilis in this region.

Concrete lumps of syphilitic origin may occasionally be found in the cellular tissue behind and at the sides of the first and second parts of the duodenum, and by their presence and almost cartilaginous hardness produce jaundice. These are distinct from syphilis of the pancreas, which is said to be rare, Petersen having found it in only one case out of 88 p.m.'s on the subjects of tertiary syphilis. It seems likely that some of the cases of chronic pancreatitis forming tumor are due to syphilis and some of the fibrotic cases also, but I think we cannot speak with much certainty yet.

Syphilis of the small intestine I have seen in two forms, both of which tend towards narrowing of the lumen, but neither of which, I believe, has the extreme constriction which I have seen in sigmoid ulceration due to syphilis. Even as gummatous deposit at the pylorus may produce obstruction without ulceration, so the small intestine may be strictured by similar deposit without a corresponding ulcer inside.

Syphilitic ulceration of the small intestine is rare, but has often been described. We have a good example in the Melbourne University Museum, in which the ulcers are oblong, about three-quarters of an inch in length, situated at the fundus of the intestine, and presumably starting in Peyer's patches, Their bases are indolent and indurated, thickened and opaque, with much congestive engorgement. On the peritoneal surface the ulcers are represented by little filamentous tags and adhesions. These were diagnosed as syphilitic before microscopical examination, mainly because they would fit no other classification, though, of course, the induration of their bases was suggestive.

Of the second variety, which is very different, I have seen no description. The ulcers are small, run transversely to the intestinal lumen, exist in considerable numbers, and reveal their position on the peritoneal surface by silvery scars, very like linæ albicantes indeed. Each scar of itself would do but little to narrow the gut, but existing in large numbers, as in this case, they diminish the calibre very considerably, giving rise to symptoms of obstruction. On my return to Melbourne I expect to have to remove some 18 inches of intestine for this cause from a lady upon whom I have operated before, and who I know has this peculiar condition which much evidence permits me to certainly class as syphilitic.

These numerous ulcers from time to time give rise to blood in the stools with much mucus, and attacks of an obstructive character. It is strange how the terminal result in skin ulceration differs from what is seen as the final scar in internal ulcerations due to syphilis. The papery scar left eventually by extensive and long-continued cutaneous ulceration has little tendency to contract, in obvious contradistinction to the strongly marked disposition to cause stricture, even to total closure, which we see with such disastrous endings in syphilitic ulcerations of the trachea, of the bronchus, of œsophagus and pylorus, sigmoid flexure and rectum. These ulcerations of the small intestine which I have just described seem to occupy a position between the external and internal ulcerations of syphilis, leaving small thin silvery scars on the peritoneum, which, however, in their multiplicity undoubtedly lessen the calibre of the intestine.

As far as I can make out, the region of the cæcum, the ileo-cæcal valve, and the appendix is but little susceptible to syphilis—an exception in strong contrast with the frequent manifestations of tubercular disease.

In spite of having had a number of appendices examined microscopically, I know of only three which can safely be classed as syphilitic. They were very chronic cases, and the specimens were densely fibroid and hard, with very little appendical lumen left.

It is an interesting speculation, upon which, however, I can throw no light, as to what part syphilis plays in obliterative appendicitis, and in those cases where the appendicitis has been of a chronic nature from the first. It is probably only one cause, and not a common one, of chronic inflammation of the appendix. Of course the appendix and cæcum and any part of the colon may be engaged in a large enveloping gumma, such as I have described earlier in this address, but more concrete syphilis affects but rarely the large bowel until we reach the sigmoid, when we find both ulceration and tumor-like formation not uncommon. Both may be associated with similar lesions in the rectum. I have seen stricture of the sigmoid due to girdle ulceration of the gut so tight as to be almost impermeable, and this with very little tendency to induration around.

Another case in which there was undoubted syphilitic ulceration healed by drug treatment developed marked obstructive symptoms, which were entirely relieved by the freeing and division of strong adhesions round the sigmoid, though the operation was started with the idea of resection.

Syphilis is undoubtedly a cause of peritoneal adhesions, even as traumatism produces them ; but it is very difficult to assign the proper importance to either as a causal factor. Wherever there is, or has been, an ulcer or gumma, the nearest peritoneal surface may be the seat of adhesions ; but also, I believe, the syphilitic poison can produce adhesions without ulceration or obvious gummatous deposit.

Remarkable are the developments of the disease we are discussing in the retro-peritoneum, and some of these are massive in their formation. There is no doubt that mesenteric and mesocolic glands enlarge in the secondary stage, in that mucous membrane disturbance which corresponds with the cutaneous efflorescence, but the size they attain is inconsiderable, and their diagnostic value and clinical importance are slight. In the tertiary stage, however, the retro-peritoneal glands generally may become transformed into very large tumor masses easily mistaken for sarcoma. Their stability is but slight, and in some instances mere inspection by operation is sufficient to induce their absorption, frequently to the astonishment of the surgeon who has given an unfavorable opinion. It is well worth while, in every case of supposed sarcoma of the retro-peritoneal glands, to remember the possibility of syphilis being the cause—a cause the effects of which are easily removed by simple operation and drugs. Not so tumefied, and with their cells not so loosely aggregated, however, are all these glands altered by syphilis ; some are hard, even shotty, and but moderately enlarged, similar in physical aspect to the glandulæ concatenatæ when enlarged in the neck ; and gradations occur, I believe, between these extremes. Both these enlargements may be associated with syphilitic disease of the rectum, which is itself often associated with marked pyogeny, and this may be largely responsible by toxicity, for the great size at times attained by the syphilitic glands, as well as for their succulent instability,

To my mind the surgical moral to be drawn from a consideration of retro-peritoneum tumors is that in no case should the diagnosis of sarcoma be considered final until operation has proved the absence of syphilis and hydatid, the former being a simulator of the softer sarcomata and the latter of the harder varieties.

Again, syphilis works marvellous mutations in the cellular tissues itself. It appears to be responsible for a strange wasting fibrication of the usually fat-

aden sup-peritoneal tissne, and at the opposite extreme there may arise from the syphilitic poison a widespread homogeneous gelatinoid mass, in which the kidneys, great vessels and glands may be embedded as in glue. The same material filling the cavity of the sacrum and surrounding the nerves and vessels of the pelvis may easily convince the surgeon of the presence of inoperable cancer, as it may also in the mesometrium. In such cases, I think, the extraordinary results are largely due to the spread of toxic matter from a pyogenic infection of syphilitic disease of the rectum and sigmoid. Surgeons have long since realised the far-reaching effects of a mixed infection in tubercular disease, but I think have hardly yet appreciated its importance in modifying syphilitic manifestations.

Mine will not have been a failure in this presidential address if I have directed your attention to the fact that syphilis is not very rarely the cause of pathological conditions within the abdomen, and that these pathological conditions are easily confused with other commoner diseases, especially carcinoma.

When one has once grasped the idea of the role played by syphilis in abdominal pathology, so deceptive are the facts, so elusive are the differences between it and other chronic inflammatory conditions, that one is apt to see syphilis everywhere, to discover a bandit behind every bush; but what I have set down in this paper has been of my own knowledge, or of others whose judgment I trust, and I have discarded many cases which, syphilitic in my belief, have not been fortified in diagnosis by the microscope; thus I hope you will think that I have "taught myself that honorable stop, not to outsport discretion." Yet there float into one's mind the words of the dying Ricord: "They reproach me with having seen syphilis everywhere; I, on the contrary, reproach myself with not having recognised it often enough."

Though the stndy of syphilis—"the specific disease which stands almost alone in mystery and malignancy"—is one of the most fascinating mental exercises that we have in our profession, we cannot but fervently hope for the good of the race that the *Spirochœte pallida* of Schaudinn will prove to be the germ of the disease, and Metchnikoff and Roux are very hopeful of the parentage.

Until we have eliminated syphilis—and it has been in possession of the human race from times immemorial—so long will man be unable to come into his kingdom, his complete mastery over Nature, the *Regnum Hominis* which appeared to the mental eye of Bacon as a prophecy for the fulfilment of which modern science bids us to cheerfully hope.

THE OPERATIVE TREATMENT OF MOVABLE KIDNEY.

By William S. Collins, M.B., Lond., M.R.C.S., Wellington, New Zealand.

In introducing the subject of operation on movable kidneys to you, I wish it to be understood that I do not consider an operation is called for in a large number of cases. It is surprising in how many people the kidneys are found movable, and yet in whom this moblity seems to cause very few symptoms. Movable kidneys are most common in women, and are coexistent very often with some disease of the genital organs. In determining whether in a given case an operation is likely to be attended by benefit, one

has first of all to find out if the kidney is the real cause of the patient's sufferings. You have to find out whether the movable kidney is healthy, or the seat of carcinoma, tubercle, stone, or abscess. You have to eliminate the symptoms which may be caused by dyspepsia and constipation, appendical disease, gallstones; in women, by ovarian and uterine disease. I have frequently found movable kidneys in women who suffer from dysmenorrhœa, also in women who have had severe labors, and in those who have become thin and emaciated from having had children too frequently. Those cases where the kidney has been dislocated by injury are most likely to be benefited by operation. In highly neurotic women who have movable kidneys I should hesitate in advising operation, nor would I operate on women who are approaching the climacteric period. In most poorly nourished women I believe that an operation can be avoided by rest and improvement of the general health.

Seven years and a half ago I was consulted by a lady for pain mostly in the right side, nausea, vomiting, and general abdominal discomfort. She was thin and emaciated. She had had three children in seven years. Her symptoms were always relieved when she was lying down. Both kidneys were found movable, the right more than the left; the right kidney was, moreover, tender on manipulation. I explained to her that I believed the mobility of the kidneys had something to do with her symptoms, and stated also that if she would rest and be fed up she might possibly avoid the necessity for an operation. On making her lie flat in bed without a pillow, and by raising the end of the bed 14in., I found that it was much more difficult to feel the kidneys. I suggested that she should lie in bed for six weeks or two months with the end of the bed raised 14in., and be in the meantime fed up. This she consented to do. By appropriate feeding she improved in general health, and at the end of two months she was allowed to move about. Since then I have seen her occasionally, and there has been no return of her symptoms.

For many years I had been dissatisfied with the results of the operations for movable kidneys, and it was this case which first suggested to me the operation I now employ for fixing the kidney. It always appeared to me that by the operations usually performed the kidney was fixed in an abnormal position; that it was fixed opposite the incision in the abdominal wall (in the majority of the cases the kidney could be easily felt after the operation in the position in which it was fixed); that in this new position it was more liable to be affected by the movement of the muscles to which it was attached or adherent; that it was more liable to injury and exposure to cold; and that even though fixed, and though the symptoms which had been caused by its mobility had ceased to exist, yet in time it became enlarged, tender, and painful, and caused as great discomfort as it had previously done when it was movable. I have on three occasions operated on kidneys which had been fixed by the ordinary operation, and have separated the adhesions and refixed the kidney again in its natural position, and up to the present with good results. According to the statistics given in "The International Textbook of Surgery," in operations on movable kidney there are 22 per cent. of relapses, and in about 10 per cent. there is only some improvement. In all operations for movable kidneys I believe that it is necessary for success to replace the kidney as far as possible in its normal position, and it is with the view of accomplishing that object that I use the operation I am about to describe to you.

In the operation which I advocate the kidney is replaced as far as possible in its natural position, and when the operation is completed the lower pole of the kidney is above the level of the umbilicus. The patient, having been

anæsthetised, is placed in the lateral position, the surgeon standing behind the patient. The length and position of the twelfth rib is carefully defined, and a point ½in. below the rib and 2½in. from spine is marked out.

A kidney-cushion or pillow is placed under the opposite flank of the patient. From the point marked out, an oblique incision is made downwards and forwards for about 4in. The skin and fascia are divided, the anterior fibres of the latissimus dorsi and the external and internal oblique muscles. If possible, division of the last dorsal nerve is to be avoided, owing to the pain which is caused after the operation, but usually it is impossible to prevent division of it. The lumbar fascia is cut through and the perirenal fat exposed, the fatty capsule is opened, and the kidney felt for and examined *in situ.* Should there be any suspicion of disease, the kidney should be delivered out of the wound (in order to do this more easily the wound may have to be enlarged), and examined carefully. The fatty capsule is now removed, the kidney capsule is nicked at the centre of its convex border, and divided on a director from end to end, and reflected to the extent of about ¼in. on each side. The upper pole of the kidney is bared more freely from its capsule than the lower pole. A temporary ligature of silkworm-gut is passed through the substance of the kidney, and the kidney replaced in the abdomen. The object of this temporary ligature is to steady the kidney during the subsequent steps of the operation.

The fingers of the operator's right hand are passed up under the ribs into the position in which the kidney is to be fixed, in order to feel what space there is. The patient is now placed in an extreme Trendelenburg position, and immediately the kidney is seen to move upwards under the ribs into its natural position, unless prevented by traction on the temporary fixation ligature. It is now gently and without force pushed up as far as it will go by the fingers of the right hand of the operator being placed under the lower end of the kidney. A silkworm-gut ligature is passed through the lower end of the kidney, including reflected and attached capsule, and parenchyma of kidney, and both ends of suture are drawn out of the wound and held by forceps. A second silkworm-gut suture is passed through the extreme end of the kidney, and also drawn out of the wound and held in a similar manner. The temporary steadying ligature is now removed. Each end of the first suspending ligature is threaded on to a curved needle, the fingers of the operator's left hand are passed into the wound and underneath the tissues forming the upper part of the wound, the twelfth rib is felt, by the fingers the needle is guided and passed through the abdominal wall, emerging on the surface just below the last rib. Both ends of the ligature are in this way passed through and the ligatures are about 1½in. apart on the skin surface. The ends of the second suspending ligature are passed in the same way, emerging on the skin just below the last ones and in the same line. The second ligature is tied first over a piece of gauze, and then the first one. Both these ligatures must be tied without exerting any forcible traction on the kidney; the amount of traction necessary is only that which is necessary to approximate the lower end of the kidney to the abdominal wall. If the dorsal nerve has been divided and the cut ends can be found, they may be sutured together. The wound is closed with deep silkworm-gut sutures in the ordinary manner. The patient is removed to a bed, *the foot of which is raised* 14*in.*, and I consider this to be a most essential part of the treatment.

After the operation she is made to lie on her back, but with rather an inclination over towards the side upon which the operation has been performed. *The patient kept in this position with the end of the bed raised* 14*in. for three weeks.*

The wound sutures are removed on the seventh day, and the suspending ligatures at the end of three weeks. If there has been much manipulation during the operation, it may be advisable to introduce a drainage-tube at the back part of the wound and leave it in for twenty-four hours. As a rule, however, this is not necessary.

The main features in the operation are the stripping of the capsule, the fixation of the kidney in its normal position while the patient is in the Trendelenburg position, the passing of the ligatures for the anchorage through the lower end of the kidney, and the keeping of the patient in such a position after the operation that there will be the least possible disturbance to the organ caused by coughing, vomiting, &c. The kidney by this method of operation is fixed in its normal position with very little injury to the kidney itself or disturbance of the surrounding parts, and it appears to me to be more satisfactory than fixing the organ exactly opposite the external wound or planting it on to a muscle.

On May 5th, 1905, at a meeting of the Wellington Division of the New Zealand Branch of the British Medical Association, I again read a description of the operation, and asked if any of the members had come across any of the patients on whom I had operated where the kidney had again become movable, and I was gratified to find that none of the members had seen any cases of failure.

Dr. Faulke stated that he had done some cases latterly, and the operation was a great success. He found that there was too much tension from two sutures, and so used three. In his later experience he found that there was no occasion to raise the end of the bed after the operation, and since discarding this position his cases had done equally well.

Since the above discussion I have heard of one case on which I operated over two years ago, in which the kidney had again quite recently become displaced, and on inquiry I found that it was in one of these cases which had been operated upon before by one of the usual methods, and where the kidney had become much enlarged and painful after the first operation.

STEREOGRAPHY.

By C. H. Souter, M.B.

There is nothing new in the subject of stereography. So many of us are amateur photographers nowadays that it is somewhat surprising that this well-known method is so seldom made use of. There is, however, an easily recognisable advantage in the binocular over the monocular photograph. I hope it may not be impertinent to give a very brief description of the manner of taking a stereographic picture, and the special points that are most important. It is necessary to take two photographs of the same object in such a manner that one photograph shall represent the object as seen by the right eye, and the other photograph shall represent it as seen by the left eye. For this purpose two cameras are used—fitted each with a lens of equal focal length to that of its fellow. For convenience the two cameras are made in one, and divided by a collapsable partition between the lenses. The plate-holder or dark slide may carry a half-plate or film of corresponding size. For correct work, and more especially in medical and surgical subjects, it is better, in fact essential, to have an adjustable front by which the lenses can be brought

nearer or further away from one another at will, in order that, as far as possible, the same angle may be subtended for near as for distant objects. If your lenses are about 3½ inches apart, distant views will appear in bold relief; but if more than 2½ inches from centre to centre separates the lenses, exaggerated perspective will be seen in photographs of very near objects (say about one to two yards away). Thus the nearer the object the closer should be the lens centres to one another.

All the good makers supply stereographic outfits, and any half-plate camera can be converted into a stereographic camera by fitting on a Thornton-Pickard adjustable stereo time and instantaneous shutter, and procuring a pair of well-matched but not necessarily expensive lenses for it. All that remains to do is divide the camera inside with a piece of accordeon pleated black stuff sprung in between the front and back. The original half-plate holder or dark slide will do. I have brought this subject up to night, and draw your attention to certain characteristics of these pictures, in the hope that some day producers of medical and surgical works will use this means of illustrating. Authors and publishers are coming to depend more and more on photography as a means of illustration in surgery and medicine. It is right that this should be so, for although the old jibe that a man may "lie like a tombstone," might well be read nowadays "lie like a photograph," it remains true that, untampered with and properly interpreted, a photograph is an actually scientific record of what was there to be seen. Unfortunately it is easy to tamper with photographs. The usual portrait photograph is so much faked or "touched up" that the original often only knows it for his counterfeit presentment by taking the artist's word for it. I do not think I can say a greater thing in favor of the systematic use of the stereograph than this—you cannot retouch or otherwise tamper with a stereographic picture without the fact being plainly evident to the most casual observer. As I consider this a really most important fact regarding stereoscopy not only from an illustrator's point of view, but from that of the medical jurist, I will detain you a moment to see how this may be clearly shown. It may be foreseen from the nature of things. If you imagine two pictures or views—one seen by the right and one by the left eye—the two visual axes converging at the centre of a certain object say a hundred yards away, it will be plain that no two points in the intervening space that are passed through by the visual axial line of one eye will also be passed through by that of the other. Thus in any plane at right angles to that of the mean line of sight the right and left lines of sight will never pass through corresponding points in the pictures. They will do so at the plane of the object on which they are focussed. On examining a pair of stereoscopic prints carefully, it will be seen that the central focal points of the pictures correspond, but that the relations of all other points of the pictures differ in the two pictures.

Another way of putting this is to say that if one look at a clear glass ball held a yard away from the face, the centre of the ball will be the centre of the picture for either eye, but the right eye will see further round the right side and the left eye further round the left side. This would also be true of a stereograph of such a ball. Now if one take a single photograph and make some alteration in it by retouching, it may be so skilfully done that close scrutiny will fail to detect the alteration. If this be tried with a stereographic pair of pictures it will fail most completely from two causes. Firstly, it is impossible to retouch one picture so absolutely similarly to the other as to make even a mechanical correspondence between the two. Secondly, even could this be done, it is obvious that as the point of view of each object in either picture differs some degrees (especially for close objects), the retouching would have to correspond with this difference in the respective

pictures, a physical impossibility. The practical result of all attempts to "retouch," or "touch out," or "spot," or make any adventitious marks on a stereoscopic pair of photographs is that these marks, seen through the stereoscope, appear to stand forward in a plane of their own, not subtending any particular angle of the eye, and having no part in the composition of the picture as focussed by both eyes. This can easily be demonstrated. Take one of the slides before you and endeavor with a pen or pencil to make a mark on both of the pictures, so that it shall produce a concrete effect with binocular vision through the stereograph: you will find it impossible.

This fact I consider of much medicolegal importance.

The other and much more obvious virtue of the stereoscopic process for illustrating surgical and medical work is two-fold. The fact that there is perspective and relative distance, and even *actual* distance given to the component parts of the picture causes the stereograph to furnish a vastly more accurate impression to the eye than the most brilliant and detailed single photograph can, and it further follows that inferior, over or under exposed, and badly printed photographs, which would be useless for illustrative purposes used singly, are, if stereographic, still capable of showing a great deal of useful detail.

There is but one thing that stands against the use of stereographic photography for printed works. It is the misfortune that as all photographs are reproduced by copying through a mesh—finer or coarser according to the class of work—all "process work," as it is called, shows the copies in stipple effects, with no true gradations from light to shade—only a series of large or small points of light. For this reason—and, as I have already shown, because no two points can be said to accurately correspond except the central focal points of the pair of pictures—the "process" copy of a stereographic picture looks like a *real* scene or object viewed through a dense wire screen (which indeed it really is), and this takes off much of the brilliancy, important detail, and realistic effect.

Nevertheless, a surgical or other work might well be supplied with a wallet of original photographs, numbered to correspond with certain diagrams or pages.

Fancy a few dozen good stereographs sold with books like Ward's Osteology, Grey's Anatomy, Ellis, Treves, Hart and Barbour, Guy and Ferrier, or Taylor.

I think half the bald descriptions in students' text-books on surgery, midwifery, anatomy, botany, zoology, medical law, dermatology, and a good many other subjects could well be done without, or at least cut down by half, and replaced by stereoscopic pictures which, in suitable subjects, give a concrete, realistic, and objective impression to be gained only in one other way, viz., by seeing the real thing itself. As to the possible objection ot expense, publishers should be able to supply good stereoscopic photographs from negatives supplied by authors at sixpence a dozen.

THE OPERATIVE TREATMENT OF THE ENLARGED PROSTATE.

By H. L. Maitland, M.B., M.Ch.

In a paper I read before the New South Wales branch of the British Medical Association, which was published in the July (1903) number of the *Gazette*, I expressed my ideas as to the method of performance and value of perineal prostatectomy. That paper was based on a series of five cases. A few months later I published another series of five cases. The paper which

I bring before you to-day is based on the experience of forty-four cases operated upon for the relief of symptoms due to enlarged prostate. The series consists of twenty-five cases enucleated by the perineal route; six cases enucleated by the suprapubic route; eleven cases of perineal prostatotomy; two cases of supra prostatotomy.

The series of cases extends over a period of five years. The first case of perineal prostatectomy I did in 1900.

The so-called senile enlargement of the prostate is due to a fibro-adenomatous growth, which is practically an overgrowth of the normal glandular tissue. Clinically the enlargement may be divided into a soft and a hard variety. In the soft variety the adenomatous tissue predominates, and in the hard variety the fibrous tissue.

The soft or adenomatous variety usually forms outgrowths of a larger or smaller size, which project into the bladder or towards the rectum .

The hard or fibrous variety is very small, hard, and tough, forming a lip rising above the level of the urethral floor, then dropping in a perpendicular behind, to form the anterior wall of a bladder pouch.

There are many gradations between the two varieties, and they merge into one another, but it is important that there should be a distinct recognition of these two varieties, because I maintain that their treatment is absolutely different.

I will first of all discuss the operative treatment of the adenomatous or soft variety; and this is briefly *enucleation.*

There are two routes by which the prostate may be enucleated: 1st, the suprapubic; 2nd, the perineal. Which of the two routes is the better? For many reasons I consider the perineal is the better.

(1) From an anatomical standpoint every fact is in favor of the perineal and against the suprapubic. When one considers the situation of the prostate—that it lies outside and below the bladder in the perineum, bounded below by the triangular ligament—surely it is evident that the prostate is more easily reached from the perineum than from in front, through the abdominal wall and bladder. And it is a reasonable surgical axiom that in operating on any organ the most direct route should be taken, provided important structures can be avoided. And the approach by the perineal route is through comparatively unimportant structures.

(2) *Drainage is more satisfactory.* We are accustomed to get perfect drainage of the bladder by a suprapubic opening. This I freely admit, because the superior wall of the bladder is kept opposed to the inferior by intra-abdominal pressure, and the urine does not collect; but when the prostate has been enucleated, you have quite another condition existing, viz., a cavity which is outside and below the bladder. It is not possible to drain this cavity suprapubically. By the perineal route you get drainage which is perfect. It is in the direction of gravity, and the drainage tract is a short one.

This question of drainage is an all-important one, and in the majority of cases in which I have enucleated the prostate through Freyer's suprapubic opening I have employed perineal drainage.

(3) There is less danger from sepsis, because there is better drainage.

(4) There is less shock, because the perineal is a less severe operation.

(5) There is less damage to the bladder: when the suprapubic route is employed the bladder is opened above and below. In the perineal route it is possible not to open the bladder at all, but I always incise the bladder wall as a matter of choice.

(6) The perineal operation allows the prostate to come more into view during the enucleation, that is if Kocker's transverse incision is used, and the prostate enucleated from outside the urethra,

The suprapubic operation I admit allows of a visual examination of the visceral configuration of the prostate ; but the perineal allows digital exploration, which is just as good in these cases.

(7) The patient can early be placed in the semi-recumbent position, perfect drainage taking place when in this position. This is of some importance, when it is remembered we are dealing with the aged.

(8) The shortening of convalescence. The aged patient can sooner leave his bed if he has no abdominal wound.

(9) The prostatic urethra is better preserved. Both lobes cannot be completely removed without some laceration of the urethra. The ejaculatory ducts, as well as the prostatic ducts, open into the floor of the urethra on each side of the veru-montanum, and, as they are torn through, the floor of the urethra must suffer. I am perfectly well aware that many cases have now been recorded where the whole prostatic urethra has been removed, and the urinary function re-established.

One of Reginald Harrison's cases early became strictured, and I have seen the same thing occur.

(10) There is less hæmorrhage : because I think it is better controlled from the perineum than suprapubically.

(11) There is less danger of secondary hæmorrhage. In two cases removed suprapubically, in which I did not drain by the perineum, most alarming second hæmorrhage occurred, the explanation being that there is more danger of sepsis from inefficient drainage in the suprapubic operation.

The main difficulty in the perineal operation is that of bringing the prostate down sufficiently low in the perineum so as to get round it with the finger.

Various suggestions have been made to overcome this difficulty, viz. :—

(I.) Pressure from above, through a suprapubic wound, or through Retzius space.
(II.) Traction downwards by means of a dilatable bag introduced through the prostatic urethra into the bladder.
(III.) By traction downwards made by fingers of left hand in rectum.
(IV.) By introducing the left index finger through the prostatic urethra, and hooking the prostate down while enucleation is done extra urethrally by the right index.
(V.) By such an instrument as Gouley's or Young's depressor.
(VI.) By traction downwards with a volsella forceps.

DESCRIPTION OF THE OPERATION OF PERINEAL PROSTATECTOMY.

The steps of the operation which should bear Gouley's name are as follows :—The patient, placed in the lithotomy position, has a catheter introduced, and the bladder well irrigated. Sufficient of the irrigation fluid normal saline is left in without producing undue distension, usually from four to six ounces. A median lithotomy staff is then introduced. I have had several specially made, with a broad groove and a prostatic curve.

The finger is then introduced into the rectum, and placed on the apex of the prostate. A median incision, beginning about a quarter of an inch in front of anus, is then made with a plunge of the knife, with the blade forwards—the point of the knife is made to enter the urethra at the apex of the prostate, and is then withdrawn, enlarging the incision upwards while withdrawing the knife. A probe-pointed bistoury is then introduced into the groove on the staff, and the floor of the prostatic urethra divided and prostate urethra as far as the capsule ; a gorget is then introduced, the right

index finger follows. The bladder is explored, and the prostate is palpated between the two index fingers, one being in the wound and the other in the rectum.

It can then easily be ascertained whether the prostate is suitable for enucleation or cauterisation, and whether there is a line of cleavage or not. If enucleation is decided upon, the left index finger is introduced and swept round laterally, and on either side beneath the capsule. This frees each half of the prostate, which is then drawn down by a volsellum; the attachments of the gland to the roof of the urethra are then divided by a scissors, so as to preserve as much of the urethra as possible: it is far better surgery to preserve the urethra, and leave a small portion of the gland behind, than to remove the whole gland as well as the prostatic urethra. The bladder is irrigated, a perineal drain introduced, and the wound plugged—the after treatment is that of an ordinary external urethrotomy.

The operation which I have here described can be very rapidly done, and the saving of time in an operation on the aged is an important factor. In a favorable case I have done the operation in four and a half minutes.

If more room is required, the median incision can be converted into the inverted Y incision by two lateral incisions.

CONDITIONS INFLUENCING THE CHOICE OF OPERATION IN ENUCLEATION OF THE PROSTATE.

When I published my first series of perineal prostatectomies in 1903, I expressed the opinion that in some cases it was necessary to have a suprapubic opening, but increased experience in the perineal operation inclines me strongly towards the view that a suprapubic opening is seldom required. If there be very offensive alkaline urine, then preliminary suprapubic drainage with bladder irrigation, followed by a perineal prostatectomy when the local conditions have improved, is the operation of choice.

TREATMENT OF THE HARD FIBROUS PROSTATE.

In a certain percentage of cases of senile enlargement of the prostate, the fibrous tissue greatly predominates, so that it is firm and unyielding, and very hard, intimately connected with its capsule, and quite impossible to shell out; in one case I remember the gland was as firm and as hard as muscle tendon. The gland in this condition is smaller. These are the cases in which Bottini's operation has given good results.

Many prominent surgeons differ widely as to the treatment of this form of prostate. The main indications in the treatment are:—

1st. To remove the mechanical obstruction to urination; and

2nd. To obtain diminution in the size of the enlargement as a result of cicatrisation, and the modification of its circulation.

There can be no question but that any form of prostatotomy which removes the mechanical cause of the obstruction will give relief.

Prostatotomy may be done in two ways: 1st, by cauterisation; 2nd, by removal of wedge-shaped piece of the prostate, followed by prolonged perineal drainage.

Cauterisation relieves the obstruction, and causes some shrinkage of the gland. I agree that the idea of cauterisation, which originated with Bottini, is in principle correct, but I condemn and reject his method of applying it.

Many remarkable statistics have been published of Bottini's operation, but nevertheless the question which presents itself to every surgeon's mind

is—is Bottini's operation the most reliable and safest method of cauterisation? I am convinced that Bottini's operation is an unsurgical procedure, and I have, when the opportunity has been presented, expressed that conviction.

Some of the disadvantages of cauterisation as performed by Bottini :—

(1) The difficulty of diagnosing the pathological condition of the prostate.

The anatomic relations of the prostate prevent a perfect examination, either by sight or by palpation. One is unable to determine the form of enlargement accurately.

The two methods of examination we have at our disposal, viz. :—Palpation per rectum, and by means of the cystoscope, are both faulty. Palpation gives some idea of the consistence of the gland and of the condition of the enlargement towards the rectum, but every surgeon must admit that it gives us no idea of the intravesical enlargement.

The cystoscope at first sight appears to do away with this difficulty, but it is a matter of common experience that the lengthening and distortion of the prostatic urethra very frequently interfere with its introduction.

And, further, if it be introduced, the close proximity of the intravesical outgrowth to the end of the instrument materially interferes with its effective use, since neither the prostatic outgrowth nor trigonal pouches can be seen.

(2) The operation is performed in the dark.

It is quite impossible to tell where the beak of the instrument is; one may feel it per rectum, but that only gives an approximate knowledge, both of its position and the tissues within its grasp.

(3) Inability to gauge the length of incision.

As I have already stated, there are no means of ascertaining accurately the intravesical conformity of the prostate and the length of the prostatic urethra. How then is it possible to tell the required length of incision? You can tell from the instrument how far you have to cut, but you cannot tell how far you ought to cut.

These are the main objections to the operation, but there are others—such as the want of drainage and the dangers of post-operative hæmorrhage. These are real, and not theoretical, objections.

The operative measure I adopt in the treatment of the fibrous prostate has the advantage of being a simple procedure.

It is cauterisation through a perineal wound.

The operation was first described by Chetwood and Wishart, and the method has been followed by many surgeons who have been mindful of the deficiencies in the technique of Bottini's operation.

This method appealed strongly to me, because I felt that when the cautery was used upon the prostate the operation area should be under the direct observation of the operator; that opportunity should be given for digital exploration of the prostatic urethra, bladder, and visical orifice; and that bladder drainage should be secured. All these conditions are fulfilled in the operation I describe.

METHOD OF OPERATING.

A perineal section is done in the manner I have described.

A Bodenhumer's rectum speculum with long valves is then introduced, and a clear vision obtained by a head light on the operator.

A triangular piece is then burnt out of the prostate with a Pacquelin or, preferably, an electro-cautery. I have tried removal of a triangular piece with a knife, but the cautery seems to give the better results.

This method I also apply to the very old and enfeebled, instead of the more radical and severe operation of enucleation.

DANGERS OF ENUCLEATION.

Prostatectomy is a capital operation, therefore it is accompanied by serious complications, and it is only by a proper selection of cases that good results will be obtainable.

Age is not a correct guide to follow in selecting a case, because it is not a correct index of the patient's physical condition. If his heart arteries and kidneys are in good condition, then he is a fit subject for operation.

The dangers to be apprehended from the operation are, I think, in the order of importance :—

Uræmia.
Sepsis.
Hæmorrhage.

More remote complications are :—

Injury to the rectum.
Incontinence of urine.
Perineal fistula.
Orchitis and epididymitis.
Stricture of the urethra.

There is one other sequela, and that is impotence.

This is of no importance in an old man, but is of grave consideration in a younger and more virile one.

We will consider these dangers and complications in their order.

Uræmia.—This, I am convinced, is the most frequent cause of death. And I believe that it should be almost a *sine qua non* that the kidneys are not seriously involved before prostatectomy is performed. Unfortunately the condition of the bladder often makes this a most difficult matter to ascertain. Bladder irrigation and the administration of urotropin for some time previous to the operation are aids in helping the aged patient over on to the safe side.

Careful examination of the urine from time to time during the preparation are, needless to say, the guide to the kidney condition.

Sepsis is a grave danger, especially in the presence of an infected bladder. The best safeguards are careful preparation by bladder irrigation and efficient drainage, which is only provided for by the perineal operation.

Hæmorrhage, I am of opinion, is a more common complication of suprapubic than of perineal prostatectomy. It has occurred in two of my cases of suprapubic prostatectomy, but I have never had it occur when the perineal route had been chosen.

It is best avoided by using the median incision as much as possible, and by tying all vessels, instead of controlling the hæmorrhage by packing

Injury to the Rectum.—The rectum may be injured directly, or the wound may subsequently slough and open into the rectum.

This complication can be avoided by keeping close to the urethra, and opening it at the apex of the gland, and then in the enucleation keeping well within the capsule of the gland.

The accident is more likely to occur if Rocker's curved incision is used, and also it is more likely to happen in an extra-urethral than it is in the intra-urethral enucleation.

Incontinence of Urine.—This is not an uncommon complication after the operation, but it only lasts for a short time, and is confined then to some little dribbling. It is due to both muscle and nerve injury, and most frequently happens when extensive injury is done to urethral muscles by opening it too far forward.

Perineal Fistula.—This may persist for some little time, but it is a complication that is easily avoidable. Too protractive drainage may cause it.

I seldom leave the perineal tube in longer than a week, but I have drained the bladder for a month without a fistula remaining. An unnecessary large incision is also a factor in its causation.

Orchitis and Epididymitis are complications which sometimes occur, and arise often through the injudicious use of instruments.

I have placed the dangers and complications before you because they are real and exist, and have all happened in the series of cases that I present to you. I notice that as a rule they are carefully omitted from the literature on prostatic enucleation.

THE RESULTS OF THE REMOVAL OF THE OBSTRUCTION IN ENLARGED PROSTATE.

To understand these there must be a clear conception of the pathological changes which take place in the bladder.

In prostatic enlargement there are two conditions present: 1st, obstruction to the urinary outflow; 2nd, a circulatory obstruction.

The effects of urinary obstruction fall entirely on the muscular coat of the bladder, and the result is hypertrophy, with lessening capacity and distensibility. This is the condition in a stricture of the urethra, and removal of the cause is followed by an involution and partial disappearance of the hypertrophy.

But in enlarged prostate another condition is to be taken into account; it is one of the greatest importance, viz., a circulatory obstruction. This is produced in the following way:—The vesico-prostatic veins are valveless, and as the prostate enlarges they are pressed against the fibrous envelope, thus obstructing the venous circulation of the bladder, producing a venous hyperaemia of the bladder walls, which in turn produce trophic changes in the bladder walls. This leads to trabeculation, sacculation, atony, and distension. These changes are further modified by cystitis.

There is a third condition presented, and that is degenerative change due to senility, taking the form of an increase in connective tissue in the bladder walls.

Can these pathological changes be removed entirely by removal of the urinary and circulatory obstruction? My experience teaches me that as a rule they cannot be removed, and that such a thing as an absolute cure of all the symptoms which follow in the train of an enlarged prostate is rarely possible, despite what has been said and written to the contrary. Great relief can be given. If the cases be done before the advent of cystitis, and in moderately young persons, then ability to completely and naturally empty the bladder may be obtained; but if acute cystitis has supervened, and the patient is aged, then there is a lessening of the symptoms—the urine is not so alkaline, and there is less frequency, and the patient enjoys greater comfort.

WHEN IS OPERATIVE TREATMENT INDICATED.

Since the operative measures are accompanied by less danger than is entailed by habitual catheterisation, they are indicated when the enlarged prostate produces symptoms which render habitual catheterisation necessary. The indications for operation become more imperative when catheterisation becomes painful and cystitis supervenes.

This not a hard and fast rule, because there are certain conditions which modify it—such as the physical condition of the patient. One would be inclined to allow catheterisation in a cultured educated man who is capable of being taught the necessity of surgical cleanliness; on the other hand, you would advise against catheterisation in one who may be in a similar condition, but who does not possess the same mental advantages.

RESULTS OF THE CASES OPERATED UPON.

1. *Enucleation by the Perineal Route.*—Up to the present date I have operated upon twenty-five patients by this route. I did the first case nearly five years ago.

Mortality.—In this series two cases died (numbers IV. and X.), both previously recorded. No. IV. died of sepsis; he developed acute mental symptoms two days after operation, and removed and fouled his dressings. No. X. died of uræmia on the seventh day.

Of the twenty-three cases all were improved after operation.

I have been able to ascertain the subsequent history of seventeen of these cases, and find that four of them died of subsequent urinary trouble.

Nos. V., VIII., XIV., and XVII., aged respectively 68, 70, 71, and 69 at the time of operation. All were improved after the operation.

Of the thirteen cases remaining, ten of them had had subsequent attacks of cystitis, in most cases mild.

In No. VI. the perineal fistula still remained, but he was more comfortable with that than with his prostate.

2. *Enucleation by the Suprapubic Route.*—Six cases. Five cases recovered from the operation.

No. III. died of uræmia.

No. IV. died twelve months after operation, of urinary trouble. He had frequent attacks of cystitis, which began six months after the operation. I was disappointed in this case, as he did very well for the first six months.

Nos. I. and V. both had secondary hæmorrhage after the operation, but recovered from it.

No. V. I have lately had under my care. He developed an acute attack of cystitis, and the suprapubic wound broke down; this has since healed.

Prostatotomy.—Eight cases cauterised: three cases wedge-shaped piece removed.

All eleven cases were much improved after operation: seven subsequent histories were ascertainable. Five of them, Nos. I., V., VI., IX., and XI., were in good urinary health. Nos. VI. and X. had suffered from moderately severe attacks of cystitis.

Prostatotomy by Suprapubic Route.—Two cases: both cauterised. Both these cases did well. No. II. has had one attack of cystitis since the operation.

SUMMARY OF PAPER.

1. Clinically there are two forms of enlargement—the soft or adenomatous, and the hard or fibrous—but there are many gradations between the two varieties.

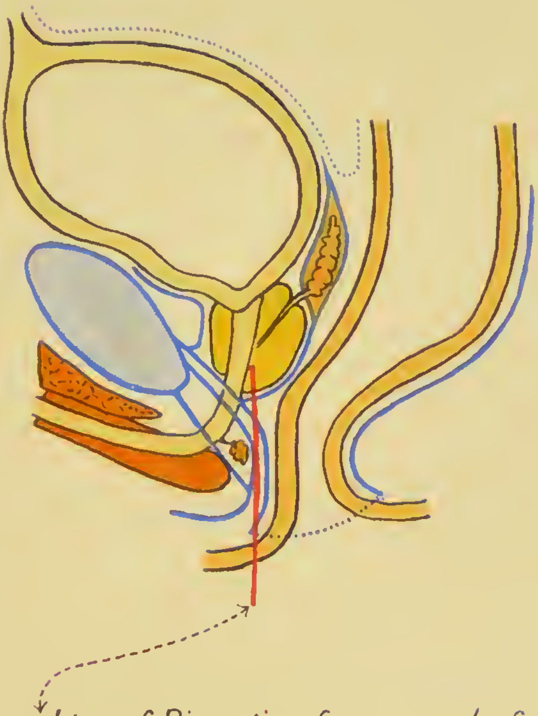
2. The operative treatment of the soft variety is by enucleation—preferably by the perineal route.

3. The operative treatment of the hard variety is by cauterisation through a median perineal incision.

4. A median perineal section should be the first step in any operative measure on the enlarged prostate.

5. That if suprapubic enucleation be performed, the perineal section lessens the risk, since it provides the necessary drainage.

6. That it is necessary to digitally explore the prostate before being able to decide on the proper operative measure.



Line of Dissection for removal of Prostate by the Perineal Route.

SURVEYOR GENERALS OFFICE, ADELAIDE A Vaughan, Photo-lithographer

CRITICISM BY DR. HINDER.

That the shock of the operation depends not so much on the route adopted as on the time taken, and the blood lost. Suprapubic drainage can be efficiently carried out by a tube introduced through the wound, and a catheter tied in the urethra, by means of which a septic bladder may be washed out by the nurse without exposing the patient. I am sorry to hear the Bottini operation condemned, because it gives, in selected cases, extremely good results, and has a very low mortality. I am inclined to think, from my experience of my own cases and that of my friends, that the ultimate result of the suprapubic operation is better than that of the perineal method.

Dr. Maitland, in reply, said: I have endeavored to make clear that there are two forms of enlarged prostate, in which the treatment is entirely different. If enucleation is attempted in some forms of the fibrous prostate, disaster will result.

I have also tried to show that a median perineal section should be part of any operation for the relief of symptoms in an enlarged prostate.

Dr. Hinder has evaded the point at issue with regard to Bottini's operation. I advocate cauterisation—the principle of Bottini's operation—but I object to the cauterisation being done in the dark, and it is evident to every surgeon that if there are two methods of cauterising the prostate, one of which is done in the dark and the other which is controlled by vision, surely the latter—which I advocate—should be the one of choice.

It must not be forgotten that there are many methods of performing perineal prostatectomy, but the one I advocate is quickly performed, and free from hæmorrhage.

Dr. Hinder was not just to the segregator. Some four years ago I advocated its use, and then said that it must be used in conjunction with the cystoscope. These two instruments serve different purposes: the cystoscope gives a view of the bladder, the segregator collects the urines separately from each kidney. Dr. Hinder suggests that an ulcer in the bladder renders the segregator valueless; but that ulcer should be first diagnosed by means of the cystoscope, and then such an end is obviated. Both instruments are aids to diagnosing obscure kidney diseases, but have entirely different objects in view.

SURGERY OF ENLARGED PROSTATE.

By Henry M. O'Hara, F.R.C.S.I.

Mr. President and Gentlemen—I see I am listed to read a paper on Prostatectomy; but it is my intention to give you my experiences in the treatment of enlarged prostate, a subject which has become of great interest to surgeons. If my paper seems disjointed I must crave your indulgence, as it was written during the intervals of a busy practice. Let us hope that, like many other great innovations in modern surgery, prostatectomy will not be overdone and fall into discredit. The dissection of the male perinæum was, to me, in my student days, a difficult *crux;* and after twenty-four years as surgeon to a large metropolitan hospital, and having my share of private work, I must confess I still consider it a most complicated region. It is only a small triangular space bounded by a base line drawn from tuberischium to tuberischium, and the sides of the triangle made by lines drawn from those points to the pubes; but its contents are exceedingly intricate. To understand the supports of the pelvic viscera, it is necessary to know the position of the

pelvic fascia which go to form the floor of the pelvis: these are the obturator fascia, the pyriformis fascia, and the recto-vesical fascia. The most important structure in connection with the surgery of the prostate gland is the recto-vesical fascia. It comes off the inner surface of the obturator fascia forming the white line, with its concavity upwards. It blends posteriorly with the pyriformis fascia. It passes downwards, covering the levator ani and coccygeus muscles, and beneath the bladder prostate and rectum, where it splits into two layers—one being reflected on the rectum, the other, or lower layer, sends a lamina downward to enclose the prostate and forms its capsule, and becomes continuous with the deep layer of the triangular ligament. This fascia completely closes the pelvic outlet and supports the pelvic viscera. My reason for giving this description I shall explain when comparing the different routes in the operation of prostatectomy. Chronic enlargement of the prostate gland has been looked upon as a necessary disease of old age; and, realising the fact that atrophy is the usual accompaniment of senility, I doubt whether every unfortunate old man is condemned to spend the winter of his days in the tortures of enlarged prostate. The idea seemed unnatural to me, and I therefore determined to find out by an examination of as many cases as presented themselves in private and hospital practice. I have now the result of 200 examinations, in patients ranging from 30 years to 82 years, and I come before you to-day to refute the long-established theory of senile hypertrophy of the prostate gland. It is not necessarily a senile condition, and I have discovered a greater percentage of enlarged prostates in men between 35 years and 50 years than in those over 70 years. I have always wondered why the prostate gland should be the only one in the human organism to undergo hypertrophic changes as the result of senile decay. In other glands we expect atrophic changes to accompany old age—particularly in the uterus, which is looked upon as the analogue of the prostatic utricle. As the result of my examination of 200 cases suffering from urinary troubles I found the following results:—

Between 35 years and 50 years, eighty-six cases, with eighteen enlarged prostates; fifty-one had suffered from gonorrhœa, one had tubercular disease.

Between 50 years and 60 years, sixty-two cases, with twelve enlarged prostates; five had marked atrophy, thirty-four had had gonorrhœa.

Between 60 years and 70 years, sixteen cases, with three enlarged prostates; two admitted gonorrhœa, one had atrophy, one had malignant disease, two fibro-myoma.

Over 70 years, twelve cases, with two enlarged prostates; one had atrophy, five had had gonorrhœa.

Thirty-two of these cases required prostatectomy. Of these, eighteen admitted to having had gonorrhœa; five of them had had syphilis also. Only the most serious urinary cases come into a general hospital; hence the large percentage requiring operation. In the healthy adult, the prostate gland is a sexual organ composed of muscular and glandular tissue. In its normal state it plays no part in the act of urination, but its function is to secrete a fluid for the dilution of the semen and nourishment of the spermatozoa. The verumontanum, a vertical elevation on the posterior wall of the prostatic urethra, is its most sensitive part, and becomes intensely congested during the sexual act. This region is a fertile ground for the development and subsequent lodgment of the gonococci. The inflammation extends from this point to the middle and the two lateral lobes, and these in their turn become engorged and swollen. Should the condition go on to abscess, and discharge its contents, the gland will contract as disintegration takes place; and, unless a cicatricial contraction of the urethra at the neck of the bladder

is produced, an atrophied prostate may be the only result. Gout, trauma from instruments, and inflammation extending from cystitis, the result of stone, may also cause inflammation of the prostate gland; but this form generally subsides without suppuration. A very different condition, however, takes place if the acute gonorrhœal congestion, left untreated, goes on to the chronic inflammatory stage. The muscular and glandular tissue may remain permanently enlarged and eventually become a mass of fibrous tissue. The fibrous capsule of the gland also thickens in the inflammatory process, and becomes firmly attached to the recto-vesical fascia, causing great difficulty in separating it from the latter. A gonorrhœal attack invading the prostatic urethra spreads to the substance of the gland; and this condition, being rarely treated, produces a change in the muscular fibres and glandular tissue, and, in a strumous subject, probably it renders the gland liable to become secondarily affected with tubercle. I invariably examine the urethral discharge in every case of chronic prostatitis, and I have discovered gonococci in very old standing cases. It is an easy matter to express any fluid from the posterior urethra with the finger in the rectum. In acute cases, having assured myself of the gonorrhœal affection, I adopt the following treatment:—

Absolute rest in bed, when possible: a bland non-stimulating diet: leeching the perinæum: free action of the bowels, encouraged by salines: the local application of a 25 per cent. solution of argyrol to the prostatic urethra: hot hip baths and hot douching to the posterior portion of the gland, per rectum (this can be very well done with Bozesman's uterine irrigator): suppositories of icthyol. Should the condition not yield to this treatment after a fair trial, it may be necessary to drain the bladder in the suprapubic region, and give the urethra phyisological rest. I have never seen massage to the prostate do any good. From the data obtained from my patients, I feel satisfied that the enlargement of their prostate glands commenced with the later stages of their gonorrhœa, and had these been treated surgically at the time I think the majority of the cases could have been saved the enormous hypertrophic conditions that I found. The treatment of gonorrhœa is much too lightly undertaken, and it is my invariable custom to warn every patient what the direful results may be if neglected. The symptoms of chronic prostatitis may be very slight, and you will often find it difficult to persuade your patient that there is any need for care or treatment. You are all aware, gentlemen, of the complications that may arise from an attack of gonorrhœa; but, as I am only dealing with prostatic enlargements in this paper, I will confine myself to that gland. If the lateral lobes only are enlarged, only slight symptoms may continue for years; but should the middle lobe become affected it bulges backwards and upwards into the bladder, and carries the urethral opening up above the water line, forming a valve-like condition which eventually partially or completely blocks the flow. Before advising a patient to undergo the operation of prostatectomy, except in most urgent cases, I invariably give paliative treatment a fair trial: by this I mean daily irrigation of the bladder with some mild antiseptic lotion—preferably weak solutions of nitrate of silver, one in ten thousand, followed by a wash-out with normal saline solution; the passage of large sounds, curved at first and gradually becoming straighter, so as to bring down the prostatic opening of the urethra to the level of the floor of the bladder. This treatment will sometimes break in a patient to adopt catheter life on his own account, with a certain amount of comfort and safety. Men of placid temperament endure catheter treatment for years without injury to their nervous system. So long as they can attend to strict asepsis, and keep the bladder clean, they are best left alone. Operation is demanded when, from the retention of urine—either partial or complete—these conditions cannot be fulfilled, or where the patient's life is jeopardised through

loss of sleep or general nervous irritation, the result of constant attempts to empty the bladder; where there is any indication of beginning renal disease; also, in certain occupations where it is absolutely necessary to hold the water for long intervals—such as coachmen, enginedrivers, &c. There are two forms of myomatous prostate very similar to those found in the uterus, the hard fibro-myoma—the discreet tumors—and the soft œdematous myoma. They grow slowly and insidiously, and, being of a non-inflammatory nature, do not affect the capsule of the gland, which merely stretches, leaving the recto-vesical fascia intact. We find enucleation from their fascial surroundings is therefore generally easy. The other causes of enlargement of the prostate gland, tubercle and carcinoma, I have only met with on two occasions; but I am inclined to think a small percentage of senile prostates become malignant. Of the thirty-two operations performed, two died—one, a carcinoma, of shock, on the third day, in which I was associated with Dr. Clayton: the other was a large fibrous prostate, the condition being complicated by chronic nephritis. The patient lived for three months, and eventually died of renal disease. One can hardly attribute his death to operation. I now show you his bladder and the prostate, and you can judge for yourselves the result of the operation. Where the condition of a patient is so serious as to require operative interference, several methods of treatment have been advocated of late years; but I will merely mention these, as I have had very little experience in all but prostatectomy. The American and English surgeons originally advocated castration, and I have adopted the plan in three cases. One was certainly relieved, but the other two developed acute mania and never recovered. After an operation for the radical cure of hernia in a man of 55 years, with Dr. Weigall of Elsternwick, acute cellutitis developed in the scrotum. The parts sloughed, and the testicle on the left side, was destroyed. It is ten years since the operation, and, on examination, Dr. Weigall assures me the lateral lobe on the side of the missing testicle is now completely wanting, and the lobe on the sound side is enormously hypertrophied. I think this is a good test that White's operation had something to recommend it.

Vasectomy is too uncertain in its result. It can be done under local anæsthesia; but I would not recommend it, unless the patient refused the more radical operation. The division of the bar at the neck of the bladder by the galvano cautery, or electric knife, as recommended by Bottini, is to my mind a dangerous procedure, even with the aid of a cystoscope, and I cannot recommend it. The most satisfactory operation at the present time is prostatectomy, either by the suprapubic or perineal method, and each has its advocates. Having performed the superpubic operation thirty-two times, with but one death, I must, for the present at any rate, pin my faith to that method. My colleagues at the Alfred Hospital, Messrs. Cooke and Hamilton Russell, have had equally good results.

In nine of my cases I had some post-operative complications; two elderly gentlemen, aged 77 years and 69 years, respectively, became temporarily demented, but have completely recovered. Three had urinary fistula for four and five months: two have completely recovered, but one is still unhealed. One patient developed thrombosis in the deep femoral vein, with much œdema of the leg: he has now quite recovered. Three are obliged to empty the bladder with catheter, having no muscular power in their large sacculated atonic bladders. So that, out of thirty-two prostatectomies, twenty-six are quite restored to health; one, the malignant case, died as the result of operation; and four are relieved. I have not found the help that other surgeons seem to get from the cystoscope: in fact I have given it up in prostatic cases, and rely on digital examination to form my diagnosis. I find with my finger in the rectum, and, having emptied the bladder, I can

always map out the condition. If the urethral groove can be clearly defined per rectum, the trouble is invariably in the middle lobe. Before giving a prognosis as to the probable result of the operation, it is well to find out the amount of muscular expulsive force the bladder has. This can be discovered by passing a catheter, filling the bladder with some solution, and then directing the patient to force the fluid out. If the stream is a fairly vigorous one, the muscular wall must be still active: if the stream merely dribbles out, it is an indication of atony, and the prognosis is bad. However, by removing the enlarged gland, catheter life is made bearable, and the outlet of the urethra having been lowered to the floor of the bladder the decomposing urine that had previously lain in the bas fond can be evacuated.

I am well aware of the brilliant results of perineal prostatectomy in the practice of other surgeons; but the advantages which I claim for the suprapubic operation are :—

1. Rapidity of performance. Since writing this paper I removed an immense prostate at the Alfred Hospital, on September 1st, in the presence of a number of surgeons, two of whom took the time; and the operation took exactly four minutes. The prostates that require removal are generally inside the bladder, and easily accessible from the front. With the bladder filled, and the patient placed in Trendelenberg's position, an incision about three inches long is made in the mid-line, beginning at the pubes. Having separated the recti, divided the transversalis fascia, and exposed the bladder, I fix it with an instrument I now show you, which assists in holding it in position to be opened. I then fix, with a suture on either side of the upper part of the incision, the bladder to the skin: this prevents injury and leakage into the tissues. I expose the gland by an incision through the mucous membrane on its anterior superior aspect, and enucleate with my finger, working from above downward; another finger in the rectum will help to push up the prostate, taking care to avoid tearing the urethra if possible. I have found it a good plan, when the bladder is in a foul condition, to keep up constant irrigation for the first few days after operation, and I have devised this irrigator, which I now show you. It is made of metal, has two tubes—a small one for inflow of saline solution, and a large outflow tube. It prevents the lodgment of any septic material in the raw cavity left after removal of the gland. I think it also helps to control hæmorrhage.

2. It is practically impossible to avoid wounding some branches of the prostatic flexus of veins, and unavoidable bleeding can be controlled by sponge pressure on holders. This is where the value of the facsia on the posterior surface of the bladder can be realised. The fascia and ligaments on the floor give a firm resistance to pressure from above, which you cannot obtain in the perineal operation.

3. No important structures are injured on the route to the gland if the incision is made in Retzius' space, which is above the attachment of the recto-vesical fascia to the pubes. I have always thought that splitting the valuable supports to the pelvic viscera—that is to say, the fascias lying below the prostate—must interfere with the action of the muscles and ligaments about the rectum and neck of the bladder, and to reach a middle lobe or any tumor bulging into the bladder from a perineal opening is a matter of extreme difficulty, and increases the danger of injuring those important structures—the seminal vesicles and vasa deferentia.

4. In cases of very old men it is an advantage to sit them up soon, thus avoiding the danger of hypostatic pneumonia. This can be done with suprapubic drainage; but, with a perineal tube, a patient cannot well sit up.

5. The suprapubic wound can be more easily kept asceptic, the proximity of the perineal opening to the anus causing great risk of infection.

Sixteen years ago, before Mr. Gill published his operation, I performed prostate-myomectomy in two cases, with Dr. Weigall, and they are at the present time in good health : the remaining portion of the prostate seems to have shrunk.

EXHIBITS.

I have brought with me five specimens of enlarged prostate, to illustrate the different forms met with :—

A.—An adenoma of middle lobe, from a patient of 70 years, which caused complete retention. The three calculi accompanying were found in the bas fond. I had no difficulty in shelling out this lobe. There was no history of gonorrhœa.

B.—A typical fibroid from a man of 39 years, who had suffered from gonorrhœal prostatitis at the age of 28. This was a difficult gland to enucleate, owing to the dense fibrous adhesions set up between the capsule proper and the recto-vesical fascia.

C.—A soft œdematous myoma from a man of 52 years. It is very similar to those found in the uterus. He had a history of gonorrhœa.

D.—Multiple isolated myomata, complicated by over 100 prostatic calculi, from a patient aged 77 years, who had history of gonorrhœa.

E.—Large prostate removed on Friday last (September 1st) at the Alfred Hospital. Patient aged 77 years. No history of gonorrhœa.

F.—My bladder irrigator, made for me by Messrs. Mayer & Metzler, London and Melbourne.

G.—My instrument for fixing the bladder before opening. Made from my design by Dietrich, Melbourne.

H.—A bladder from the patient who died three months after operation.

The deductions I draw from my examinations are :—That the hard, fibrous, adherent prostate, in whatever lobe it may be, is invariably the result of a gonorrhœal infection, and may develop at any age. That a mixed fibro-adenoma probably develops its excess of fibrous tissue as the result of gonorrhœal infection, and may develop at any age : that the so-called soft œdematous myoma, which attains such an immense size, is probably the result of a blocking—through some catarrhal condition, probably gonorrhœa—of the lumen of the gland ducts, so that the prostatic secretion is confined in tubules which rapidly become distended : this is the form of hypertrophied prostate, irregular in shape, that so quickly develops, and may involve only one or all the lobes, and is more often seen after 50. It does not become adherent to the recto-vesical fascia, and is therefore easily shelled out. That the simple adenoma is a rare condition, most frequently found in the middle or third lobe, and may develop at any age, has a smooth capsule, and is easily enucleated. That cancer is a rare condition. That tubercle of the prostate is generally a secondary condition.

Prostatectomy might aptly be called the " Finger operation." The diagnosis is made with the finger, and, having exposed the gland, the rest of the operation ought to be done with the finger. It requires a long strong finger, with good tactile sensibility, to accomplish the enucleation satisfactorily ; and I cannot imagine a more pitiful spectacle than a little man with a short finger attempting to dig out a firmly embedded fibrous prostate. Scoops and forceps of any kind can only be a menace to the patient.

In conclusion, I must express my appreciation for the kind and skilful assistance I have received from Drs. Cuscaden, Joske (who completed one of my operations), Power, Lang, Anderson, Clayton, Weigall, Shortt, McKeddie, Harris, Hearne, Pitt, and my house surgeon, Dr. Lind. Their help has been on every occasion a great factor in the success of the several operations, and I take this opportunity of offering them my sincere thanks.

DISCUSSION ON PROSTATECTOMY.

Dr. Steer Bowker: I was greatly interested in what Dr. O'Hara stated as the result of his examinations, that the prostatic enlargement did not necessarily belong to a senile change; for I have often noticed the enlargement in comparatively young people.

The first prostaticalism I did some seven years ago, and was by the perineal route: it resulted in a fistula between the bladder and rectum. This rather put me against the operation. But I have seen such good results at the hands of my colleague, Dr. Maitland, that I am not now so sure as to which is the best method. But of this I am sure, that the operator who always does the same operation is wrong; for judgment must be used, and in each case the most suitable operation chosen.

Dr. Maitland is rather too sweeping in his condemnation of the Bottini operation. I have had some excellent results, and this operation can be done without a general anæsthetic by the aid of adrenalin and cocaine, and as shock and the effect of ether and chloroform in these old subjects should be avoided in certain cases, a Bottini might be done when another operation would probably end badly. Shortly before leaving Sydney, I did one on an old man who had 17 ounces of residual urine. It relieved his frequency, but I was disappointed on trying a week after the operation to find he had still about 8 ounces of residual urine.

I had also some difficulty with a suprapubic operation. It must be a very easily shelled-out prostate which would allow of removal in four minutes, as Dr. O'Hara did, or four and a half minutes, as recorded by Dr. Maitland; for though I am not a weakling, and have fairly strong hands, it took a considerable amount of force, which left me with aching hands for the rest of the evening, to remove the prostate. He had complete retention about a week after. and my house surgeon could not get a catheter in. I had to open the bladder again and do retrograde catheterisation, and left a cathether in. Eventually he became quite proud of his powers of micturition. I think Dr. Hinder is rather rough on the segregator, for no doubt it has it uses, though it may have its errors. To listen to what he says would lead one to suppose that catheterisation of the ureter was quite an easy matter: this it most certainly is not. This method also has its errors and dangers peculiar to its usage.

PERSONAL EXPERIENCE OF THE OPERATIVE TREATMENT OF SENILE DISEASE OF THE PROSTATE DURING THE LAST FIVE YEARS.

By Thos. Fiaschi, M.D., M.Ch., Italy.

The following account covers the cases of senile disease of the prostate operated on by me since my return from South Africa in December, 1900.

The only palliative operative procedure which I adopted was suprapubic drainage; this was resorted to in four cases in which there was a great deal of prostration, cystitis, pyelitis, &c., rendering these cases unfit for the time being for any serious operation. In two of these cases the prostate was afterwards removed successfully. In the third, death took place from heart failure, whilst under an anæsthetic and about to have suprapubic prostatectomy performed. In the fourth case death occurred from uræmia, fourteen days after a stone was removed and a drain placed above the pubis. This patient had almost died of hæmorrhage before the drain was introduced; he had

previously been operated upon as follows :—Lateral lithotomy (in Melbourne), litholapaxy (in London), suprapubic lithotomy (in Sydney), and Bottini (on the Continent of Europe). This would have been a most suitable case for perineal prostatectomy at an earlier stage, before his kidneys got small, granular, and septic. No. 1 in the list of cases had a drain left above the pubis two years before, since no catheter could be passed per urethram. He passed two years in comparative comfort wearing a urinal, but as the prostate enlarged it pressed on the rectum to such an extent as to cause trouble; I therefore removed it.

The operative procedures adopted by me for the removal of the prostate were :—(1) The "combined operation," (2) suprapubic, (3) perineal alone.

In the cases where the combined operation was made use of, the perineal incision was a crescentic one; in front of the anus. A careful dissection was made to the apex of the prostate behind the bulb, followed by incision of prostatic sheath and enucleation with the fingers, aided by pressure from above. Draining was by the perineum, and for two or three days by the suprapubic incision also. In only one case was the suprapubic route alone made use of. In this case the difficulties of enucleation were considerable. Although the result was fairly satisfactory, this method did not appeal to me as a general procedure for the removal of the prostate. Latterly I have come to the conclusion that the suprapubic incision is quite unnecessary, and that the prostate can be removed with greater precision—and under direct view—through the perineal route alone, and that is the operation to which I now give preference.

The following is a short description of the perineal operation which I adopt :—For a few days before the operation I administer urotropine and plenty of water to drink, the dose of urotropine varying with the amount of cystitis or pus in the urine—from 5 gr. to 15 gr. thrice daily. At the same time the bladder is washed occasionally. Immediately before the operation the bladder is washed out with boroglyceride solution, and a soft catheter is placed in the urethra—either a Coudie or a Jacques. The patient is then placed in the lithotomy position, with a sandbag under the sacrum, so as to elevate the pelvis.

The parts having been previously shaved, an inverted Y-shaped incision is made in the perineum. This incision is deepened into the ischiorectal fossæ behind the line of the superficial perineal nerves, and in the middle line it passes through the central point of the perineum, so that the anterior end of the sphincter ani can be pulled back, and the accelerator urinæ forwards. The further dissection towards the apex of the prostate can be facilitated by placing the middle finger of the left hand, protected by a finger-stall, in the rectum. The incision is carried back in the middle line to the apex of the prostate, carefully avoiding the muscles connected with the bulb and the compressor urethræ in front, and the rectum behind. The dissection is then carried towards the apex of the prostate between the anterior fibres of the levator ani muscle (levator prostatæ), and the under surface of the apex of the prostate is well displayed; the finger will then pass easily between the recto-vesical fascia (prostatic sheath) and the prostate. By separating the fibres of the levator ani by means of retractors, and pulling upon the recto-vesical fascia with a pair of forceps, the prostate comes well into view. The removal of the prostate should then commence after all bleeding is arrested. The floor of the prostatic urethra is opened well back longitudinally, and the finger is introduced so as to determine the disposition of the enlarged parts. In the case of a large adenomatous prostate, one lobe is first enucleated with the fingers or a blunt dissector, and then the other lobe is removed. The finger is then introduced into the bladder, and if there is an enlarged middle lobe,

it is turned out and enucleated, or its posterior attachment divided with a pair of scissors. Search is then made with the finger for stones, which are frequently present, although not detectable beforehand. If the prostate is hard and fibrous, the whole substance of the gland can be removed with a tonsil punch, guided by the finger in the bladder through the floor of the prostatic urethra. Lastly the wound is washed, all hæmorrhage stopped, and a large drain tube fenestrated for a short distance from its inner end placed in the bladder. This tube is fixed in the bladder by first passing a stitch through the bladder wall at the posterior end of the incision that was made in the floor of the prostatic urethra, and then passing the thread through the side of the tube within three-quarters of an inch or one inch of its inner end. When the thread of catgut is tied it brings the tube into position. This stitch is made of catgut, which will dissolve in about six or seven days, when the tube comes away. If there is much oozing a light packing of gauze is placed alongside the tube. The superficial wound is then closed.

In the after treatment the bladder is irrigated once or twice daily, and the patient is kept on small doses of urotropine. If any gauze packing is made use of, it is removed after twenty-four hours. The average time for the perineal incision to heal is about twenty-one days.

In the majority of cases the floor of the prostatic urethra was removed, but the roof and sides were preserved. No attempt was made to save the *vasa deferentiæ*. On making inquiries of some of the patients I found they had good power of erection. I question if the seminal fluid is any use without the prostatic secretion, so that it might be useless saving the ducts.

The results, as seen from the accompanying table, were fairly good. There were no deaths, and with the exception of those cases which turned out to be malignant, relief was almost invariable. I kept no record of weights of parts removed, nor any photos, but some were very large. I refused operation only in a few cases where sepsis had invaded the kidney, and in cases of senile heart, and these cases only lived a few weeks. It was interesting to watch the improvement in the renal function that took place when the micturition became normal. Albumen disappeared from the urine, and the amount of urea excreted increased. The operation of perineal prostatectomy properly conducted I consider not as dangerous as Cheselden's operation, and, other conditions being favorable, so soon as cystitis commences from enlarged prostate, I think prostatectomy is indicated. Generally speaking, when an obstructing prostate is the cause of septic cystitis, it is as necessary to remove it as it is to remove a stone from the bladder.

As to the choice of operation, I prefer the perineal route alone, since one can get parts under direct view, and have more control of hæmorrhage and more efficient drain, and the largest stone can be removed safely by carrying the incision in the bladder backwards. The fixing of the drain tube to the wall of the bladder ensures the effective drainage of the urine, and does away with the necessity of pushing it into the fundus and causing pain.

Complications.—Hæmorrhage was not alarming in any of my cases, and it was much less and more under directed control in those cases removed by the perineal route alone.

Incontinence or dribbling sometimes followed the perineal method, but never to any serious extent, so far as I could ascertain. It amounted only to a loss of control when the bladder got a little full, and this invariably disappeared in a short time. I ascribed it to injury to the compressor urethræ muscle when the dissection towards the prostate was carried too far forwards.

Perineal fistula was a cause of annoyance in a few cases, due probably to the tube being retained too long a time, but it never amounted to more than the escape of a few drops of urine when micturating.

Epidydimitis was the most common complication I had to deal with. It occurred generally when the urine began to pass by the urethra. In one case suppuration took place.

There is one other feature in this list, and that is the large proportion of cases in which the disease turned out to be cancer of the organ. The proportion was about 13 per cent. This condition was suspected only in two of the cases before operation. In one case (No. 24) the finger, when passed through the neck of the bladder, gave a sensation like as if it had passed through a carcinomatous cervix uteri from above. Some of these malignant cases got temporary relief, and one had no urinary symptoms for twelve months after operation.

In future I intend to keep a more careful record of the histology of the parts removed, so as to determine more definitely the effects of removal of cancer of this organ.

For the histological examinations I am indebted to Professor Welsh.

URINARY SURGERY.

By Critchley Hinder, M.D.

It is my intention to discuss with you various matters of interest which have cropped up during the past few years in connection with a branch of work in which we must all be considerably interested. I feel that I would rather do this than confine myself to one subject in urinary surgery, for in doing so I should hardly be able to avoid repeating matters which can be gathered from any text-book, and which would be little appreciated by such an audience as that which I have the honor of addressing to-day. As tersely as possible let me give you my own convictions, while mentioning cases which have appeared to me to be of special interest.

Foreign bodies are not found in the bladder very commonly, but they certainly give rise to a considerable amount of frequency of micturition and tenesmus. I met with two cases in women, where the familiar hairpin was passed into the urethra until it slipped beyond reach. In each case the hairpin was located with the cystoscope, and then, by securing the blunt end with a lithotrite, it was removed without damaging the urethra. On two other occasions women in the general hospital were having their bladders drained with glass catheters. The nurse who had charge of these catheters had boiled them until the pot was dry, and the catheters had probably cracked : at all events about 3 inches of glass catheter were left in the bladder. An abler and more careful nurse was placed in charge, and there was no further trouble, except that one other patient—jealous of the interest that was roused—actually broke her catheter in her bladder herself. In each of these cases I located the broken piece with the cystoscope, worked it into position with a small lithotrite, and removed it per urethra. A boy of 5 years passed a pin into his urethra. It disappeared into his bladder. By the act of micturition it was passed into the lower end of his urethra, and slipped back again. With my finger in the rectum I was able, with the help of a long urethral forceps, to secure the pin at the neck of the bladder and draw it out. I removed a stone by lithotrity from an old man of 80 : the nucleus was a piece of gum elastic catheter, which had broken off four months before, and which the patient estimated to have been about an inch and a half long. The old fellow said that he had missed the broken piece, but had no idea that it

could possibly have been left in his bladder. In fact, with a cystoscope and a lithotrite, practically anything which can be passed into the bladder can be removed without a cutting operation. I have removed fairly large stones from the urethra on two occasions. In each case the stone was about the size of a pigeon's egg, and had formed in a pocket in the urethra immediately behind an old stricture. I have twice found stones in the substance of the prostate gland, once in the prostatic urethra, and once in a man of 42, who had a slight median lobe enlargement. The lobe was so developed that it formed a little pocket on the distal side.

During the past eight years I have only been compelled to cut for stone three times. One stone weighed 8 ounces, and was so very hard that the lithotrite was powerless. This patient was a flower-seller in the city, and only 21 years of age. In another case a man of 72 years had an hour-glass stone so firmly held by the lower and larger end that it could not be dragged out of the hole in which it was pocketed; in fact, it was a difficult matter to do so after a suprapubic opening had been made. On another occasion a flat dark stone was localised in a little pocket in the bladder wall. This stone appeared to exactly fit the cavity, and could not be dislodged with sound or lithotrite. A suprapubic opening revealed the fact that the stone, flat and smooth, exactly fitted the depression. In crushing a stone it is better to use a lithotrite, about number fifteen, and a flushing catheter about the same size; larger instruments are rarely necessary, and they are inclined to tear the urethra.

The cystoscope should always be used after the operation has been completed, in order to see if any fragments remain. The old rule, that we may consider the bladder cleared if we hear no further rattling against the evacuating catheter or if we feel no fragment with the sound, is a fallacious one, as on four or five occasions the cystoscope has revealed the presence of a fragment which had escaped detection, or another stone which had formed on the bladder wall in a limpet-like fashion, notwithstanding the fact that an ordinary examination had convinced me that I had left the bladder clean. On one occasion another stone as large as a pigeon's egg was discovered at the end of four months after operation, showing how remarkably quickly stones are formed. Lithotrity should be performed with the greatest gentleness, and, though it may not be possible to avoid the production of a little hæmorrhage from the prostatic urethra in old men, the bladder wall need not receive the slightest damage. If the patient is kept in bed for a few days, so as to diminish the irritability of his bladder wall, small stones may be crushed without an anæsthetic. I have operated in this manner on three occasions in feeble and phthisical patients. The lithotrite should be introduced once only in the majority of cases; frequent introduction of instruments, and the use of large instruments, is accountable for a great deal of the discomfort which lithotrity patients experience after operation. The pelvis should always be tilted, so that the stone may be crushed away from the sensitive trigonal region. Very frequently patients are able to get about on the third day, or even the following day if the stone is small.

I have on four occasions been able to detect with the cystoscope the presence of a stone in the lower end of the ureter. The history of the case, the puffy appearance of the lower end of the ureter, together with a slight bulging of the bladder wall immediately above the orifice, made the diagnosis fairly certain. A good skiagraph under such circumstances is invaluable if further verification is needed—though, in one case, the cystoscope revealed a small stone which two or three photographs missed.

Stones in this situation are best reached by placing the patient in the Trendelenberg position, and, through a suprapubic opening in the bladder,

slitting up the ureter; or the orifice of the ureter may be dragged down, a probe passed up it, and when the stone is reached the stone and the probe are pinched up in a pair of forceps and cut down upon. In this manner stones may be removed at least one and a quarter inches from the urethral orifice.

The free abdominal incision adopted by some men for the removal of stone in the lower part of the ureter is sometimes followed by the most disastrous results. Owing to the necessary interference with the nerve supply the muscles waste, and a large ventral hernia may be formed. It certainly is much wiser to make all abdominal incisions away from the median line, parallel to the line of the intercostal nerves. Thanks to the excellency of recent skiagraphic work, stone in the kidney is diagnosed with much greater ease than was formerly the case. In removing a stone from the kidney I draw the kidney on to the loin, if possible; and we all know how often this manœuvre is simply impossible. A small shallow incision is made in the middle line and my finger does the rest. This is safer than making a large deep incision. On one occasion I met with dangerous hæmorrhage, which I was unable to control without removing the kidney. It was owing to the fact that the renal artery had bifurcated, and the lower branch had been torn across. Under ordinary circumstances simple pressure and mattrass sutures will be found sufficient to stop any renal hæmorrhage occasioned by a median incision. In cases of suppression of urine, associated with stone, it is safer to incise the kidney and its capsule, allow it to bleed, and leave the stone until the patient has recovered somewhat. These patients have very little reserve power, and usually collapse suddenly. Primary growths of the bladder form but a small proportion of the urinary cases which fall into the surgeon's hands. A portion of the bladder wall should be removed with the growth in every case. None of my cases lived more than twelve months without recurrence.

Villous papillomata are rarely single. I have only met with one of this kind. There are usually two, or perhaps several, patches to be found. They should be removed as early as possible, partly because one can never tell whether the growth is wholly simple and partly because these growths have a strong tendency to degenerate and become carcinomatous. As an example, let me quote the case of a man of 45 years of age, who had been ill for five years with occasional attacks of slight hæmaturia with frequency, and a little cystitis. These attacks appeared to be so slight that for a long time he paid little attention to them. When I examined him with a cystoscope a villous-covered growth could be seen, which, when removed, had a basal attachment of the size of my forefinger nail. An examination by Professor Welsh showed it to be a villous papilloma with carcinomatous degeneration at the base. Another patient had large villous patches—in fact, I might say masses of growth scattered all over the bladder. His history extended over three years, and it was only during the past few months that the hæmorrhage and frequency had increased to a marked extent. On introducing the cystoscope it felt as if it was thrust into a soft loose sort of seaweed. I opened suprapubically, and was astonished at the large masses which were present. A small portion removed for examination purposes was reported by the pathologist, Professor Welsh, to be a carcinomatous degeneration of a villous papilloma. The long history would be apt to induce us to presuppose that the growths were originally benign. Many malignant growths appear to become covered with villous processes. Our knowledge of these cases, then, convinces us of the necessity of advocating the early removal of villous papillomata—or I might go further, in saying that it forces us to the conclusion that every case of hæmaturia, however slight, should be submitted to cystoscopic examination early and while the blood is present in the urine.

About four years ago, towards the end of summer, I saw a man of 28 years of age. He told me that he had had slight hæmaturia and frequency during this and the previous summer, but that during the winter he was perfectly well. The cystoscope showed an irregularly nodular and superficially ulcerated surface, which appeared a little like tubercular disease; but, per rectum, there was a density that I could hardly understand, in fact I only put aside carcinoma because of the patient's youth and the history of recovery during the winter. I drained through the perinæum; but, although he seemed to improve at first, he was soon about as bad as ever. He was going to England, so I asked him to see Hurry Fenwick. Fenwick was at first in doubt, but after a short time came to the conclusion that the condition was carcinomatous. Henry Morris concurred. The man died eight months after of extensive malignant disease, so that he had symptoms for about two years and eight months. He had a strong tubercular history; and I often wonder whether he had a tubercular bladder with superadded carcinoma. At the same time, Fenwick has reported two cases of early malignant disease very similar to this.

Unfortunately, malignant disease may advance considerably without any sign or symptom. I once saw a man who had a sarcoma as large as a walnut situated above and to the right of the urinary meatus. Four days before he was sent to me he had a profuse hæmaturia: previous to this he was absolutely unaware of any indication of illness. My experience of malignant disease of the kidney has been a sad one. It is difficult to detect until well established, and has invariably recurred within twelve months of the removal of the organ. No doubt all hope of success must depend on early recognition. Malignant disease of the prostate appears to me to be very difficult to recognise. I have had very many cases of enlarged prostate sent to me for removal which have proved to be malignant; and, again, I have seen many cases where I found it impossible to say whether the case was malignant or inflammatory until the patient had been under observation for two or three weeks. These latter cases were usually cases of slight hypertrophy of the prostate, in which inflammatory conditions of a low type had been set up—either by the presence of stone or by too frequent and at times altogether unnecessary catheterism. Slight irritation and frequency in early enlargement of the prostate is more easily set right with a dose of saline aperient than with a catheter. Malignant disease of the prostate is very common, and it frequently intrudes itself in cases of hypertrophy of the gland; but my own personal observation has shown me that its chronicity is often a very marked feature. As an example, let me quote a case. A man well over 60 years of age was sent to me. I stated that he had undoubted malignant disease of the prostate, and operative treatment was out of the question. He did not feel very ill, and was anxious to go to Queensland in connection with a mining matter. He came back at the end of twelve months, a stone heavier than when he left, and with about the same amount of slight frequency. The patient was sent to me again, and a rectal examination showed that the growth had spread across the pelvis to a marked extent. Within a few weeks he suffered more pain and went down hill, dying within six months. This case is an example of a statement which I have frequently made, namely, that malignant disease does not *per se* give rise to loss of weight, but that the loss of weight is always due to interference with the body functions, or the mental worry occasioned by pain, or the knowledge of its presence. If ulceration takes place the consequent illness is due to mental worry and septic absorption, and not to the mere fact that the disease is malignant. I have operated upon the prostate for non-malignant hypertrophy one hundred and four times. In forty-seven cases by partial

prostatectomy, twice by simple section and drainage, twenty-four times with Freudenberg's modification of the Bottini instrument, and thirty-one times for the complete removal of the gland.

Simple perineal section and drainage I found to be of little permanent service. Partial section was done on all sorts of cases by the suprapubic method, and perineal drainage was adopted so that the tube might maintain a free opening while scarring was taking place. The mortality was 12 per cent.; but some of the patients had to be relieved, and the additional re-section hardly aggravated the shock attending the cystotomy, which gave them their only hope of relief. Some of these partial re-sections were done eight and nine years ago for absolute retention, and are perfectly well now. I find that partial re-section relieves impotence and need not necessarily cause it. In order to get a high percentage of recoveries from prostatectomy great care must be made in the selection of cases. Freyer is reported to have said that he has operated upon only one out of every twelve cases sent to him. I have been somewhat unfortunate, having lost seven cases out of thirty-one. I say unfortunate, because I could hardly prevent their deaths; three died uræmic a little over a week after operation. One of these came to me with profuse hæmorrhage: even after suprapubic cystotomy I was unable to stop the blood which oozed up from his prostatic urethra, so that at the end of three days I was compelled to remove the gland; it weighed eight and a half ounces. The patient was 79. The hæmorrhage ceased, and, so far as his bladder was concerned, he was well, but hiccoughing set in and he died of uræmia on the eighth day. As an evidence of what will happen at times, my friend Dr. Clubbe tells me that he had a patient a short time since who hiccoughed continuously for a fortnight after prostatectomy and then recovered. Two others died of pneumonia. Another at the end of the sixth day raised himself on his elbow, to speak to the nurse, and fell back dead. Another man, who suffered badly from prolapse of the rectum before operation, strained when the nurse was administering an enema on the fourth day to such an extent that the nozzle of the Higginson enema tube penetrated his bowel, for he experienced sudden pain and died in twelve hours. I found the enema in his peritoneal cavity: since then I have vetoed enemata in these cases. It is hardly right to refuse operation to an old man if he is prepared to take the risk. He knows too well what a miserable existence he is leading, and we know what a miserable death he is likely to die if left alone, apart from the by no means uncommon incidence of malignant disease. I urged an old man to submit to operation for hypertrophy of the prostate: he objected. About six years after he came under my care with great frequency and bladder spasms. He asked for operation, but I refused, because I felt that his condition was almost hopeless. He died within two weeks in abject misery, with intensely painful and almost continuous spasms up to the time of his death. The manner of his death made a great impression upon me. Ever since that time I have simply operated with a view to cure, if possible; and if not, to give the patient a maximum amount of comfort.

A train of accidental circumstances has given me a poor percentage of recoveries, but notwithstanding this I feel that I acted in the best interests of the patients in operating. The actual enucleation of the prostates took from two and a half to eight minutes. I prefer the suprapubic method, because there is no chance of interfering with the sphincter, the patients are more comfortable, and can be easily kept clean by the nurse. I operated perineally once. I saw no advantage in the operation, and the patient used to get dirty—his scrotum hung into the wound, and he resented the necessary meddling with his genitals. All my cases who lived recovered with normal

micturition. I do not decry the perineal operation, for I feel that a careful operater could get as good results from this method as by the suprapubic method. The Bottini operation I have performed on twenty-four cases, and without a death. Only small prostates were attacked, and some of the patients had very dirty bladders. In five of them there was foul urine, and tenderness on pressure over the kidneys; whether it meant pyelitis or not, I cannot say, but they gave me the impression that they would stand very little in the way of operative treatment. On four occasions a stone was crushed and removed, and the prostate dealt with at the same sitting. Only small prostates were operated upon though the residual urine varied from one and a half to thirteen ounces. On one occasion when I attacked a prostate which, I should say, would have weighed about two and a half ounces, the final result was by no means equal to that of the others, and the recovery took some time owing to the many necrotic pieces which of necessity had to be passed. My experience has convinced me that the Bottini operation is well adapted for small prostates, which, although small, may give rise to marked or even complete obstruction to micturition, or to irritable and frequent micturition—disagreeable symptoms, which appear to annoy most prostatic patients. The risk attending the operation is certainly small, for I have never refused this operation to any patient with a small prostate whose symptoms have demanded relief. If the operation falls into disfavor it will be because it has been used indiscriminately, without due thought as to its action and its power to give relief,

Tubercular disease of the genital organs is fairly common. It is astonishing how often a tubercular deposit will be found in the testes of a man who is the picture of health and vigor, and this certainly occasionally leads the practitioner astray. Re-section of the diseased part is attended with good results in early cases, and one is not averse to such a procedure because any recurrence is so easily detected.

Operative interference in the case of tubercular disease of the prostate and the vesiculæ seminales should not be lightly undertaken, for we all know what a remarkable effect climatic and dietetic treatment has upon tubercular conditions, and how dangerous it is to import the septic element into such cases; so that, unless old inflammatory tissue gives rise to pain and discomfort, they are much better left alone.

Tubercular ulcer of the bladder almost always occurs in the vicinity of the urethral orifice. If the irritation is marked, drainage and careful frequent washing out with weak solutions of silver nitrate is of service. In all cases open-air treatment and rest is invaluable. In tubercular ulcer there is a marked tendency to recurrence, even after a lengthy period without symptoms. The simple ulcer is nearly always solitary, and very like a gastric ulcer: bladder drainage and washing out three or four times daily gives excellent results. I should like particularly to call your attention to the ease with which the ureter can be felt bimanually, especially in women. Even in thin normal subjects the ureter can sometimes be felt. In suspected tubercular disease a thickened ureter, which can often be felt bimanually, is a valuable diagnostic aid.

Re-section of a part of a tubercular kidney is to be condemned: either remove the whole, or leave it alone. In desperate, or, I might say, hopeless cases, the kidney might demand drainage for the relief of pain, but I am under the impression that the patient will usually live longer if left alone.

There is no reason why the whole of a tuberculous ureter should be removed, though some advocate it. The incision required would be very large, and such huge rents are a grave menace to the integrity of the abdominal wall: besides, deposits would still be left in the bladder wall. On the other hand, I have myself had cases in which a distinctly tubercular ureter was left and

tubercular deposits were to be seen in the bladder, and yet these evidences completely disappeared after the removal of the affected kidney, and the building up of the patient's health.

Some might say that recurrence is certain. That is not altogether correct; for, although the probability of recurrence is great, still the same can be said of tubercle in other parts of the body.

I have absolutely no experience of tuberculin in urinary surgery. Urethral discharges are a source of great worry to patient and practitioner. In gonorrhœal urethritis the very best treatment is to put the patient to bed, give salines, plenty of diluents, and encourage him to return to a full diet, free from alcohol, as soon as possible. Locally there appears to be no special virtue in any antiseptic. The silver salts are probably the best, but all injections must be used in weak solution and frequently, say about every three hours.

My experience has been more closely associated with chronic cases of urethral discharge, and, though I use the urethroscope freely, I must confess that ulcerative patches necessitating prolonged treatment do not come under my notice: in fact, in a large number of instances, the patients are suffering from the effects of too much treatment. They will not allow their medical attendant to practise a masterly inactivity, but worry themselves and their adviser until, acting against his better judgment, he treats the urethra. At the same time there are cases where a distinct cause for a chronic urethritis may be discovered. Let me quote two or three cases of chronic prostatitis, where there is a little excess of the normal mucus in the urethra and little whitish casts of the prostatic ducts are found in the urine. These patients often complain of an uncomfortable ache in the perinæum, aggravated by erections or by connection, or by a loaded rectum. Per rectum, the prostate feels somewhat hard and toughish. A linear prostatotomy usually gives great relief, and will almost invariably get rid of all symptoms. It is a pity that more active measures are not adopted with the object of bringing about the early relief of acute inflammatory conditions of both prostate testis and sacculæ seminales, all of which appear to me to be often followed by impotence. I feel convinced that many cases of supposed sterility in women are due to impotence in men; but women bear the brunt of the trouble more easily than men. I have seen patients with purulent urethral discharges which were due to tubercular disease of the vesiculæ seminales, and they have been much annoyed by being told that it must be due to gonorrhœa. A suspicious discharge may be caused by malignant disease, but such a condition is not often seen in malignant disease.

Undoubtedly the most difficult cases to handle are those in which the patient refuses to believe that he is well, and, by persistently squeezing and manipulating his urethra, he irritates the mucus glands so that he is able to express a little glairy material, which the quack trades upon. The consultant is able to deal with this class of patient by giving him a thorough examination, and quietly winning his confidence. The patient receives the same advice from his family physician, but the weight of authority assists the consultant, so that his expressed opinion conveys greater conviction, and consequently the hopeless malady gradually disappears.

Friends of mine have very often asked whether it would be worth while for them to get a cystoscope, or a segregator. The segregator has recently caused a stir among British surgeons, although the instrument has been in use in America and on the Continent for some little time. Probably after a time its use will not be so strongly advocated. The instruments of Cathelin or Luys are the most complete, and act the most efficiently as separators of urines. The segregator is of use only if the urine is perfectly clear. We

know that experimentally, under favorable conditions, it has been shown that it does separate the base of the bladder into two parts ; yet, under most circumstances, we can only conjecture that it does do so, for the simple reason that we meet with cases which the cystoscope will show are positively inadmissible for the use of the segregator. At all events, in cases where the urine is clear, we may by examination discover a considerable difference in the character of the two samples obtained. We may, for instance, discover the presence of a unilateral nephritis, or certain details which would considerably affect the prognosis of the case. If the urine is not clear the segregator, unassisted, gives no definite information. If pus is obtained from one catheter, why may it not come from a patchy cystitis—from a tubercular or simple ulcer ? Suppose the segregator acts so as to completely shut off one side of the bladder, and blood-stained urine flows from one catheter only, the blood may come from a ruptured varicose prostatic vessel ; I have seen this on more than one occasion. Or it may come from a papillomatous or malignant growth, or from an ulcer near the orifice of the ureter. The fallacies of the segregator must be evident to anyone who will give the subject a moment's thought. If, on the other hand, a cystoscope supplement its use and clearly defines the source of that which alters the appearance of the urine, then the segregator may be of some service as a rough collector of the two urines, inasmuch as we can never swear that it shuts off one side from the other with accuracy. I would like you to clearly understand, therefore, particularly in cases where the urine is turbid from any cause, that the segregator used by itself is apt to be a delusion and a snare.

On the other hand, let us examine for a moment the capabilities of the cystoscope, particularly with reference to those points which the segregator is expected to clear up. Let us deal with a case of proposed nephrectomy. The surgeon's great desire is to know the condition of both the unsound and the apparently sound kidney. Kidneys are usually removed for growth, tubercle, or advanced suppurative disease. Suppose we suspect the presence of growth, the cystoscope will indicate the side from which blood is coming—and renal hæmaturia is very commonly associated with the presence of growth in the kidney. It is extremely rare to find a growth in both kidneys, at any rate in the early and operable stage of its existence, so that if clear urine were seen to be regularly emitted from the other ureter there need be no hesitation in removing the affected kidney. Suppose we are dealing with a suspected tubercular kidney. It is almost invariably to be found that the orifice of the corresponding ureter will show some sign of tubercular infection, and perhaps pus or *debris* will be expelled with the urine. If, at the same time, no pus is seen to be expelled from the other ureter, and if its orifice and the adjacent bladder wall are healthy, one can infer with tolerable certainty that this kidney is unaffected ; nor is it likely that any number of examinations of the urine collected from this kidney will show any positive indication of the presence of tubercle if the visible signs I have already mentioned are not present. Again, if advanced suppurative disease is present and an exploration is made with a possible view to complete removal, it is an easy matter to wash out the bladder and watch the other ureteral orifice, in order to see whether clear urine only is discharged.

I very much doubt whether the collection of the separated urines can supply information of greater practical value than that which can be obtained from the careful examination of the bladder and ureteral orifices. A careful use of the cystoscope has enabled me to remove the kidney for malignant, suppurative, and tubercular conditions seventeen times during the past eight years, without a death. Any surgeon, with the same information, would probably get equally good results.

The measurement of the renal capability is always a difficult task; and every now and again suppression of urine, or a uræmic condition, will be established when we least expect it. On the other hand, the recuperative power of a kidney will occasionally astonish us. We know of instances where one kidney will have been completely destroyed, while the other is a mere pathological remnant, and yet the patient has lived with it. I once operated on a man for a suppurative kidney and drained it. I knew that it was secreting very little urine, as the whole of it came through the loin. After three weeks he had a sharp attack of pain in the opposite kidney and suppression. I cut down on this kidney, removed a stone from the cortex without any difficulty—or I should certainly have left it for a later operation. This kidney secreted nothing for three or four days, but the opposite kidney, hitherto secreting a little pus and urine, roused itself, and urine poured away fairly freely, until eventually both wounds closed and the man recovered.

The accurate collection of the separated urines, whether by means of the ureteral catheter or segregator, is beset with such difficulty in those surgical cases where we most need it that I fear that its efficacy as a practical working method must be very limited. In theory the idea is charming, but as a working method which can be readily adopted I fear it will be found wanting.

TREATMENT OF CEREBRAL HYDATIDS—WITH NOTES OF A FURTHER CASE.

By ALEX. MACCORMICK, M.D.

In the transactions of the third session of the Intercolonial Congress held at Sydney, 1892, Dr. Verco, of Adelaide, in a paper on Cerebral Hydatids, brought the literature of this subject up to that date. According to Verco, six cases of cerebral hydatids had been operated on and published before that date, and out of the six only one had recovered, viz., the case published by Drs. Graham and Clubb. Since 1892 I can find record of only seven more cases, and I append notes of another case, which will make eight cases recorded since 1892. Of these additional cases six are reported to have recovered.

Summarising all the fourteen cases, three methods of operation have apparently been adopted, viz.:—

1. Trephining and removal of a moderate-sized disc: emptying and removing the cyst.

2. Trephining; emptying hydatid cyst with aspirating needle; replacing disc, and, at a subsequent period (about a week or ten days), re-opening the wound and removing the cyst, after opening the dura mater.

3. Osteoplastic re-section of a large portion of bone, fully exposing the suspected area of cerebrum; opening the dura mater and removal of cyst.

It is impossible to estimate the true death rate of the operations for cerebral hydatids, as the successful cases are the more likely to be published; but, in any case, it has been very high. According to the published cases the last two methods are equally successful, but, with further experience, I am more convinced of the correctness of the conclusions which I recorded (*A.M.G.*, November 20th, 1904), viz.:—

1. That, as the diagnosis of hydatids is generally uncertain, osteocutaneous re-section enables the surgeon to explore a larger area of brain surface.

I would further suggest that in cases where the hydatid is not at once evident on reflecting the flap, or where there is much shock, the flap be replaced and the operation completed in a week or ten days.

2. That this method has the advantage of leaving the skull whole.

3. That the cyst can be emptied without so much risk of infecting the surrounding tissues. It might even be turned out whole, and therefore there would be no need for much handling of the brain for lavage.

In the case here reported I used a guttapercha drain internally, *i.e.*, within the cavity left by the cyst, and a gauze drain outside the dura mater. I used the film of guttapercha tissue internally with the idea of easing tension, should the cavity fill up with cerebro-spinal fluid, as it subsequently did : the gauze outside the dura mater was to drain the serum, which must inevitably accumulate under the bone, between it and the dura. This was apparently a wise thing to do, since the temperature remained normal for five days ; and subsequently, when it did rise, it generally came down after a free escape of cerebro-spinal fluid. Apparently it is wise to limit the escape, but not to block altogether the flow of cerebro-spinal fluid—to stop it gradually.

The case is as follows, taken from the House Physician's notes :—J. W. H., aged 12, male, was admitted into the Royal Prince Alfred Hospital on February 17th, 1905—In a "delirious" state. According to the account given by his friends, he had been complaining of headaches for the last fifteen months. They were not severe at first, and did not prevent him going to school ; lately they have been more severe. For three weeks he has been blind of one eye, the left, and at the same time a weakness in the left leg has been noticed ; one week ago he became blind also in the right eye. Has fits of screaming, and complains of very severe headaches and vomiting. There is marked paresis of the muscles of the left arm and leg. Fundus examination showed marked changes in both discs and retinæ—the discs were blurred, and the vessels indistinct. The head was much larger than normal, but the enlargement was uniform.

Dr. Scot Skirving and myself agreed that there was probably a hydatid in the right Rolandic area, and that an attempt should be made to remove it.

For the following notes I am indebted to Dr. McKelvie, my house surgeon. On February 28th, under chloroform, a large osteoplastic flap—about four inches in diameter—was turned back from the right Rolandic area. The dura mater bulged through the opening in the skull, and showed no signs of pulsation. A whitish area showed through the dura, as if a cyst were underneath it. An incision was made in the dura just inside the margin of the bone, when a hydatid cyst immediately protruded in an hour-glass-shaped fashion. The head was tilted over, the cyst punctured, and the contents emptied into a basin, the tissues being protected from contact with the fluid. The cyst was then lifted out in one piece. The space that was left would more than accommodate a cricket ball. The sub-dural space seemed to be obliterated at the point where the cyst was opened, so that there was no apparent escape of cerebro spinal fluid at the time of the operation. The dura fell away from the bone around the opening in the skull, so as to leave the bone overhanging all around so much that altogether there seemed to be about a quarter of the space inside the skull empty. The opening in the dura mater was closed with fine mattress catgut sutures, and a fine guttapercha drain placed leading to the cavity occupied by the cyst. A gauze drain was placed in the wound leading to the space between the bone flap and dura mater, when the flap was replaced. The fluid removed contained little white bodies, which were scolices in a living state, as demonstrated by the microscope on a warm stage by Dr. Buchanan. March 1st, 1905—Passed a good night, pulse 108, temperature normal, no vomiting, drain removed.

March 2nd—Getting on well, pulse 108, temperature normal. A good deal of clear fluid is escaping from the wound, the dressing on which has to be frequently changed. No vomiting. Complains of headache occasionally. Had convulsions on left side of face when being dressed. March 5th—Doing well, temperature normal, pulse between 98 and 100. Occasionally has pain in vertex. Can move his arm and leg much better, sight not much improved, wound looking well. A good deal of cerebro-spinal fluid escapes. March 6th—Temperature suddenly shot up this morning to 103 ; no vomiting. From the 6th March to the 20th, temperature very variable. On occasions patient dull and lethargic—paresis entirely gone. The temperature generally comes down after a more profuse discharge than usual of cerebro-spinal fluid.

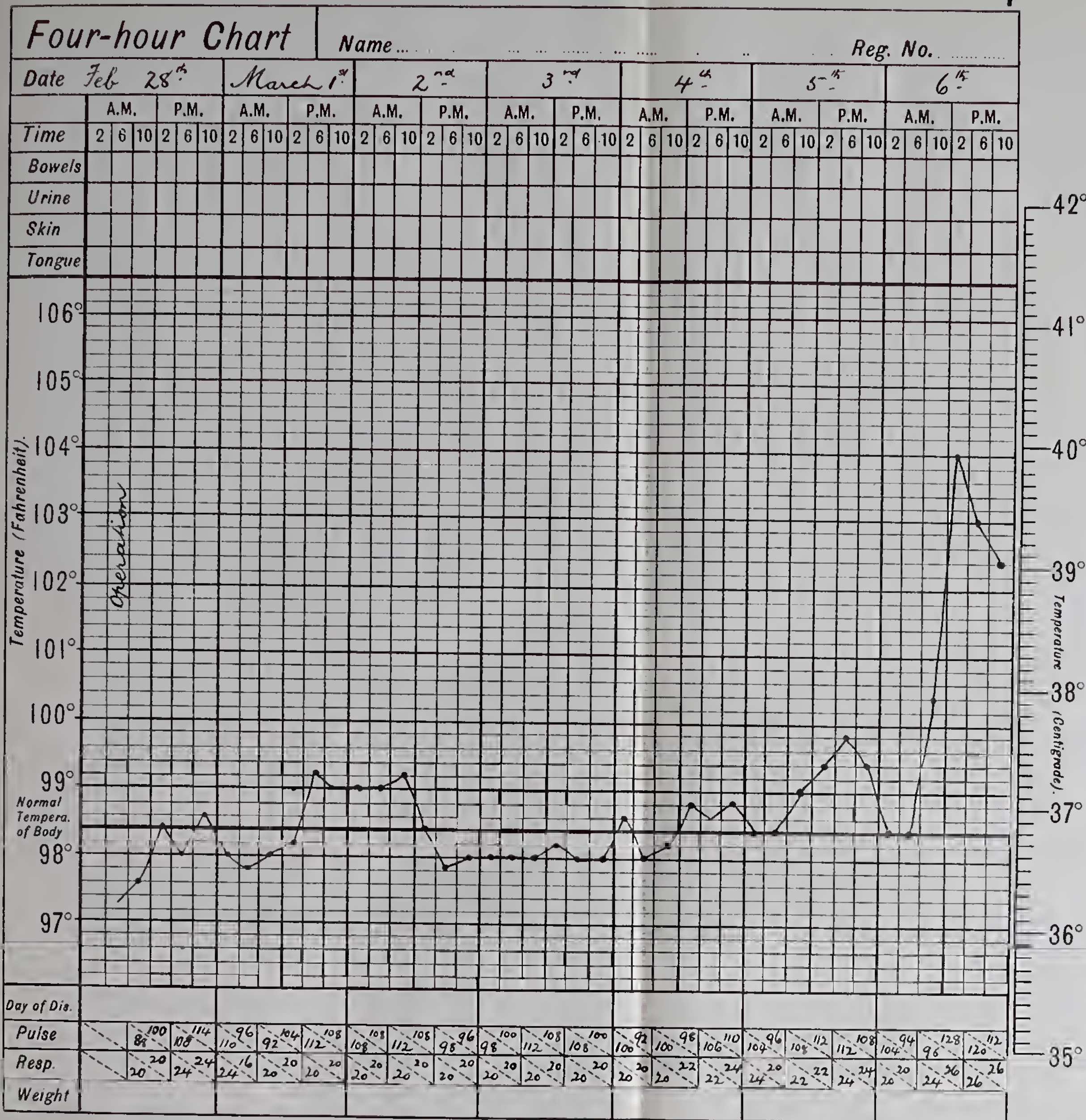
About this date the discharge of cerebro-spinal fluid ceased, and then the wound appeared to be healed. Gradually his intellect improved, but he could only distinguish light from darkness. One month after the operation he was walking about. A few weeks ago—about the end of July—he was reported to be well, but there was no improvement in his eyesight.

INTESTINAL ANASTOMOSIS.

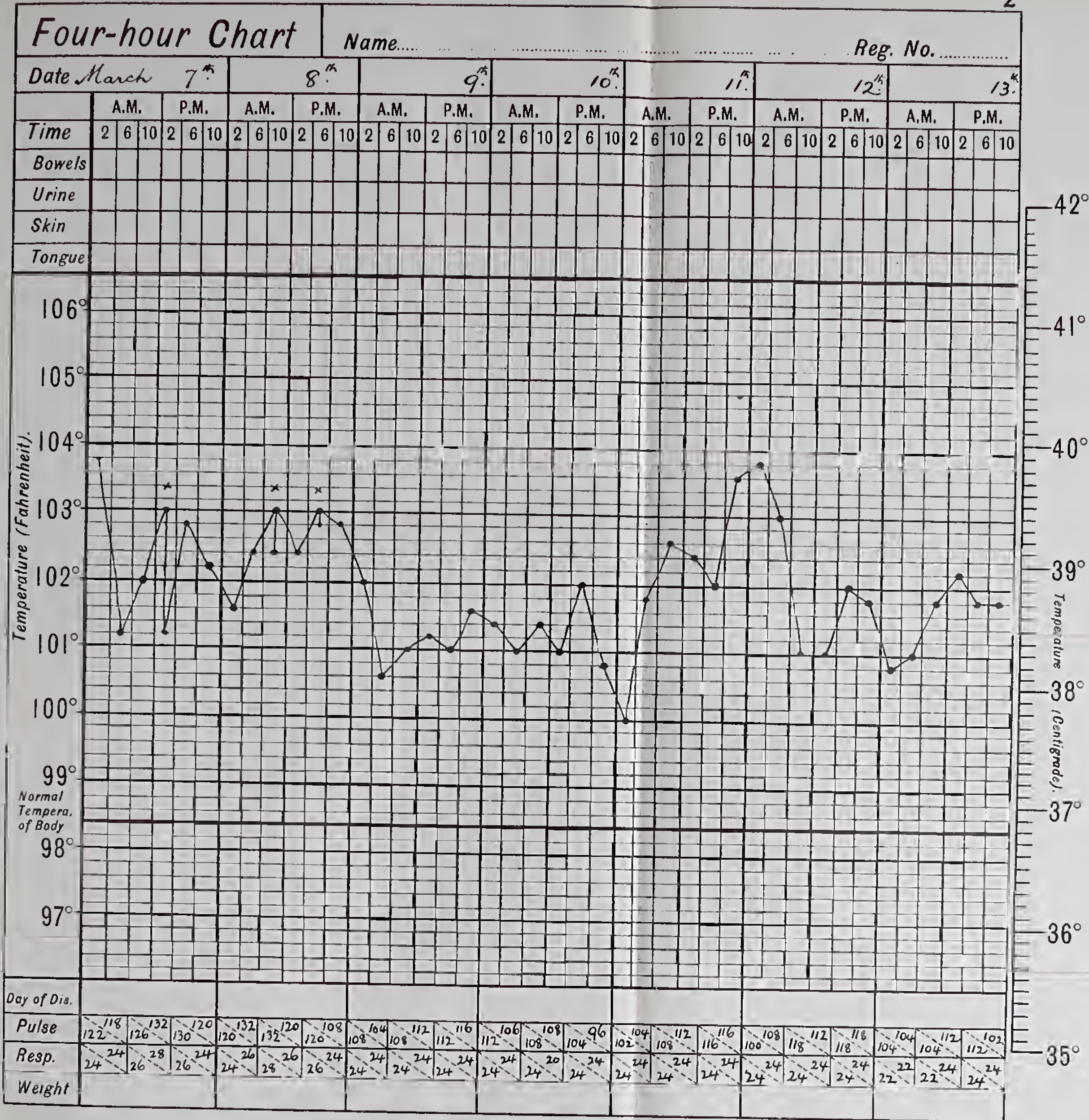
By Alex. MacCormick, M.D.

The following three cases, in which I established an anastomosis between different parts of the intestine, are, I think, of sufficient interest to place on record, as illustrating some of the uses to which this procedure may be put.

Case I.—Mrs. E. J. W., age 25, was admitted into the Royal Prince Alfred Hospital, April 28th, 1897, complaining of a fœcal fistula on the anterior abdominal wall above the pubes. Dr. Brook Moore, of Bathurst, who sent the patient to me, gave the following interesting history :—" I first saw her on February 17th, at her own home, twenty-four miles from Bathurst, in a tent on a mining camp, at 4 p.m. She had, the previous night, been delivered of a dead six-months fœtus : for a month previous to this she had been feeling ill. An unqualified practitioner was called in to remove the placenta. In doing so he evidently pushed his hand through the uterus, and pulled down a quantity of small intestine. When I saw her she was suffering intense pain, and looked desperately ill. I found a large rent in the uterus extending into the vaginal vault, and several coils of intestine hanging into the vagina through the rent : one coil had its mesentery detached for about 8 or 9 inches. I douched the intestines and vagina with hot saline solution, and then reduced the intestine—with the exception of the latter coil, which would not stay back, so I thought it best to leave it where it was, hanging out of the vagina. I expected she would die before next morning. I visited her daily for a week. A large piece of small intestine sloughed off, leaving a fœcal fistula through the torn cervix uteri. She was very ill during this time. I had her removed to the Bathurst Hospital, with the idea of performing an intestinal anastomosis. She had horrible eczema of the skin of the vulva and buttocks. On March 22nd I opened the abdomen, and found many recent adhesions, which bled freely on separating them. I was able, however, after a very tedious dissection to trace the small intestine from the duodenum to the fistula ; but the patient began to feel the shock so much, on account of her already weak state, that I decided to hurriedly divide the intestine, leaving a stump of about 3 inches connected with the uterus. I closed the intestine rapidly with a row



SURVEYOR GENERAL'S OFFICE, ADELAIDE A. Vaughan, Photo-lithographer



SURVEYOR GENERAL'S OFFICE ADELAIDE A. Vaughan, Photo-lithographer

Four-hour Chart

Name .... Reg. No. ....

Date March 14th, 15th, 16th, 17th, 18th, 19th, 20th

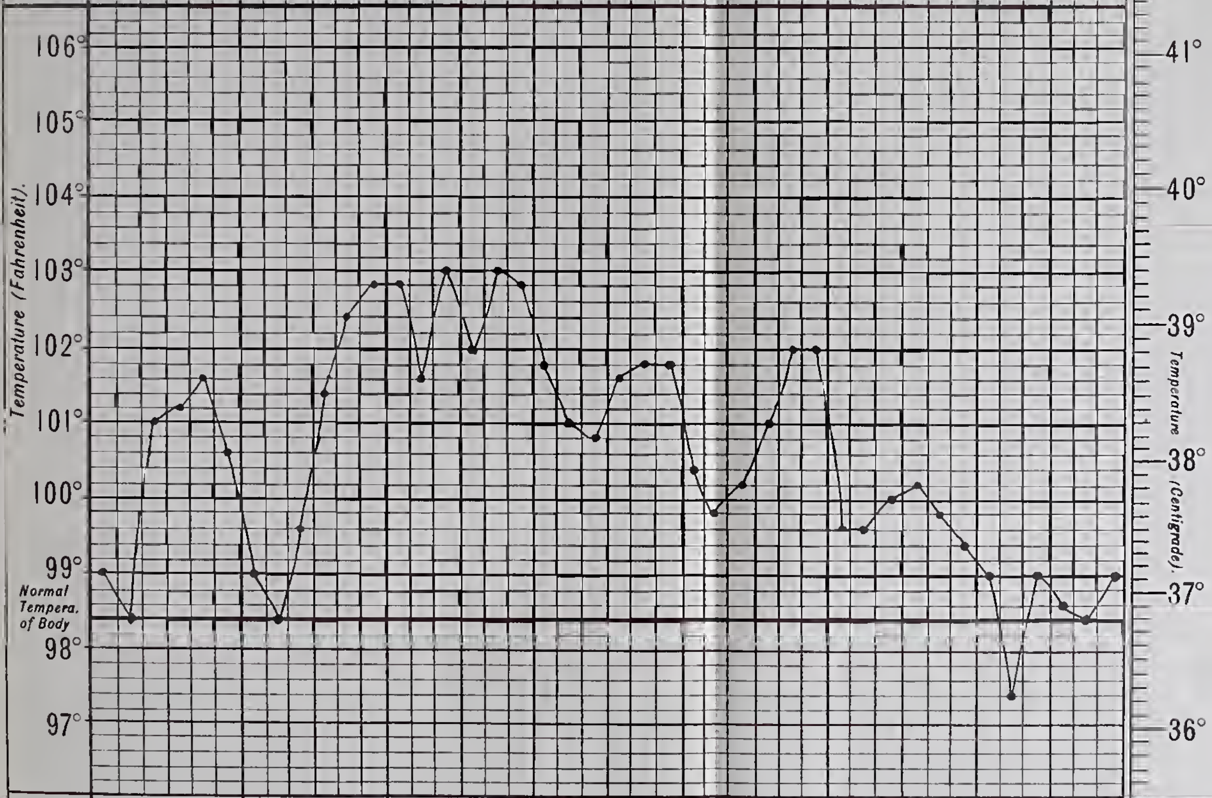
Time: A.M. 2, 6, 10; P.M. 2, 6, 10

Bowels

Urine

Skin

Tongue



Day of Dis.

Pulse

Date	A.M. 2	A.M. 6	A.M. 10	P.M. 2	P.M. 6	P.M. 10
March 14th	88	102	104	100	104	100
15th	88	88	108	108	116	108
16th	108	96	120	120	120	106
17th	106	100	84	100	100	96
18th	88	80	88	100	100	112
19th	108	88	92	96	98	92
20th	92	80	92	98	100	86

Resp.

Date	A.M. 2	A.M. 6	A.M. 10	P.M. 2	P.M. 6	P.M. 10
March 14th	20	24	24	24	24	24
15th	20	20	24	24	24	24
16th	20	20	24	24	24	24
17th	24	20	20	22	24	22
18th	22	20	22	24	24	24
19th	22	22	22	22	24	24
20th	24	20	20	22	24	20

Weight

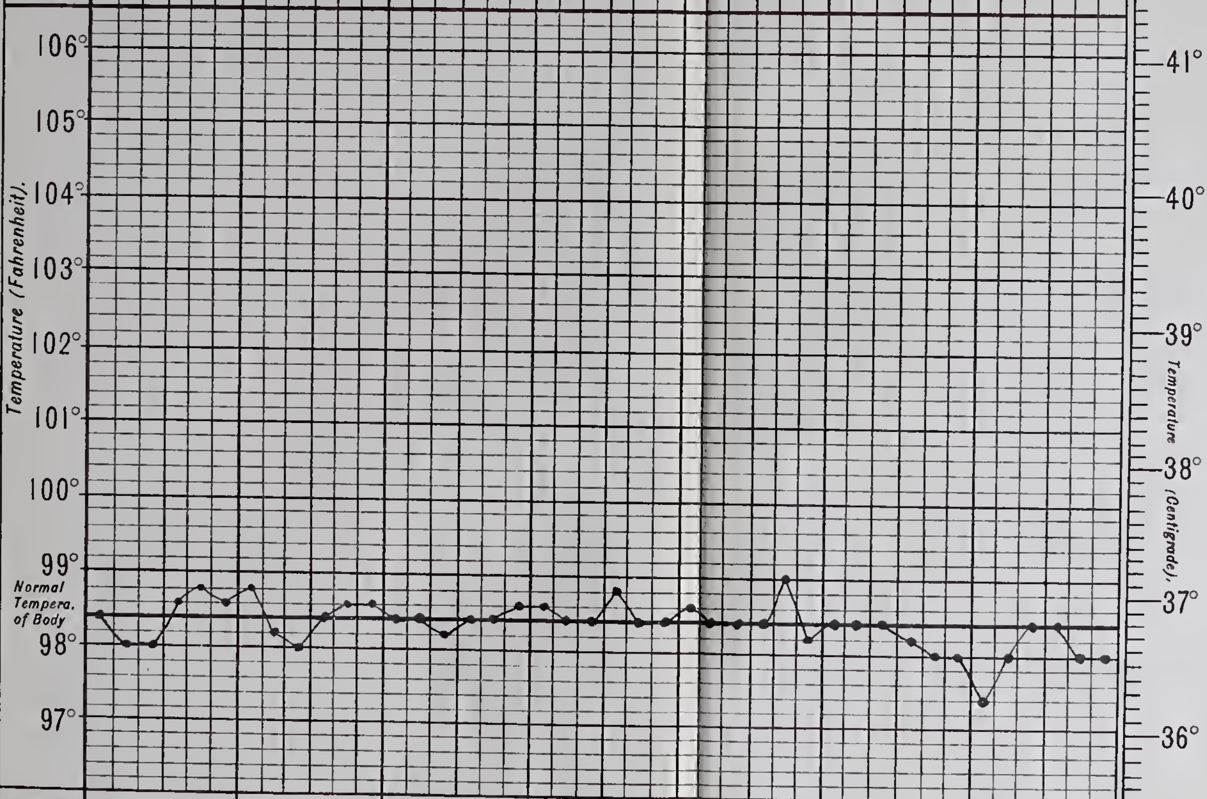
SURVEYOR GENERAL'S OFFICE ADELAIDE A Vaughan, Photo-lithographer

Four-hour Chart

Name........................ Reg. No. ....

	March 21st						22nd						23rd					
	A.M.			P.M.			A.M.			P.M.			A.M.			P.M.		
Time	2	6	10	2	6	10	2	6	10	2	6	10	2	6	10	2	6	10
Bowels																		
Urine																		
Skin																		
Tongue																		

	24th						25th						26th						27th					
	A.M.			P.M.			A.M.			P.M.			A.M.			P.M.			A.M.			P.M.		
Time	2	6	10	2	6	10	2	6	10	2	6	10	2	6	10	2	6	10	2	6	10	2	6	10
Bowels																								
Urine																								
Skin																								
Tongue																								



	March 21st						22nd						23rd					
Day of Dis.																		
Pulse	88	92	98	104	104	88	80	92	100	92	100	88	80	66	96	100	88	96
Resp.	20	20	20	22	22	22	22	22	20	20	24	20	28	20	22	22	20	20
Weight																		

	24th						25th						26th						27th					
Day of Dis.																								
Pulse	84	76	84	88	100	100	100	96	104	100	104	88	96	80	96	98	112	108	80	80	98	96	100	96
Resp.	20	20	20	20	20	20	20	20	20	20	22	20	22	20	20	20	20	20	20	20	20	20	20	20
Weight																								

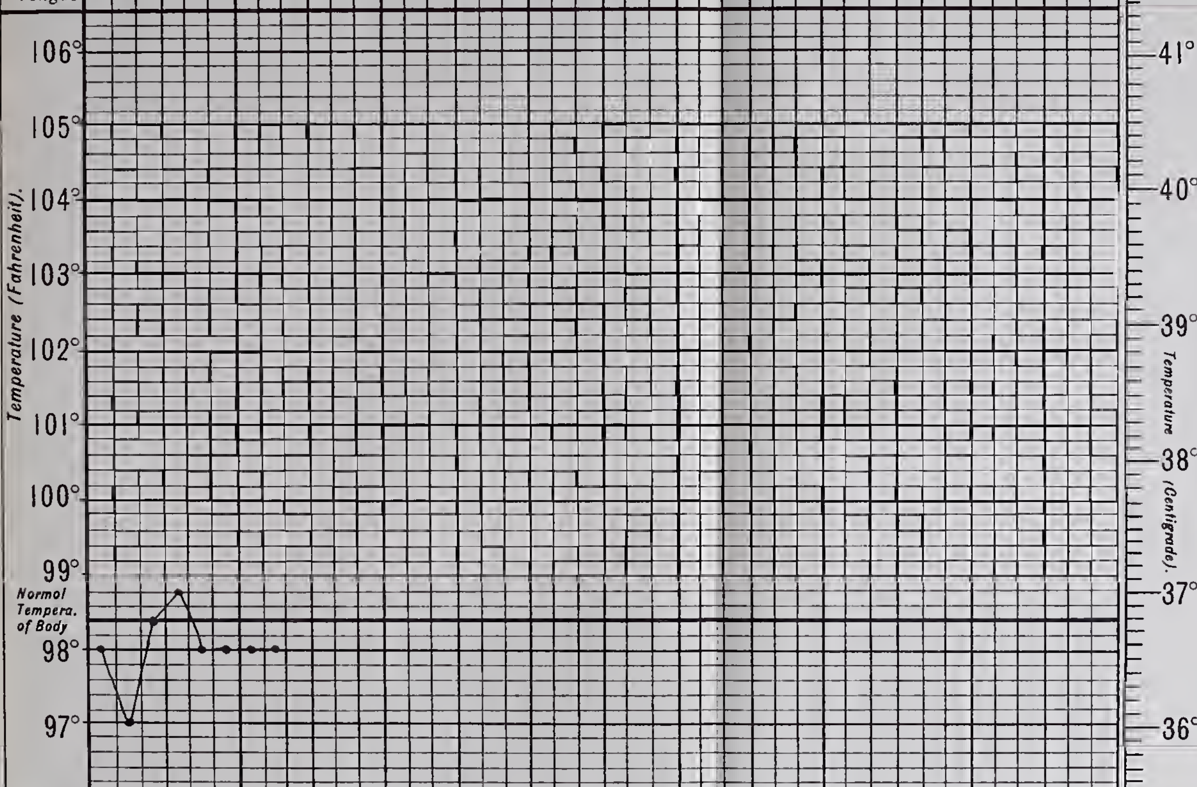
SURVEYOR GENERALS OFFICE ADELAIDE A Vaughan, Photo-lithographer

Four-hour Chart

Name .... Reg. No. ....

Date March 28th						29th																																																																																	

	A.M.			P.M.			A.M.			P.M.			A.M.			P.M.			A.M.			P.M.			A.M.			P.M.			A.M.			P.M.			A.M.			P.M.		
Time	2	6	10	2	6	10	2	6	10	2	6	10	2	6	10	2	6	10	2	6	10	2	6	10	2	6	10	2	6	10	2	6	10	2	6	10	2	6	10	2	6	10
Bowels																																										
Urine																																										
Skin																																										
Tongue																																										



Day of Dis.							
Pulse	88 82	98 100	96 84	84 80			
Resp.	20 20	20 20	20 20	20 20			
Weight							

SURVEYOR GENERAL'S OFFICE, ADELAIDE A Vaughan, Photo-lithographer

of Lembert sutures, and dropped it back. The upper end was attached to the skin to form an artificial anus. I recongised that I was doing so in the most unfavorable position ; but one had no choice, as it was urgently necessary to cut short the operation. I intended subsequently to perform an anastomosis between the large and small intestine ; but the patient left the hospital on account of some disagreement with the nurses."

On examination at the Royal Prince Alfred Hospital, there was a fœcal fistula in the middle line between the umbilicus and the pubes. The liquid contents of the small intestine were constantly escaping, causing a horrible eczema, which extended to her knees. The cervix uteri was transversely torn. On syringing fluid into the rectum, none returned by the vagina. In spite of all this, the patient was fairly well nourished, and appears to have been a fine handsome woman. She was kept lying on her left side, so as to get the right side of the abdomen free of eczema, and, on May 20th, under chloroform, an incision was made over the cæcum, and on opening the abdomen the large intestine was seen to be pale and shrunk, the lower segment of small intestine was short and stretched from the cæcum to the uterus. On passing a bougie into the fistula, the upper segment also was seen to dip down into the pelvis, and therefore could not be readily brought into apposition to the large intestine One had, therefore, to seek for a freer loop higher up, which was apposed to the front of the cæcum at its junction with the ascending colon, and an anastomosis completed by three tiers of suture, in the usual way. I was anxious about the atrophied and contracted condition of the large intestine —that it might not functionate readily ; but I was soon relieved of my anxiety, for flatus passed per rectum within twenty-four hours, and fæces soon followed. The patient left the hospital, wearing a truss over the fistulous opening, in comparative comfort ; but subsequently she came back and had the opening closed. She now menstruates regularly, and is apparently in good health.

Case II.—Mrs. B., aged 56, was operated on at " The Terraces " Private Hospital, September 20th, 1903, for a uterine fibroid and double ovarian cyst, and the uterus and ovaries were removed. On account of extensive adhesions, the whole of the raw surfaces could not be covered effectively with peritoneum. A vaginal gauze drain was inserted. Convalesence went on uninterruptedly until October 9th, when the patient complained of abdominal pain and sickness after walking back from her bath. This was followed by free vomiting, by constipation, and increase in pulse rate. On October 11th, all the symptoms of intestinal obstruction became intensified ; and, as the usual medicinal remedies failed to relieve the obstruction, and the pulse was getting weaker, I determined to open the abdomen without delay. Under chloroform, the abdomen was opened through the old cicatrix. Coils of intestine were found adhering to the front of the rectum, and as each coil was separated bleeding was free, and had to be stopped by the application of hot saline swabs. Recognising that the patient could not stand too much handling, and that there was no actual strangulation of the intestine, I determined to effect a lateral anastomosis. I traced the small intestine down from the commencement of the jejunum, and found that, after passing about 6 feet through my fingers, it got engaged in the adhesions in the pelvis. I then lifted the lowest loose loop across, and effected a junction between it and the ascending colon—there was only about 5 feet of small intestine above the seat of anastomosis. The abdomen was then closed. The relief was immediate, and the patient was discharged from hospital on November 2nd, and has since remained well.

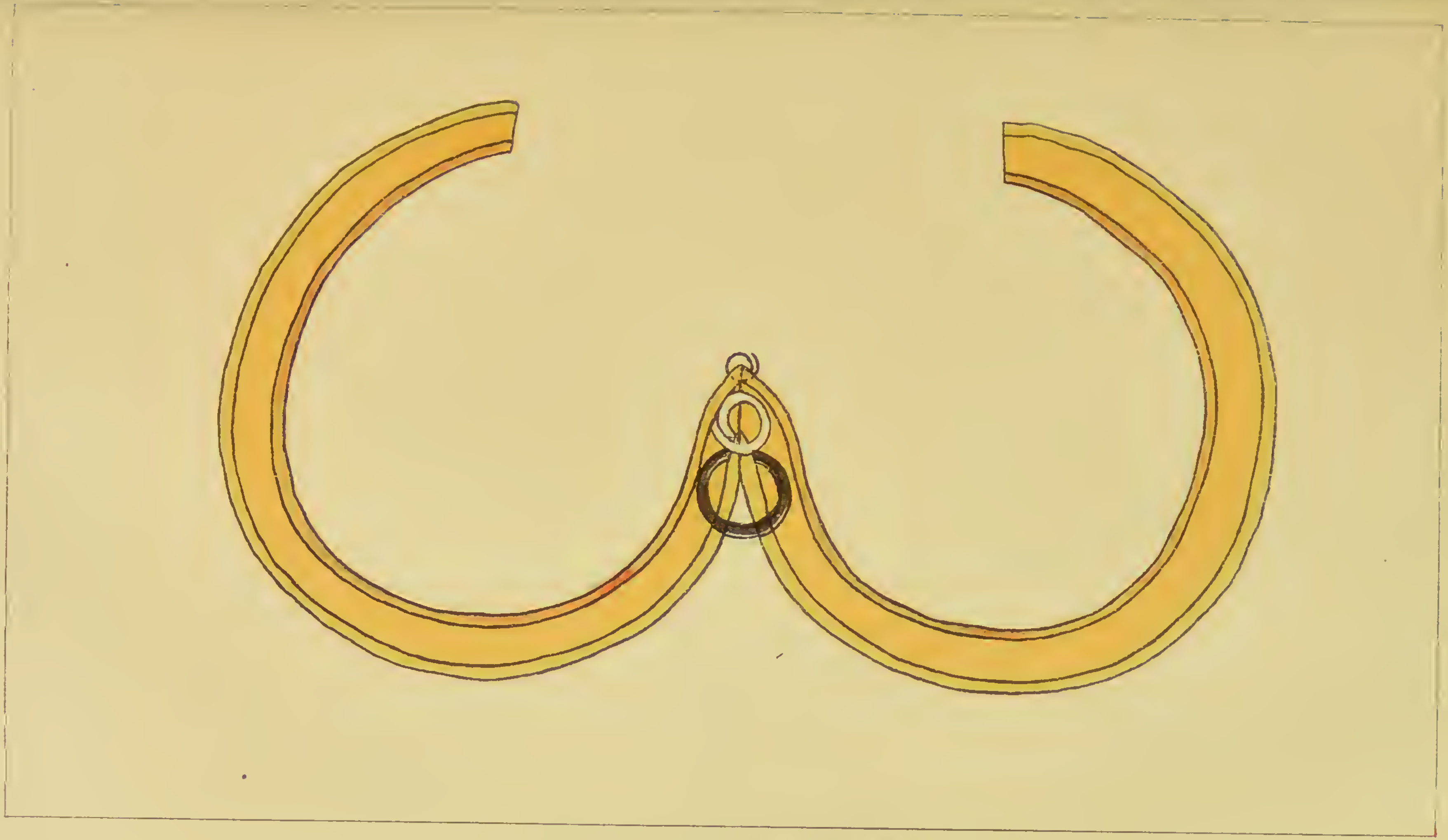
Case III.—H. T., age 21, was admitted into the Royal Prince Alfred Hospital, February 1st, 1905, suffering from an appendiceal abscess. On the same date the usual " gridiron " operation was performed. There was 1 ounce of foul pus, with a perforation and a fæcal concretion, found. The appendix

was removed; the septic cavity washed, and packed with gauze. February 6th—Pulse 86, temperature normal, but patient complaining of pain in the left side of the abdomen, high up—no vomiting. February 7th—Pain in the left side is worse, and seems to come in paroxysms, pulse 104, temperature 99°, bowels slightly opened. February 9th—The pain in left side getting worse; general condition not so good; enemata failed to open bowels; no flatus passed; vomited a little clear fluid. Abdomen became greatly distended during the night, and pain all in the left side below costal margin, and going round to the back. As there was evidently intestinal obstruction, and the patient would soon be *in extremis* if not relieved, immediate operation was determined upon. Under ether, the abdomen was opened. The large intestine was empty, but nearly the whole of the small intestine was enormously distended and dark-colored. There were some adhesions in the cæcal region, which were easily broken down, and were evidently the cause of the obstruction. The upper half of the distended small intestine was emptied, by bringing forward a distended coil and inserting a Paul's tube, and, by manipulation, emptying its contents through the tube into a basin. The opening made for the tube was then closed, the intestine washed with salt solution and returned into the abdomen. The lower half of the small intestine was similarly dealt with—the Paul's tube being inserted into an area which was denuded of peritoneum from the latter having given way. When the whole of the small intestine was emptied, enterostomy was performed, and the remainder of the abdominal wound closed. The patient made a steady convalescence; but, as usual in these cases, suffered horribly from eczema of the skin of the abdomen as soon as the Paul's tube came away. On several occasions he had severe attacks of pain in the lower abdomen, probably due to a band of adhesion. He was kept lying on the left side for some weeks, so as to get the skin of the abdomen on the right side into a fit state for operation for lateral anastomosis between the small intestine, above the fistula, and the large. This was carried out under ether on May 11th, the anastomosis being made between the commencement of the transverse colon and the small intestine, 9 inches above the fistula. May 25th—The last abdominal wound is now healed, and patient has been passing flatus and fæces per rectum. Under ether the fistula was closed, and in twenty-six more days the patient left the hospital. July 24th—He has put on condition, and has no abdominal trouble.

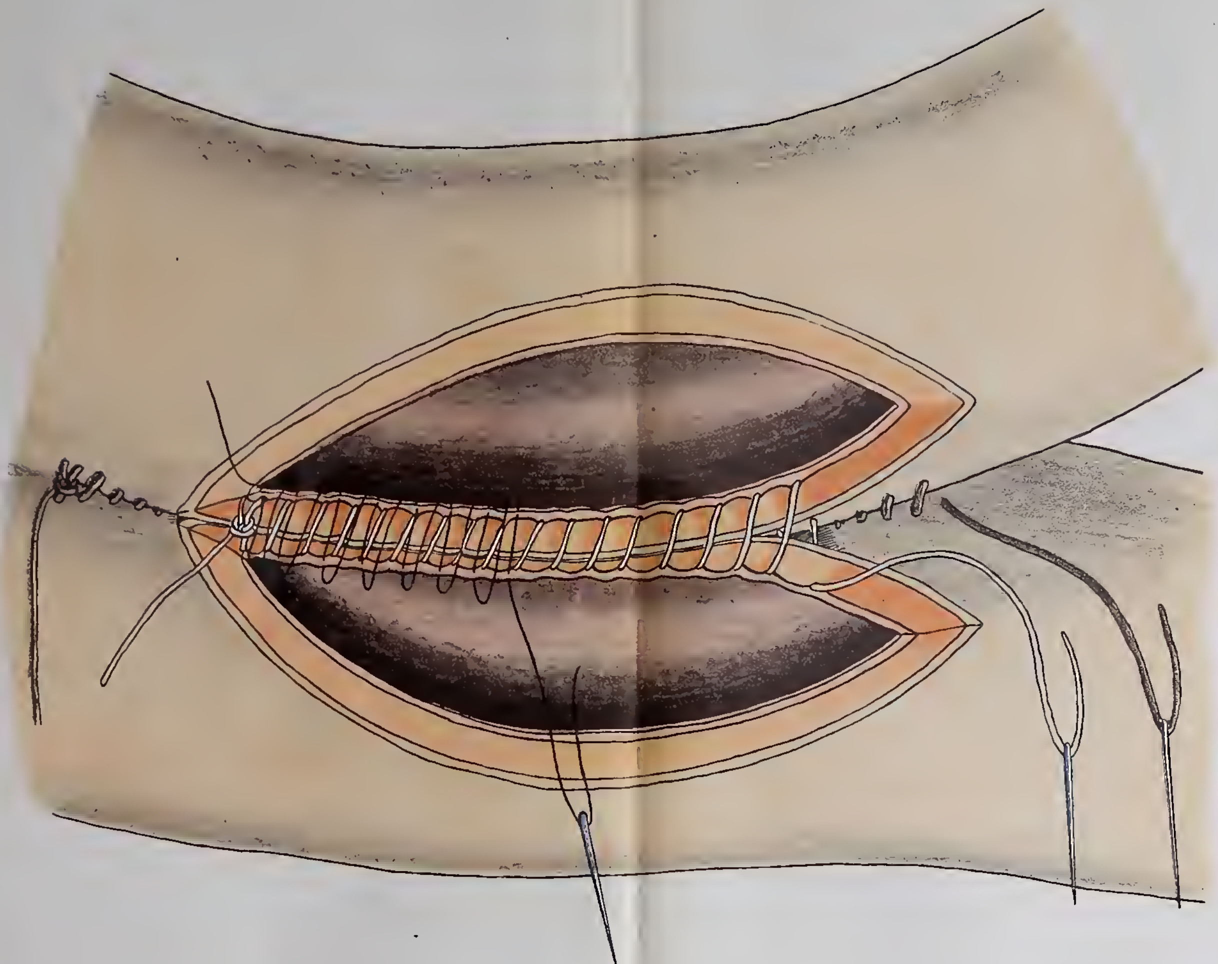
The first case illustrates well the power of endurance of a woman during the puerperal period, and Dr. Brook Moore is to be congratulated upon the success of his treatment at the time of the accident. Case No. 2 shows the great advantage of short-circuiting in cases where there are extensive adhesions which one cannot be satisfied of completely separating, so as to get rid of all chances of obstruction. When the bowel is healthy, and the patient in good condition, this method has many advantages over an enterostomy, for which it should be generally substituted. In this case it is possible that the intestine beyond functionated partly. In case 3 the patient was so collapsed that the operation had to be got over quickly. In this case the emptying of the intestine of its contents, so avoiding fæcal poisoning, probably saved the patient's life.

In cases of fæcal fistula with eczema, and a condition of skin around the fistula which will not allow of its being rendered even moderately clean, a short-circuiting operation is very useful; and there is not the same necessity afterwards for trying to ensure a patency of the intestine when closing the fistula. In the first case the blind end of the upper segment of small intestine was closed, and no inconvenience followed.

The method of anastomosis which I make use of is the same as I have been in the habit of using in gastro-enterostomy, viz., three tiers of continuous



SURVEYOR GENERAL'S OFFICE ADELAIDE A Vaughan, Photo-lithographer



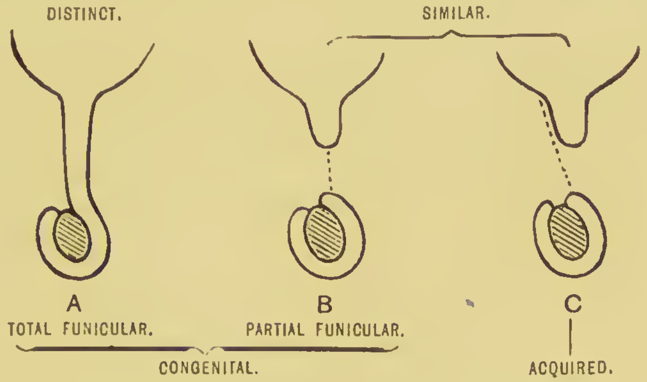
SURVEYOR GENERAL'S OFFICE, ADELAIDE. A Vaughan, Photo-lithographer

suture—(1) sero-muscular, (2) sero-muscular cut edge, (3) mucous membrane only. The first and second are composed of silk, and the third of catgut. Reference to the diagram will illustrate this method. In case of great hurry, two tiers of continuous suture might be sufficient, but the introduction of a third tier will not take more than five minutes, and it will give much greater security.

THE ETIOLOGY AND TREATMENT OF HERNIA IN ADULTS.

By R. Hamilton Russell, F.R.C.S.

These three diagrams, A, B, and C, represent the three main groups into which oblique inguinal hernia is usually classified. The first two owe their origin to the processus vaginalis. In A there has been complete failure of the obliterative process throughout, so that, should a hernia descend into that, it will pass into the tunica vaginalis; this we call a total funicular hernia. In B there has been incomplete obliteration of the processus vaginalis, so that when a hernia descends it is debarred from passing into the tunica vaginalis;



The dotted line from B and C indicate the obliterated portion of the funicular process.

Fig. 1.

this we call a partial funicular hernia. The obliterated portion of the processus vaginalis may be long, leaving a small hernial sac ready prepared and a normal tunica vaginalis; or it may be short. It may be high, leaving a small sac and a large tunica vaginalis; or it may be low, leaving a large sac and a small tunica vaginalis. It is very often sacculated, and a secondary sac may be placed subpertioneally, when the resulting hernia is called properitoneal; or it may be inserted between the abdominal muscles, giving rise to interstitial hernia; or it may be placed between the skin and the abdominal wall, in which case the resulting hernia has not been dignified by any particular name, but we have been content merely to wonder why it should behave in so unusual a manner. These latter varieties are frequently associated with imperfect descent of the testis, and the ingenious view has been advanced that the retained testis is to be regarded as the cause of these hernial divagations. It would be much nearer the mark to call it the result. The truth is that the retained testis and the laterally expanding hernia are common results of a developmental accident of frequent occurrence. These variations in the saccular arrangement of the funicular process that I have named are only samples; the actual varieties that come under our observation from time to time are almost unlimited in number. The last of our three varieties (C) is called *acquired* hernia; this is quite independent of the funicular process,

and is caused by weakness of the abdominal wall, which allows the hernia to escape through the inguinal canal, pushing the peritoneum in front of it to form the sac. Thus C is, in origin, quite distinct from A and B; it is produced by a different cause, and will call for a different remedy. Strangely enough, however, as I have pointed out once before, while A and B, which are identical in origin and nature, are easily distinguished clinically, B and C, which are quite diverse in origin and nature, are yet virtually indistinguishable from one another; so that the surgeon, every time he meets with a case of hernia in which the sac is distinct from the tunica vaginalis, is quite unable to tell whether he should be guided by the assumption that its cause is a congenital sac (B) or a weak abdominal wall (C). It is therefore not surprising that a large number of devices should have been instituted for the cure of hernia, all of them designed to meet either contingency, somewhat in the manner that has been not altogether unknown of putting several drugs, each appropriate to some special indication, into a bottle, and labelling it "The Mixture." This, I need hardly say, is not a very good plan, nor is it likely to yield results that are altogether satisfactory. I would further point out that all the operations that have been devised have been, in the main, prompted by the supposed necessity of meeting the case of class C; and there is not the slightest doubt that the prevailing opinion has been that the predominating type of inguinal and of all other forms of hernia is the acquired type, and that the congenital forms, A and B, although quite well recognised, are merely of the nature of pathological accidents peculiar to the inguinal canal. Thus it has come about that all operative procedures have comprised two elements—first the removal of the sac; and, secondly, some device for strengthening the abdominal wall by stitching it up in various ingenious ways. Now, with respect to these two factors in the operation, I wish particularly to put to you this: The first step, the removal of the sac, has been regarded as a very simple matter, an obvious and necessary incident in the operation; whereas the matter of chief importance is the method of closing the canal so as to restore its valvular mechanism (whatever that may mean), and upon this project much thought and ingenuity has been expended. I wish to-day to review this matter of the "Radical Cure of Hernia," and perhaps I may succeed in arresting your attention with some considerations not altogether unworthy of your notice.

The only place in my opinion in which the etiology of hernia can be effectively studied in the first instance is a children's hospital. In an adult hospital it cannot be done; nor do the deadhouse and the dissecting-room help us very much in our primary investigations, though they will often be found to afford interesting and valuable confirmatory evidence. I shall not attempt on this occasion to re-state the arguments that I have already adduced in favor of the views as to the etiology of hernia that I believe to be correct. Suffice it for me to state simply that in children acquired hernia (class C) does not exist.

Six years have elapsed since, at the Brisbane Session of this Congress, I first pointed out that acquired hernia in childhood was a myth, and a myth that had proved highly prejudicial. The view was put forward that inguinal hernia in a child is invariably dependent upon the presence of a congenital sac, which, in the great majority of cases, is provided by some portion of the processus vaginalis. It was contended that if the sac be completely removed from the inguinal canal, there will be no return of the hernia; that, moreover, by this operative procedure alone can the hernia be cured; that the so-called curing of hernia by the use of a truss is a deceptive misnomer; and, as a practical corollary, that operative removal of the sac should be advised and carried out in every case of hernia in a child. It must be confessed that these views seemed rather sudden and shattering at the time; nevertheless,

I think that their correctness has, in these intervening years, become established in the opinion of so very many of those best qualified to judge that they will henceforth need no further advocacy from me. The views which six years ago seemed strange and provocative of a very natural feeling of incredulity are strange no longer. That which does appear to me strange when I now look back at the occasion to which I have alluded is that the above views should have appeared, at that time, only appropriate to one form of inguinal hernia—the oblique inguinal of childhood; for the oblique variety of inguinal hernia was deliberately contrasted with all other forms, such as the direct inguinal, the femoral, obturator, and other varieties, all of which I, at that time, supposed to be of the acquired type. Subsequent opportunities for observation have, however, shown, as it seems to me, quite unmistakably that this view was erroneous; and in a series of papers that have been published during the last few years—one of which papers was read at our last session at Hobart—I have advanced reasons for believing that acquired hernia, other than traumatic hernia, has no existence, and that the presence of some sort of congential peritoneal pouch is an essential condition for its occurrence. The correctness of this view, in respect at any rate of the hernia of childhood, is, in my opinion, no longer open to question. By this I by no means imply that it is universally accepted; I am, however, absolutely confident that it has already acquired so strong and increasing a hold on influential surgical opinion that it may be left now to take care of itself. I, accordingly, do not intend to deal other than incidentally to-day with the hernia of childhood; my subject is the hernia of adult life, its etiology and operative treatment. As you will naturally expect, however, I shall approach the subject from the standpoint afforded by the information we have only recently acquired as to the etiology of hernia in childhood.

The first great question that arises, and that I shall endeavor to answer, is this—What etiological difference, if any, is there between the hernia of childhood and that of adult life? Or, to put the question in another way—Is the hernia of adult life always, as in the case of children, dependent on incomplete closure of the funicular process, or some other form of pre-existing sac; or do we meet with genuine acquired hernia in adults? How does adult hernia answer in the tests that have sufficed to satisfy us as to the congenital nature of hernia in childhood? The evidences of the congenital origin of the herniæ of young children are redundant and overwhelming; but there is one test that far outweighs all others, and that would be absolutely incontrovertible even if all the other evidences were withdrawn. This is the evidence afforded by the operation of simple removal of the sac, as proving effective in curing the hernia. I repeat that, if there were not another particle of evidence forthcoming as to the congenital origin of hernia in childhood instead of the abundance of it that we have actually found, the evidence afforded by the results of operative removal of the sac would be all-sufficient, and could not be successfully opposed by any line of reasoning that I can conceive. Can we, however, employ this test in the case of adults? It is obvious that a grave complication is introduced in many cases of hernia in adults where the hernia has existed for a long time, through the stretching and permanent weakening of the hernial opening. It is scarcely necessary for me to remark that an acquired hernia will develop at any situation in the abdominal wall, provided the musculature be sufficiently damaged and weakened at that spot. We should, therefore, expect, *a priori*, that if we were to remove the sac in a large number of cases of adult hernia, indiscriminately collected, guided by the same principles that have served us so well in the case of children, we should, in our adult operations, have a very large percentage of recurrences.

It is therefore clear that, should any surgeon treat a large number of adults by simple removal of the sac, on the assumption that the etiology of adult hernia is identical with that of childhood, he will, in his adult work, be carrying a very heavy handicap, which he escapes in his work among children. On the other hand, should he succeed, even in spite of this handicap, then its imposition will have only availed to enhance the value of the evidence, and to render the proof of the correctness of his principles the more convincing. Before giving you, in very simple form, the statistics of my operative experience, we must first come to some agreement as to what we are going to accept as evidence of success. After what lapse of time may we conclude that all fear of recurrence is past, and write a case down as a successful operation.

I am in entire agreement with Deanesley, and I think most surgons are, in respect of this matter. Recurrence after operation, when it occurs at all, does so, in the vast majority of cases, within six or eight months from the date of operation. I have never met with a case of recurrence supervening after a longer period had elapsed, and I am sure that such an occurrence must be quite rare. I therefore propose that we should take a year's immunity from recurrence as our standard of success, always recognising that, even although possibly not an infallible standard, it will, for all practical purposes, satisfy our demand for a fair working estimate of the value of any particular method of operating. I therefore include in my statistics the cases of adults who were operated upon more than one year ago. Of eighty-nine such cases, I have lost sight of twenty-six, whom I have failed to trace. The others—sixty-three in number—I have recently seen or heard of, or otherwise known about. Of these sixty-three, recurrence has taken place in three. One was a woman with an ordinary inguinal hernia, which recurred almost immediately she got up after the operation. Evidently I had blundered badly in some way. The second was a man with a very unusual kind of hernia, which I wished to examine with much deliberation. Unfortunately, he took the anæsthetic badly, and I was unable to satisfy myself that the operation had been efficiently done, and I predicted that recurrence would take place; this happened in about five months. The third and last case was specially interesting. A man, *æt.* 62, of poor physique, in whom a left inguinal hernia appeared for the first time, and became at once strangulated. He was promptly operated on, and the sac removed. Some five months later he came back to me with a commencing recurrence. I remarked at the time that this case appeared to be really one of genuinely acquired hernia. The hernia had appeared at the age of 62 for the first time; it had been at once operated upon, and the sac removed; the abdominal wall had suffered no damage from the prolonged existence of the hernia, and yet the hernia began to recur a few months later. He was a flabby, pallid individual, and I could come to no other conclusion for the moment than that we had to deal with a case of real acquired hernia, the only instance that I had ever seen reason to regard as such. Nevertheless, I was wrong, for on referring to the notes of the operation I found that my careful house-surgeon, Dr. MacKeddie, had made a special note to the effect that the case was one of total funicular hernia, *i.e.*, hernia into a totally patent funicular process. The explanation of the recurrence is, doubtless, that, under the circumstances surrounding the operation, I was less careful about getting to the upper limit of the funicular process than I ought to have been. In addition to these three cases, I must mention that two other of my earlier cases had a transient recurrence, which was cured by the use of a truss for three months. Both of these cases occurred more than five years ago, when I had but few opportunities of operating on adults, and when my methods were not so thorough as they are now. On one of these two cases I operated

a second time, and found only a portion of the bladder in the canal. Nothing was done, and the hernia never came down afterwards; and, after wearing the truss for three months, it was discarded, and the patient has remained perfectly well for the last five years. Those are, in brief, my statistics of operation in adults. In all cases the operation has consisted solely in thorough removal of the sac; in no case has the canal been closed by sutures of any kind, except in one or two of the earliest ones. The list includes subjects of all ages, and herniæ of various duration. The oldest was a man of 75, with a large irreducible hernia of thirty years' standing. One fatality must be chronicled. An old man, with a very large double irreducible hernia, who gave his age as 74, but it subsequently transpired that he was probably over 80. It will be noted that, while the principle of operation adopted in all these cases of adult hernia has been identical with that which we have followed in the case of children, the results have been, to all intents and purposes, identical, in spite of the heavy handicap to which I have alluded. I would further state, in general terms, that the various signs of congenital origin which I have been accustomed to note in the case of the herniæ of children have been noted with equal frequency and consistency in the case of adults. Nevertheless, it must be admitted that the stretching and alteration in the structures that is displayed in herniæ of long standing will conceal these evidences unless they are very carefully looked for by a practised eye. It must also be remarked that we meet with occasional borderland cases, which begin with a very small sac, in which the clinical history and the appearances observed are, perhaps, indistinguishable from those that would be presented by a genuinely acquired hernia. It has been a matter of surprise to me that such borderland cases are not more common than they have proved to be in my experience. If now the question to be put as to whether the hernia of adults is identical in mode of origin with that of childhood, or whether, on the contrary, adult hernia is sometimes acquired, I think I shall be held justified, and shall be assuming a sufficiently guarded attitude, if, in the light of the facts I have detailed, I answer the question in the following manner:—

I have never met with a case on the operation table that I have seen any reason to regard as being a case of genuine acquired hernia; all my experience in the surgery of adult hernia has been such as to force upon me the conclusion that there is substantially no difference between the hernia of adult life and that of childhood. This being so, however, I quite recognise that the possible occurrence of rare exceptions is not excluded thereby, and I am by no means unmindful of the immense operative experience which, at the present day, has fallen to the lot of a few favored surgeons, contrasted with which my opportunities for observation have been relatively trivial. It may be that, in the case of elderly people who have put on much adipose tissue, hernia is occasionally acquired. From the practical standpoint, however, I would urge that such rare exceptions, assuming that they exist, may be disregarded, for the conditions of their occurrence would be such as to contra-indicate an operation of expediency, and any such operation would be sure to fail. We shall, on the other hand, not be misled if we take, as a working hypothesis for all forms of inguinal hernia, the view that it is dependent on the presence of a pre-existing sac. Assuming then, as I believe we may do with perfect safety, that the underlying cause of hernia in the adult is the same as that which is efficient in the child, the practical question arises—"Is the treatment also identical; and will the same method of operating suffice for the adult that is found to be efficacious in the case of young children?" My answer to this question is that the principle holds good, and that simple removal of the sac in adults of any age, and in hernia of any duration, is very nearly, if not quite, as efficient and satisfactory as it has proved to be in the case of

children. But it has to be noted that there are very important anatomical and clinical differences presented by adult hernia when contrasted with that seen in children; and these differences, while in no way affecting the principles that govern our operative treatment, demand important modifications in detail. We will therefore briefly consider these differences, and the methods that seem appropriate to their management. In the first place, by reason of the length and obliquity of the canal in adults, it is not possible to get at the peritoneum at or above the region of the internal ring without first slitting up the external oblique; and this should always be done in adults, although in children it is not usually necessary. This, then, is the first great difference. The second is dependent also in part on the length and obliquity of the canal, in conjunction with the results entailed on the structures forming it by the prolonged existence of hernia in the case of elderly people. These results are progressive stretching and dilatation, until the canal becomes converted into a large hole in the abdominal wall. The kind of thing referred to is represented in Fig. II. We all, I think, recognise the appearance as that presented when

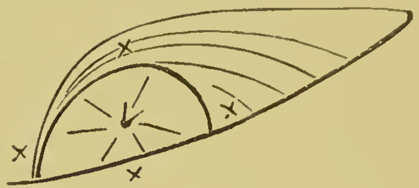


Fig II.

the sac has been tied off in the ordinary way prior to the completion of the operation by Bassini's or some other method of suturing. I used to think—in fact, I expressed the opinion at our last meeting of Congress—that the best way of operating in such a case as this is by Bassini's method, by which the muscles are brought down by sutures to cover the large exposed area of peritoneum. I have since then altered my mind; and I would point out now, what I must confess it took me a long time to discover, that the presence of an exposed area of peritoneum and fascia, like that represented here, is, *ipso facto,* proof that the sac has not been properly removed, and for this reason: The mouth of the sac is situated normally under cover of the muscles, and any portion of the peritoneum that is seen to emerge, and come to light below the muscular border, must be regarded, for operative purposes, as a portion of the sac. It will therefore follow that the whole of the area of exposed peritoneum in reality belongs to the sac, and should have been removed. On the other hand, if our ligature, instead of being applied in the way that is here represented, had been made to encircle the exposed peritoneum in such a way that, when tightened, the periphery should have been made to converge centripetally into the knot, it is obvious that the area of peritoneum would have disappeared almost entirely, and the opening in the abdominal wall would have been closed, or at least so nearly closed that we should no longer experience any pressing desire to close the opening by sutures. The actual technique employed in applying the ligature is a matter in which some choice is open to us. A purse-string suture will doubtless answer very well; or we may boldly cut away the entire area of peritoneum and suture, taking care, in any case, to include the fascia. The method I personally prefer is a special form of knot of my own devising. This knot has four fixation points, each point being indicated in Fig. II. by a cross. Each cross indicates a spot under cover of the muscles where the ligature fixes the peritoneum and fascia. When the ligature is tightened, each of these four points is brought convergently into the knot.

There is one other matter of technique to which I must allude, and that relates to the clearing of the sac preparatory to the application of the ligature. Where the hernia has been of long standing, the peritoneum of the inguinal region becomes lax and voluminous, and the redundancy should be included in the ligature and removed. This is done in the following manner :—The left index is passed through the mouth of the sac into the abdomen, and the abdominal wall lifted up on the finger and pushed back from the sac. Externally, it is pushed back while the first step in the fixation of the ligature (at the external fixation point) is effected ; above, the curved fibres are drawn well up, while the ligature is passed through the fascia and peritoneum at that spot. The most important part of this stage of the operation relates to the inner part of the mouth of the sac. Here the peritoneum is very lax ; it is lifted well up on the finger, and the epigastric vessels and subperitoneal fat are pushed inwards. What appears to be a secondary sac will now make its appearance on the finger. This is really the peritoneum of the internal inguinal fossæ. Frequently the bladder will appear, and some of the peritoneum can be readily stripped off that viscus. The whole of the peritoneum thus isolated may be now included in the ligature and removed, and the operation concluded by suture of the incision in the external oblique. It will be obvious that this method of operating relies for its efficiency, first, on the removal of all lax peritoneum from the inguinal region ; and, secondly, upon closure of the opening in the abdominal wall by concentric traction upon the peritoneum and fascia, as distinguished from the usual method of drawing down the muscles by suturing them to Poupart's ligament. I venture to think that no one present will have the least hesitation in arriving at a decided conclusion as to which is the more correct principle of the two. But it will doubtless be asked, and with a great show of reason—Why not close both the internal and external layers ? Why not add the suturing of the muscles to the tying off of the peritoneum in the way I have advised ? I am far from saying that there is any particular harm in putting one or two sutures into the walls of the canal in the way that has been customary ; nevertheless, I think that if surgeons conclude that the principles I have enunciated are sound principles, and that the practice I have described as based upon those principles is to be trusted, there will be but little inclination to suture the walls of the canal. Personally, I do not put any stitches into the muscles, because I am convinced that they are unnecessary, and that the principles of treatment point altogether elsewhere for the means of preventing recurrence. And if sutures are unnecessary, their employment cannot be justified on the ground of their being harmless.

There are several reasons, none of them being perhaps of great individual weight, but collectively forming a sufficiently strong indictment against the use of sutures in the muscular wall to at least give us pause. Here are two of them—

(1) In the event of suppuration occurring (and such an accident will occur in the practice of every surgeon occasionally), I believe the sutures will do great damage to the musculature, and will greatly tend to promote recurrence ; whereas if sutures are not used, the accident of suppuration will in no way impair the result; some surgeons are inclined to think it might even be improved thereby.

(2) In the event of recurrence taking place after any of the suturing operations commonly employed, any subsequent operation will be greatly hampered, and the chances of success greatly diminished by the altered arrangement of the parts brought about by the suturing ; whereas, on the contrary, where the procedure adopted has been that which I advocate, the conditions for a second operation in the event of recurrence will be in no way affected prejudicially.

It is obviously impossible for me, in the time at my disposal, to attempt to discuss in detail the management of the numerous varieties of inguinal hernia that we meet with. There are, however, one or two features of the subject to which I should wish to allude. When we say that the hernial sac is congenital, or to use the term I prefer, " of congenital origin," this does not imply that the whole of the sac, as it appears at the operation, was present at birth. A small sac is always susceptible of being rapidly enlarged, up to a certain point, by virtue of the looseness of the peritoneum in the inguinal region. The hernial sac will therefore be composed of the original congenital pouch, plus an added portion. I have illustrated this point by these two diagrams (Fig. III., A and B). This " added portion " is of great importance

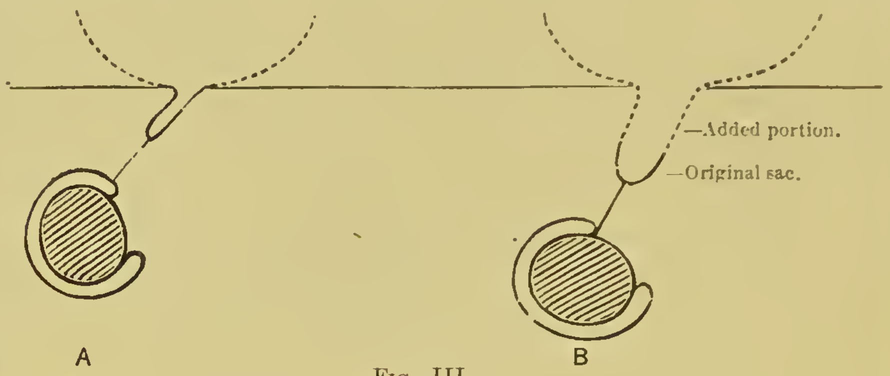


Fig. III.

from two aspects—first, it incidentally provides some remarkable and very important varieties of inguinal hernia ; and, secondly, it plays a prominent part in facilitating the manipulations of the operator engaged in removing the sac.

Let us consider this latter aspect of it first. When the sac has been exposed, and the separating and isolating process is being carried out, we are enabled to increase the length of the sac very materially by pulling down some of the peritoneum which, prior to our proceedings, was in the abdomen. If now we apply the ligature to this " added portion " and cut off the sac, the ligature will spring back into the abdomen well above the muscles of the abdominal wall. We, shall in fact, have applied the ligature intra-abdominally. Now, that which we are enabled to do, by pulling on the sac while we are operating, may also be effected by the pressure of a hernia so soon as it has completely filled the sac. So that if the sac be small in the first instance, a process of pulling down the abdominal peritoneum so as to enlarge it will be rapidly accomplished, and the sac will be rendered much larger than it was originally. This power of adding to the original sac, by inducing the descent of a portion of the abdominal peritoneum proper, is not only of extreme utility to the operator, but, when it occurs as the result of hernia, it may be, and frequently is, fraught with exceedingly important and interesting results. It must be remembered that the peritoneum forming the funicular process is, virtually, a portion of the same structure that lines the pelvis, and has intimate relations with the pelvic organs ; and it is in complete accord with all our experience of developmental accidents that, in the formation of the funicular process, it should sometimes happen that contiguous pelvic and abdominal organs should be implicated in its formation, and included with it in the inguinal canal. In this way a portion of the bladder may enter the canal on either side ; while on the right side may be found the cæcum, and on the left side a portion of the colon and sigmoid flexure. In the female, the ovary and Fallopian tube, and even the uterus, may be drawn in. Any of

these organs may be actually present in the canal at birth ; or, short of that, they may be congenitally disposed in such close proximity to the mouth of the sac that, should hernia occur, they will be at once drawn down into the canal with the " added portion " of peritoneum. When this occurs the particular viscus implicated—cæcum or bladder or colon, as the case may be—may make its initial entry into the canal at any period of life. It is obvious that when this condition is brought about, the operation will be complicated, for the application of the ligature to the neck of the sac will be interfered with. There are, as usual, two ways of meeting the difficulty, a right and a wrong way. The method which, I think, is frequently employed is to ligature the sac as high as pos ible (*i.e.*, below the point of fusion of the intruding viscus with the sac), then to close the canal by suturing according to the surgeon's fancy. This is the wrong way, and is pretty certain to be followed by recurrence. The right way is to cut out from the sac the portion of, say, the large bowel that interferes with the application of the ligature, return it into the abdomen with its attached portion of the sac, and then ablate the remainder of the sac by ligaturing it at the highest accessible point. If the sac be treated in this way, the chance of recurrence will be exceedingly small. I have adopted this expedient in five cases, of ages ranging from 5 months to 68 years. In no case has recurrence taken place, although all, with the exception of the youngest, were operated upon more than three years ago. Where the bladder is the viscus in the hernia, the manœuvre I have just described is unnecessary, as the peritoneum can be readily separated from it.

Another class of complication that may occasion perplexity is caused by variations in the anatomical arrangement of the sac—varieties of sacculation. These are most commonly, but by no means exclusively, seen in association with imperfect descent of the testis.

I have already referred to the most notable varieties of hernia that arise in this way, the interstitial and properitoneal forms, chiefly remarkable for their having been the joint heroes of the most romantic etiological stories that even the voluminous literature of hernia is able to show. Another not uncommon form of sacculation is that represented in Fig. IV., where a pouch

FIG. IV.

appears high up on the inner aspect of the funicular process. This will closely simulate a direct hernia if the bowel should descend into it. I have twice seen hernia occur in manhood into such a sac while the funcular process was open, but empty. Again, doubtless the funicular process might be closed, leaving the pouch open, giving rise to an anomalous form of hernia, apparently non-funicular in origin. I have never met with such a case, however. The varieties of pattern displayed in the sacculation of the processus vaginalis are unlimited ; they will, however, but rarely cause any great perplexity if the surgeon is imbued beforehand with right views as to their origin and

nature. If the funicular process is ligatured in the proper place and removed, all the sacculations, of whatever particular arrangement, will be removed also.

Time has permitted me only to indicate here and there a feature of this very wide subject, while many others I cannot even allude to. I should have liked to allude to some of the better known and more popular methods of operating, such as Bassini's and Kocher's. On the whole, excellent as is Kocher's method of treating the sac, I am inclined to think that the method of removal that I have described satisfies the requirements that, to me personally, appear most important more completely than any other plan. I do not wish to appear intolerant about the practice of suturing the walls of the canal; it cannot, as a matter of experience be so harmful, because such excellent results are obtained by (or in spite of) it. There are, however, certain theoretical considerations which I can find no sufficient reason for disregarding. One great object that we wish to attain is to strengthen the musculature enclosing the inguinal canal. It has even been suggested that hernia should be treated by local massage and exercises with this aim in view, a suggestion which is absurd, because it ignores the predominant part played by the sac in the causation of hernia. The strengthening of the musculature, however, being an obviously desirable end upon which we are all agreed, I fail to see how the stitching of the muscles to a tendinous structure is likely to strengthen them. The principle is, in my humble opinion, entirely fallacious and wrong, although I must own to a great disinclination to express myself thus about a proceeding which is systematically followed by a number of surgeons for whose individual ability and judgment I have far more respect than I have for my own. If, however, the principle is wrong, can the practice be accounted at least harmless? If it can, then it furnishes the only instance known to me, either in the domain of surgery or in any other of our mundane affairs, in which the systematic following of a wrong principle yields results as good as the systematic following of a right one. Furthermore, I would urge, what I know from observation to be no mere theory, that the surgeon who holds what I may term the generally accepted views as to the causation and treatment of hernia will remove the sac in a very different way from the surgeon who is convinced that the sac is the cause, that everything depends on its efficient removal, and that the abdominal wall has nothing to do with the matter, and may be disregarded. I am therefore unmoved by the contention that removal of the sac is a feature in all operations for hernia; it is not sufficient that it should be a feature of the operation—it must be the operation. I am sometimes asked some such question as this—"Is it true that in operating for hernia you simply remove the sac?" Simply removing the sac sounds very easy; as a matter of fact, it is by far the most elaborate and difficult operation for hernia that has ever been devised, and all other methods are child's play in comparison with simple removal of the sac, if it be efficiently and thoroughly carried out.

Finally, I would observe that a correct view of the etiology of hernia is everything, and, unless we can attain to that, a secure pathological basis for our work will be lacking. An insecure pathological basis for our theory and practice will mean, in the future as in the past, a multiplicity of operations, all of them more or less successful, none completely trustworthy; it will mean a most pernicious acquiescence in failure, so that when our operation fails we excuse ourselves by saying that the patient has a congenitally weak abdominal wall, that it is impossible to strengthen it, and that it is an error in terms to speak of the "radical cure" of hernia.

Turning from the practice to the literature of hernia, we find that an insecure pathological basis has been productive of the most extraordinary looseness and inaccuracy. We have not even taken the trouble to adopt an

ordinarily rational nomenclature, as witness the custom of naming these two elementary varieties A and B, Congenital and Funicular; whereas they are, of course, both equally congenital and both equally funicular. Furthermore, an insecure pathological basis has conferred exemption from criticism, for a critic will rarely be found ready to find fault while conscious that his own pathological basis is insecure. Hence the custom has grown up among authors of writing things that would certainly never have seen the light but for this security from criticism of which I am speaking. On the other hand, a secure pathological basis will mean a single operation or, at any rate, an established and immutable principle for the guidance of the surgeon. It will mean a refusal to acquiesce in failure, so that when recurrence takes place after operation, instead of comforting ourselves with the patient's congenital muscular weakness, we shall put to ourselves the question—"What mistake have I made?" While, if we turn again to the literature of hernia, we shall find ourselves suddenly subjected to restrictions unknown heretofore, and we shall no longer enjoy the privilege of romancing—a privilege of which we have been accustomed to avail ourselves rather liberally. To aim at these results is, at least, a high and worthy aim, and I shall be abundantly gratified if the few facts and suggestions I have been permitted to offer should seem to you to contribute, however modestly, towards the right kind of progress in this important department of surgery

DISCUSSION ON HERNIA.

H. S. Maitland: I have listened with very great pleasure to Dr. Russell's views, more especially so as they coincide with my own. I regard obliteration of the sac as the main factor in the cure of hernia in children and the young adult when the obliquity of the canal persists. In the older man, with protuberant abdomen, where the conjoined tendon is atrophied, I do a Bassini's, but I only operate on these older people if for some reason they are unable to wear a truss.

Dr. Russell speaks of a hernia in which the bowel forms part of the sac, and suggests dividing the sac on either side, and returning the gut: but I would like to sound one word of warning against treating the sac so in a hernia par glissament or sliding hernia, because such a method might possibly cut off the blood supply to the bowel, with disastrous consequences. I have no doubt that this is sometimes the cause of those cases of fæcal fistula which are recorded after hernia operations.

Dr. Steer Bowker: I have been greatly interested in Dr. Hamilton Russell's paper, and will certainly on some future occasion try it, and will get him to show me how he places his puraeotomy, though I do not consider one year is nearly long enough to judge of the risk of recurrence.

For my own part I am well pleased with the results of the operations that I have been doing since I was assistant to Dr. McCormick some years ago, the first idea of which I got from him, and have added to and altered since, when I have assisted at the operation afterwards. It is exactly the operation described in Bryant's book, recently published, and called "Halstead's operation." Halstead's operation used to be after the manner of a Bassini, both of which operations I have always considered to be bad operations, and have always dropped the cord in and not displaced it outwards; for if there is a recurrence, it always takes place where the cord has been brought out in either a Bassini or a Halstead. The operation I do, and which is now claimed by Halstead, differs absolutely from the old Halstead or Bassini, and is made up from points which I considered good in various operations—even to the imbrication of the external oblique at the close of the operation.

APPENDICITIS.

By Critchley Hinder, M.D.

Appendicitis is an extremely well-worn subject; but, none the less, it would certainly be a very extraordinary thing if we were all of one mind in respect to many matters in connection with this common and dangerous malady. In the limited time at my disposal I shall endeavor to deal with those aspects which appear to me to be likely to excite the greatest amount of interest.

First, let us deal with the cause of appendicitis. Every case of appendicitis is, as far as we know, associated with the presence of organisms, and these organisms are similar to those which normally inhabit the intestinal canal. The degree of severity of the attack depends, in a large measure, on the virulence of the particular organism.

In order that any organisms may be able to run riot with the body cells it is necessary that they, as an attacking force, should be of greater power than the defending cells. In appendiceal infections it happens very rarely that the organisms already within the intestinal tract have acquired an increased degree of virulence, although such cases will occur in different forms of enteritis. I think, then, that the cause of the beginning of the trouble lies in that which diminishes the tissue resistance of the appendix itself. In a very large percentage of cases in which the appendix is removed for appendicitis, one or more concretions will be found within its lumen. I have only on four occasions found a foreign body, nor has an examination of the appendix disclosed the presence of any massive pieces of undigested food stuffs which might have passed in from the cæcum. The concretions varied in consistency, and some have been too hard to be broken in the fingers, and, in fact, appeared to be gall stones, but a careful examination showed that this was not the case; and the impression conveyed by the varying degree of solidity is that they, like gall stones in the gall bladder, have been gradually formed within the appendix.

When operating for the removal of an appendix about the third or fourth day, when the clinical symptoms induced one to believe that the attack was subsiding, I have noticed that the appendix gave me the impression that it had recently been distended. Again, every appendix which is removed during the middle of an acute attack will be found to be either greatly distended or ruptured. I think, therefore, that we would hardly be wrong if we were to assume that an inability of the normal secretions of the appendix to escape into the cæcum is the cause of a very large percentage of cases of appendicitis. Under normal conditions the secretion escapes readily: under abnormal conditions the secretion escapes with difficulty, and the residuum becomes more solid and forms a concretion, even as a concretion is formed in the gall bladder or in the urinary bladder when it is unable to completely empty itself. Directly the concretion comes into existence it acts as a foreign body, and excites inflammatory changes in the wall of the appendix—catarrhal at first, then ulcerative, and finally perforative. The perforations are very often indeed right opposite the concretion. The gangrenes are often similarly situated; but, at times, a considerable area is affected, and this can be readily explained by the great tension which exists within the appendix or by a thrombosis or arteritis being occasioned by the intense activity of the organisms, which have gained in power and virulence owing to the marked diminution in vitality following upon the primary increased tension. It is possible that an attack may be occasioned by thrombosis or arteritis of the mesentric vessels, for such a rare condition occurs in connection with intestinal vessels; still

those who operate largely on these cases must have been impressed with the very overwhelming proportion in which an increased tension is found within the appendix which is about to be removed during an acute attack.

To sum up, I maintain that obstruction to the outflow of the appendiceal secretion is most usually the cause of acute and chronic appendicitis. Certainly, to the clinician, the many different kinds of appendicitis so often spoken of are simply unrecognisable.

Next let us inquire into the probable cause of the obstruction. Kinks and twists are said to be common. These kinks and twists must be either congenital or acquired. If the kink is congenital it is possible that it may become more marked as the child grows and its abdominal contents alter in character. If the kink is acquired it will be found associated with inflammatory features in its immediate neighborhood. If in the pelvis, in women, pathological changes occurring in the genital organs will be quite sufficient to account for the kinking of the appendix. Typhlitis or peritphlitis is not to my knowledge particularly common, so that I should not like to say that it could be the cause of more than a very limited number of attacks. In fact a great number of the kinks and twists found in an appendix are caused by previous attacks of appendicitis.

We have now got rid of the probable causes which act from without. In examining appendices which have been removed during acute or chronic attacks it will be observed that, although there are no twists or kinks present, nevertheless a constriction exists—or maybe there are two, or even three, constrictions. The most constant constriction will be found at, or very close to, the cæcal extremity. These constrictions are congenital, and may be looked upon as evidences of a similar condition existing in the lower animals. It would perhaps be absurd to assume that this alone is the cause of an attack, but where the lumen of a mucus canal is narrowed it is surprising how little it will take to effectually block it. We have evidence of this in the ease with which an extra glass of alcohol will block the urethra of a man suffering from stricture, or hypertrophy of the prostate. We know that the constriction and the retention of the appendiceal contents is remarkably common, so that the least amount of swelling of the mucous membrane at the neck of the appendix defines the onset of an attack. It is probable that constipation, or diarrhœa, will cause some swelling of the mucous membrane of the cæcum, and produce sometimes a partial and sometimes a complete closure of the canal. I am inclined to think that dyspeptic attacks, obscure abdominal pains accompanied by malaise and a general feeling of unwellness, are very frequently due to a chronic appendicitis and consequent absorption of septic products. It is said that constipation is a common cause of appendicitis; but constipation is such an extraordinarily common complaint of modern life that he would be a rash man who would contend that it alone is the cause of appendicitis. If it were the case there would be an enormous proportion of us eligible for an attack of appendicitis.

I shall now proceed to discuss the question as to when we should operate. We have a scattered population, and medical men are in places few and far between. If a man is unaccustomed to operate, or if he is unable to obtain the assistance he feels necessary, he will certainly save more lives by abandoning all thought of operation and freely purging his patients in the early stage, with the object of depleting the mucous membrane and allowing the appendiceal contents to escape. Speaking from my knowledge of adhesions, I think that the damage likely to be done by the supposed breaking down of the adhesions by the administration of aperients is mythical. If an abscess forms adjacent to the abdominal wall, or the rectum, it may be opened without great difficulty through the abdominal wall, the vagina, or rectum; but if it

occur at any depth, and away from the abdominal parietes, a good assistant and the greatest of care and experience is required in order that the abscess may be opened without rupturing the bowel or spreading infection over the peritoneal cavity. A patient in an early stage, or with a localised abscess, may be transported for great distances without injury. If the patient is in the neighborhood of skilled assistance there is not the slightest doubt but that he should be submitted to operative interference immediately after the attack has begun, for no one can say, with any degree of certainty, whether the attack is likely to be perforative or accompanied by an acute peritonitis, or whether it will resolve. The pulse and the temperature are some guide, but frequently, by the time the pulse rate has increased, the favorable moment has passed, and the surgeon is invited to operate upon a patient who has a considerable amount of dirty fluid in his abdomen and a progressive peritonitis. The fallacy of waiting for a rise in the pulse rate is exemplified by the following case. A man, of 33 years, was taken ill with severe abdominal pains during the night. He was seen by his medical attendant on the following night. I saw him, in consultation, the following morning thirty-four hours after the onset of his illness. He had pain and some tenderness over the lower abdomen: his pulse was 84, his temperature 99. On opening his abdomen his appendix contained two concretions, and a perforation directly opposite one of the concretions. Offensive, thin, yellowish fluid, to the extent of a pint, was found in the pelvis, and a thin layer of it over the left loin. He recovered. This is a very unusual history, but I have seen at least one other precisely similar, and several whose symptoms were certainly hardly indicative of the grave condition within the abdomen.

If the patient is first seen after he has been ill a few days, and a small mass can be felt, careful inquiry should be made as to the course of the illness. If the pulse and temperature are low there is a fair probability that the mass does not contain pus, and that the inflammatory exudate will subside. To attempt to discover and remove an appendix from the midst of an inflammatory exudate is a difficult matter, and the operation is much more easily executed, and the patient's risk much diminished, if the removal is effected at a later stage. On the other hand, if the mass is accompanied by a rising temperature, or if a slight temperature persist, pus is almost certainly present. I would most emphatically condemn the advice given to any patient with an acute attack to wait until the acute symptoms have passed before submitting to operation. In the first place, no one, however great his experience, can say at the onset whether an attack will subside or end lethally. When a mass has formed after the fourth day we know that adhesions have been set up, and the appendix has been shut off, so that there is less risk in waiting for a short period to see if resolution is likely to take place.

When the appendix is discovered matted down in the pelvis, the Trendelenberg position should be used, and an incision made just outside the middle line. If the incision parallel to Poupart's ligament is adopted, I think it of very great importance not only to avoid as far as possible the actual division of the muscles, but also to preserve from injury the dorsal nerves and their branches. I have certainly seen cases where the union of the muscle along the line of incision has been perfect, but a bulging and laxity of the abdominal wall in that region will appear, which is very characteristic of atrophied muscle: on cutting down upon the part, the muscle fibres will be found to be pale and thin, and without tone. I do not think that many surgeons sufficiently recognise the importance of maintaining the integrity of the nerves of the abdominal wall.

Drainage through the abdominal incision is another fruitful cause of hernia. In some cases it is of paramount importance that free drainage

should be established in order to save the patient's life ; but very frequently, where the case is sufficiently septic to need drainage, a tube containing a gauze wick may be passed through a simple stab wound about an inch from the original incision. In doing this I usually make a short cut through the skin, and then, with my left forefinger inside as a guide, push a sharp-pointed scissors in an oblique fashion through the abdominal wall. This practice I have adopted for the past two years, and I have never seen it followed by hernia. A piece of gauze, changed three times daily, may usually be substituted for the tube after four or five days. I adopt the same stab method in the drainage of other abdominal conditions, and strongly recommend it to your notice.

In removing the appendix, in some difficult chronic cases, I have for the past two or three years adopted the following method :—The appendix is tied off at the cæcal end, and the stump held on one side. A forceps is attached to the distal cut end, securing the mucosa and submucosa only. Keeping a steady pull on the forceps, the muscular and peritoneal coats may be pushed off with a blunt-pointed scissors, or forceps, until the whole of the submucous and mucous portion comes away like a long earthworm. By this means the appendix may at times be removed from the most difficult situations with ease and safety.

There are many methods of dealing with the stump. Simply clamping a portion of the cæcum and cutting away the appendix with scissors, and sewing a double line of suture, as in suturing a piece of bowel, is an excellent method. However, the appendix is often so awkwardly situated that some of the more finished suture methods are impossible unless a large opening is made, and this is always to be avoided. It is not possible to ligature the base of the mucosa and submucosa, and push it within the cæcum : this is sometimes attempted, and a purse-string suture closes in the peritoneum round the inverted stump : it simply amounts to pushing the stump between the coats of the cæcum in that situation. Even though the stump has been treated with cautery or carbolic acid, it is not easy to be positive that all septic organisms have been destroyed. Returning the unligatured stump of mucosa and submucosa into the cæcum is unwise, for I have known considerable hæmmorrhage to take place from it. In this, as in all other operations, the method which allows rapidity of execution combined with safety has much to recommend it. What is, I believe, known as the " cuff " method is both scientifically sound and effective. After ligaturing the base of the appendix, a cuff of peritoneum and muscle coat is stripped down for about an inch and a quarter. The mucous and submucous coats are cut off as short as possible. This stump is treated either with the actual cautery or with pure carbolic acid. The cuff is re-drawn over the end of the stump, and left covering it. There is really no need to ligature the cuff again, because if any oozing should take place, it would be preferable that it should escape and be dealt with than that it should remain bottled up where the increased tension would be likely to interfere with the resisting power of the normal tissues. Such a trifling raw stump has never, to my knowledge, caused adhesions which gave rise to any symptoms. The stump can usually be left lying against the parietal wall, so that should any trouble take place the abscess would be in a safe position. If so placed it would probably remain there for some time, owing to the paralysis of the bowel from exposure and handling. Drainage should be adopted just as often as the surgeon thinks it is necessary ; there can be no hard-and-fast rule except it be the rule that, when in doubt, drain.

I believe there is a considerable difference of opinion as to whether the appendix should be removed even when an abscess is present. The appendix can be removed at any time by any man who is determined to do it, but the

question is, is it wise to remove it on all occasions? It would undoubtedly increase the patient's risk—perhaps not a great deal, but I am quite sure that one would be compelled to make a larger opening, and this is what should be avoided at all costs. If an abscess is present, a small opening is all that is usually needed: at a later stage this opening may be closed after the appendix has been removed, and, by means of a stab drain, if it be considered necessary to drain again, the abdominal wall is left as sound as it is possible to do so.

I do not think that any patient recovers from a true general peritonitis where the whole of the peritoneal surface is inflamed. In fact if, on opening the abdomen, the intestines are found matted together to any great extent with lymph, and more than one pocket of pus is found, the chances of recovery are small. I have seen these patients do well for a few days, and then there would follow a rise of temperature and a sudden break-away of pus in some part of the abdomen, and death would ensue. In severe early conditions there is a good chance of recovery if the dirty fæcal-looking fluid be thoroughly washed out of the abdomen. In such cases where free dirty fluid is found on opening the abdomen, I immediately find and remove the appendix, then make a cut in the left flank, and another in the right flank, and flush out through the left flank opening; so that the current tends towards the opening over the appendix. At the same time the finger is introduced into the pelvis, and recent adhesions are broken down so as to clear any pocketed collections of fluid. Gauze drains are admirable, but they act much more satisfactorily if introduced within a rubber tube, as there is a tendency for the gauze to become choked. The renewal of the gauze through the medium of a tube gives the patient very little pain as compared with its renewal without the tube. In connection with these cases I have often remarked two outstanding factors which are of immense service to us in saving the lives of our patients: firstly, the intra-abdominal pressure which tends to establish a flow towards the opening in its wall, a tendency, by the way, which is greatly assisted by the capillary attraction of the gauze; and, secondly, the disinclination of septic organisms to advance up stream. We often open into an abdomen and remove a dirty appendix, with infected material in its immediate neighborhood, but, provided a virulent blood infection has not taken place, the patient is almost certain to get well if a gauze drain is introduced so as to establish a current in the right direction.

Very free drainage must be adopted in some of these septic cases; and, partly because of this and partly because the peritoneum is already irritated, there is always a considerable risk of adhesive peritonitis giving rise to intestinal obstruction. If the pulse remains about the same as when the operation took place—it may be 100 or 120—and if there be distention, frequent vomiting or belching of flatus, accompanied by pains in the abdomen, it is more than likely that the patient has intestinal obstruction. At times, too, the bowels will be induced to act slightly; but still the patient does not sleep, and his abdominal pains persist, and his abdomen remains distended or becomes distended very soon after he expels a slight amount of flatus or fæces; he is still suffering from obstruction, and will probably die if not relieved. Many of these cases are apt to be looked upon as due to acute peritonitis, and nothing is done to relieve the patient; but with peritonitis we get a much more feeble running pulse, the vomiting is effortless, and the abdomen is usually more superficially tender all over. Nothing is easier than the relief of such a dangerous condition: and, in fact, the method I am about to mention is to be recommended in all cases of intestinal obstruction in the acute stage, for it is not advisable to attempt to discover and relieve an obstruction in a depressed patient with a distended abdomen. Make an incision one and a half inches long about half an inch to one side of the

middle line, low down ; split the rectus and open the peritoneum for about half an inch, pick up the first piece of distended bowel which presents ; see that it comes freely and easily, and is not likely to drag when made fast. Suture the parietal peritoneum to the peritoneal surface of the intestine with a curved round needle and fine catgut, so as to leave a small oval of intestine exposed. In doing this, hold the centre of the oval piece of bowel with a pair of forceps. Before opening the bowel, tuck a narrow strip of gauze round the sutured edge, so as to catch any escape of fæcal material ; then, with a sharp-pointed scissors, open the bowel and pass in a piece of tube as large as a number twelve catheter. If the fæcal fluid runs satisfactorily, let it drain away ; if not, inject a packet of salts dissolved in a little water into the tube, nip the end of the tube with a pair of forceps, and leave it there for about twenty minutes before releasing it. In almost every instance the bowels will begin to act of their own accord in a few days. The tube may be taken out and the fistula allowed to close : the opening through the muscle will greatly assist the closure. Never make the incision through the middle line or the semilunar line, and be satisfied to stitch together peritoneal surfaces only. This will greatly facilitate the sinking back and closure of the fistula later on. I have had fifteen out of eighteen recoveries after adopting this method in cases of obstruction following free drainage of extensive fæcal extravasation.

Dr. Davies and Dr. McEncroe, two of my house surgeons, have been at considerable pains to collect details of the cases operated upon by me at the Royal Prince Alfred Hospital during the past five years. These, together with those I have operated upon in other places, amount to 452. The death percentage in hospital amounted to 9.5 per cent., whereas the other series gives me but 5 per cent. of deaths. The only patient who died when operated upon during the interval died about the fourteenth day, from intestinal obstruction. Her intestines were much matted from additional pelvic disease, and the intestines were so constricted that drainage of the bowel was of little service. I have seen the same multiple constriction occur in two other cases of intestinal obstruction. Both died. Note carefully why these patients died. Every one who died had marked adhesive peritonitis, extravasation of fæcal fluid, or extensive abscess formation. On two occasions a foul sloughy appendix had so infected the patients that even after their appendices had been removed, and careful drainage had been established, they made no improvement whatever. I think it is fair to say that almost every case would have recovered if the patient had been operated upon directly the earliest symptoms had become manifest.

This record will make it plain to the greatest sceptic that immediate operation is the true safe treatment for appendicitis. Besides those patients who died, there were many other cases of abscess and general extravasation with rupture which recovered, and many who would never have reached such a dangerous condition had immediate removal been recommended as the proper scientific method of treatment. I have known cases recently where experienced surgeons have, even as early as the second and third day, recommended patients to wait until the interval before having the appendix removed, and these same patients have developed abscess, or have gone on to rupture. I can hardly be accused of possessing the *furor secandi* of a young man, nor have I blindly followed any man's teaching, but I give you the results of my own experience ; and I have demonstrated repeatedly to others the utter impossibility of saying what the future progress of an appendicitis will be. I think many of the more experienced physicians in New South Wales entertain the same views with regard to the necessity for immediate operation. Undoubtedly many attacks of appendicitis do resolve, but it is

no difficult matter for a surgeon of any experience to obtain 1 per cent., or even a smaller mortality, for operation for the removal of appendices which have not created serious systemic disturbances.

It is only natural that a larger number of deaths should occur in a general hospital, simply because among the poorer folk illness is often disregarded for a longer interval, and the patients are admitted to hospital in a much more serious condition. In private work, it is the practitioner's duty to plainly lay before the patient what should be done to give him the best chance of recovery: if he elect to take his chance, there is no help for it; but it certainly is hard upon the operating surgeon who is called in to operate upon a patient who, through delay, has but a very much-diminished chance of recovery. So long as the fault lies with the patient we must put up with it; but if the medical attendant is foolish enough to take the responsibility of the unknown future upon his own shoulders, he must make up his mind to bear the blame which will certainly be his if his patient dies.

This subject is such a very large one that I have purposely only touched upon a few points which, I hoped, would interest you; and, before closing, I should like to caution you that appendicitis in very young children may very easily escape notice. One of my cases was 2 years of age. He had an abscess, and recovered after a few weeks. The other was 3 years of age; he had a ruptured appendix, with general peritonitis, and died a few hours after operation. The oldest patient I have seen suffering from appendicitis was a man of 80 years: so that there is no age which is free from the possibility of an attack.

My contention is that the appendix can always be removed up to the third day after the onset of acute symptoms. That it should be removed in all cases where rupture or a loose extravasation of septic fluid is present. In cases of abscess, say, of a week or more old, where the abscess is shut off by firm adhesions, it will be safer to drain the abscess and remove the appendix at a later date. Large incisions have appeared to me to be too often followed by an atrophy of the muscle fibres on the lower side of the oblique incision usually adopted, so that a sound repair of the hernia cannot be brought about. I invariably use an incision coming close up to the deep epigastric artery, so that the pelvis may be explored at the same time.

ON THE TREATMENT OF APPENDICITIS.

By W. J. Long, M.B., Ch.B.

In my opinion the proper treatment of appendicitis is removal of the appendix as soon as possible after the onset of the disease. The main reasons for this opinion are:—(1) There would rarely be any complications, with their attendant risks; (2) there would be no recurrent attacks. This would mean that the death-rate from appendicitis would almost certainly be not more than 2 per cent.

The only reservations I make are:—1. Unfitness of the patient, owing to his having other illness, *e.g.*, typhoid fever, advanced tuberculosis, advanced heart disease, &c., or his own desire that he run the risk of the attack, after having this explained to him. In the latter case, the question of operation is left out of the treatment of the appendicitis and reserved for its complications: practically this is the stand taken by most practitioners and their patients at the present time. Further, the patient may not be seen until the attack is subsiding, or until some complication has arisen.

2. Unfitness of surroundings owing to their poorness, and to the paucity of attendance, or owing to the absence of a surgeon well versed in aseptic abdominal surgery. It is obviously unwise, on seeing a patient right away in the country, to operate at once unless there are grave reasons for doing so. It is safer to treat the case expectantly, and operate on any complication that arises, or in the interval if this do not happen. It is also obviously unfair, both to patient and practitioner, to operate when the latter is not in a position to be thoroughly well up in abdominal surgery.

Expectant Treatment.—After making a diagnosis, attempting at the time to localise the appendix and the nature of the disease in it, if the patient be in much pain I give one-third to one-half grain of morphia, apply heat to abdomen, and watch the patient. If the bowels have not been emptied I allow an enema: it is wise to abstain from purgatives. The patient has generally taken salts or oil and promptly vomited them. The aim of the treatment is to keep the inflamed parts as quiet as possible. The physiognomy, pulse quality and rate, state of abdomen, and temperature are carefully watched, and the decision made as to whether the case is recovering or going on to complications. No rule, such as a pulse rate of over a hundred, should guide the surgeon as to the patient's condition. It is the combination of high temperature and pulse-rate, with anxious expression and hard abdomen—or, still worse, the combination of high pulse-rate with low temperature and a beaten expression—that make the surgeon operate at once.

The Treatment by Removal.—The treatment by removal in the interval and early in the attack is the same. An incision 2 to 4 inches in length, with its centre corresponding to where the base of the appendix is expected to be, is made in the direction of the fibres of the external oblique, its length corresponding with the obesity of the patient. The fibres of the external oblique are separated and retracted widely. The fibres of the internal oblique and transversalis are then separated from the border of the rectus outwards to the ilium, and also retracted. The peritoneum is cut in the direction of the original incision. All bleeding is stopped by pressure, a ligature only being applied where necessary. The wound is retracted as required. The index and middle fingers, of whichever hand the operator is most expert with, are introduced, and a rapid careful examination is made, chiefly noting the extent of the adhesions to the surrounding organs, omentum, mesentery, bowels, bladder, pelvis, large blood vessels, muscles, &c.

In general it is best to find the longitudinal band on the front of the colon and trace it down: practically always, the base of the appendix is found at its end. The adhesions should be broken down in the inverse order in which they have been formed: this is usually determined by the ease with which they undo. Very little bleeding accompanies this as a rule. It must always be done very gently, and it is, at times, very difficult. Sterilised pads should be placed to prevent the soiling spreading from the operation area. If the omentum is tightly adherent it is better to remove the adherent parts with the appendix. In this way I have more than once removed the appendix with a surrounding abscess whole, without soiling the operation area. If there be an old fistula between the appendix and the bowel, the opening into the bowel must be turned in and sutured. Retention products, serum, pus, etc., in the appendix, must be not scattered in stripping it out. Should the serous coat of the bowel be injured the rent should be sutured, to avoid future adhesions or fæcal fistula. When the appendix is cleared of adhesions its mesentery is ligatured off, and the appendix is crushed at its junction with the cæcum. A purse-string suture is applied to the cæcum round the base, the appendix is cut through the clamped portion and inverted, care being taken to place a piece of gauze so that the area could not be soiled, and the purse-

string suture is tightened. This method is practically never followed by fæcal fistula. If the operator choose, he may re-duplicate the purse-string suture, or he may place a few interrupted sutures. There is no need to ligature the appendix nor to turn down peritoneal cuffs. If the patient is bad and the surgeon is pushed, a ligature may simply be tied round the base of the appendix: as a rule this simple method gives perfect results. The mucous membrane left may be cauterised with pure carbolic acid, care being taken to remove any excess. The field of operation is then gone over for bleeding points, is cleaned with hot saline solution, and is sutured in layers. I always use silk for all buried sutures—fine Chinese twist; one or two points through the transversalis and internal oblique are sufficient—more for the external oblique, and horsehair for the skin. A sterile gauze dressing is applied.

Difficulties may arise during the course of the operation owing to the density of the adhesions, or the position of the appendix. If, after a reasonable time, the appendix is not delivered out of the wound, the incision may be enlarged by carrying it across the fibres of the internal oblique as far as may be necessary. The fibres must be sutured together carefully afterwards. The appendix may lie hanging into the pelvis, where it often is adherent to the iliac vein: this is fairly easily torn; or it may be adherent to the bladder or rectum. It may be turned back into a retro-colic pouch, and need stripping out from that position. If it seem impossible to excise the appendix, if any one part can be found it should be opened and followed up to the two ends, and the mucous muscular coats dissected out; the outer coats can then be sutured again, and the adhesion left alone. It must not be forgotten that the adhesions formed are often worse than the original disease.

When I am expecting the operation to be difficult I usually make my incision in the semilunar line. If pus is not expected, the incision can to some extent be made more secure against hernia by incising the different layers not immediately in the same line right through. If pus is expected, it is better to go straight through. If the abdominal wall be accurately sutured by interrupted sutures, picking up each layer to the same amount on each side and very little peritoneum, hernia very seldom results. This incision gives more room than the gridiron, especially for exploring the pelvis. The deep epigastric artery may be ligatured if necessary; but this is seldom the case. It has only once happened to me that I could not find all of the appendix: in this case I only got small pieces, and went on to undo the adhesions that I thought were causing most of the symptoms.

After-treatment.—This consists most largely in letting the patient alone. He must be watched for symptoms of hæmorrhage and peritonitis, and in either case the abdomen must be promptly opened. The hæmorrhage must be stopped, or the peritonitic area shut off with gauze and drained. Morphia may be given for pain; strychnine for shock. Hot water may be used to wash out the mouth, and it or tea may be swallowed as soon as the patient desires it: it does not increase the vomiting, but it makes it much easier, as there is something more than just a mouthful of bile to bring up. Mustard leaves or linseed and mustard poultice are to be tried to relieve the vomiting, and, if bad, brandy, or champagne and soda, warm draughts of soda bicarbonate, frequent minim doses of vin. ipecac. on the tongue may be tried. The patient may be turned on the side, or on back with knees drawn up. As a rule the bowels can be safely left alone for forty-eight hours or longer. If any distension occur, enemata of soap and water, with oil and turps, are the best. When a purgative is needed I generally give 5grs. of calomel, and, after a few hours, a Seidlitz powder, or repeated drachm doses of mag. sulph. until an evacuation is secured. I sometimes give other things, as best suited to the individual cases.

The sutures may be removed from fifth to seventh day (in about ninth or tenth for through suture), and a collodion splint applied. The patient is allowed up in fourteen or twenty-one days, according to the case and to the suturing.

COMPLICATIONS.

1. *Hæmorrhage.*—This must be treated on ordinary lines, tying where possible and plugging where not. The plugs must be carefully placed, for they are liable to cause obstruction and fæcal fistula. A rent in the larger veins should be sewn.

2. *Abcess, or Localised Peritonitis and Diffused Peritonitis.*—This is the most common complication. It is most commonly caused by sloughing and perforation of the appendix; but there may be no perforation. If the inflammation is very intense and virulent, and the appendix is gangrenous, the patient is so prostrated that no barrier is thrown out, and he dies from general peritonitis. If not so intense, or if some adhesions have been formed by previous attacks, the peritonitis is localised and abscess (intra-peritoneal) is formed. In a few cases the appendix is retro-peritoneal, and then the abscess is extra-peritoneal.

Abscess.—This occurs where the perforation is, and, further, wherever the appendix is—unless transplantation by the omentum, or by gravity, has taken place: consequently its position varies very much. Many cases of abscess recover without operation: they either are absorbed, or burst into the bowel. Often the operation is simplified by leaving the abscess till it gets large, and is reached easily from the surface; but the proper treatment, undoubtedly, is to open it directly it is diagnosed. If the surgeon be fortunate enough to see the case early, he is generally able to remove the appendix as well as the pus; this is of great benefit. If left, it causes further attacks in some cases, and I am of opinion that in early operation for abscess one is more likely to get recurrence if the appendix is not removed than where the operation is done later. In the latter cases the appendix often sloughs away and comes out with the pus. Further, the drainage is less after removing the appendix, and so the patient has a better abdominal wall left. In cases not seen till late it is much wiser not to attempt to remove the appendix, unless it happens to come to the examining finger at once. Its removal is tedious and risky, as it endangers the continuity of the abscess wall. In deciding the incision to be made, the position of the previous pain and the position of the swelling must be taken into account. Unless the case is seen quite early (when the gridiron incision may be used), an incision cutting in one line through is the best, as giving the most room. When the pain and swelling are situated in the loin, and the leg is flexed and there is pain on moving the leg, and possibly mucus or blood in the stools, the appendix is behind the cæcum, and the incision may be the same as the lower end of the incision for nephrectomy, and it may not enter the peritoneum at all, or at any rate it may only enter it amongst the adhesions, and the free cavity is not opened. If the peritoneum cavity is opened the field of operation must be carefully packed off, so that the contamination will not spread.

When the pain and swelling are in the iliac fossa, and there may be some flexion of the leg, in this case, also, it is best to open well to the outer side. Often in this way the abscess may be opened without entering the free cavity.

When the pain and swelling are situated nearer to the umbilicus it is generally impossible to open the abscess without crossing the free peritoneum cavity to get at it. These are probably the most dangerous of the abscess cases. The main safeguard is in making a fairly large incision, so that a good

packing can be placed all round the abscess. When this is satisfactorily done the pus is reached by separating the adhesions where they seem easiest to undo: the pus is mopped out as it wells up, and the cavity is thoroughly emptied. If the appendix is seen, it is removed: if not, the cavity is carefully and loosely filled with iodoform gauze; a rubber tube, without side holes and wrapped round with iodoform gauze, being placed in the centre of the pus cavity. After this the packing is removed, being replaced by a new and smaller one of iodoform gauze. The end of the tube must not rest on the bowel, as it is likely to cause fæcal fistula. The tube and gauze in the abscess cavity will need renewal before the surrounding pack: the latter should be left from four to six days, so as to get a good wall of adhesions to keep the pus away from the free cavity. The centre gauze may have to be changed daily, owing to the amount of the discharge. Meddle as little as possible with the gauze. Obstruction of the bowels may come on, and, of course, it is paramount in the treatment. The gauze must be removed; and if this prove insufficient, the wound must be re-opened and the bowel examined, or a small hole be put in the nearest piece of ileum and a fæcal fistula formed. This generally heals up in four or five weeks; if not it is excised, and the hole in the bowel sutured.

When the pain is deep in the pelvis, and there is pain on passing water and on emptying the rectum, and there is considerable distention of the lower pole of the abdomen, it is due to the abscess forming low down in the pelvis. These cases are often overlooked until the pelvis is full of pus. There is no tumor to be felt on abdominal examination at first, but there is on rectal examination. It is to be treated as in other abscess cases. If seen early it is almost always possible and right to remove the appendix. It is usually the tip that is in trouble. The base can be ligatured off, and the sutured bowel packed away from the field that is to be soiled: then the appendix is traced down, and the pus removed, and then the appendix (a fæcal concretion often being found at the bottom of the pelvis), and a rubber tube and gauze drain inserted, as described. When the abscess is larger, and, as is common, fills the pelvis, it may be drained by the posterior vaginal route in women: this gives dependent drainage. Occasionally it can be drained by the rectum in men: if not, it is well to keep as near the edge of the peritoneal cavity as possible, so as to keep within the adhesions, and simply empty out the contents and place a large tube in the cavity. If the free peritoneum cavity is opened, the packing off must be perfectly done. It is well not to make the tube too long, because the cavity contracts, and then the tube may press against the bowel. I do not believe in washing out these cavities. When the appendix is left, fæcal fistula often forms in these cases owing to the dependent position of the opening in the appendix. Sometimes these abscesses are fæcal, and contain gas at the time of opening them.

Diffused Peritonitis.—It is only a step from localised peritonitis, or abscess, to diffuse peritonitis. The worst form of diffuse peritonitis is that in which the infection is so severe from the start that no limiting adhesions are set up, and if an early diagnosis and immediate operation are not carried out the patient dies very quickly. In other cases there has been localisation at first, and, owing to some accident, *e.g.*, increase of tension through steady increase of the pus, too vigorous movements of the patient, or too vigorous manipulation on the part of the surgeon, the pus escapes from the abscess cavity and sets up further peritonitis. Even then it may localise again and a second abscess is formed, communicating or not with the original cavity. I have found quite different looking pus in the two cavities.

As soon as a diagnosis of diffuse peritonitis is made it is important to lose no time in preparing for operation. The belly can be prepared rapidly under

chloroform, and a large incision made over where the abscess is believed to be. If the appendix can be found (and it generally can), it should be removed by simply tying it off. The pus must be mopped out carefully, and gauze drains with tubing inserted wherever the pus has been. If the operator is not sure about all the pus being got out, it is better to make another incision and be sure about it.

If the pelvis only has been filled with pus, two abdominal incisions will be enough. The pelvis must be mopped dry, and plenty of drainage instituted. If the pus has welled up further, the loins must both be incised, and large rubber tubes inserted, and gauze drains run from opening to opening. The main idea is to drain wherever there is pus, regardless of the number or position of the incisions. I do not believe in washing out the cavity, but if it is to be done it is best to put the irrigating tube in above where the pus has been, and wash out from a clean place towards the dirty part, so as not to soil the rest of the peritoneum. The pus may be washed out of the pelvis if necessary by keeping all the wounds well open, and holding up the bowels so that the irrigation does not spread amongst the bowels at all. Then the drains are placed, saline is put in subcutaneously, if necessary (and it seems to do good in these cases), and the patient is put to bed with the head of the bed considerably raised. Morphia has to be given on account of the great pain and restlessness. Saline enemata are given as soon as the patient can retain them. Stimulation is carried out as required. Here, as elsewhere, the patient must not be exhausted by over-treatment. All vessels and the appendix ligatured in these pus cases should have the ligatures left long: they generally come out in from seven to ten days, and save a great deal of trouble from stitch abscess later.

No sutures should be buried in the abdomen wall in pus cases. They are almost certain to suppurate, and everyone knows how much worry suppuration round a piece of silk can give.

Stitch abscess is best treated by curetting the wound with a small ear c rette (fishing), with the object of removing the suture.

When these diffuse peritonitis cases recover it is wise to give an anæsthetic as soon after as seems right, according to the patient's condition, and either scrape all the granulations away or excise the wounds and suture them up properly. This also applies to long drained abscesses. Vental hernia is usually saved by this.

3. *Fæcal Fistula.*—This generally occurs in abscess cases where the appendix has been left. The fistula is in the appendix, and generally heals as the wound granulates: in other cases it may be from the ileum or colon, particularly when much adhesion has been stripped—and I have seen it in the duodenum. The latter case wastes so rapidly that it is best to follow it up soon, and suture the rent in the bowel. Ordinary fæcal fistula usually heals spontaneously: if not, follow down the channel, dissect it right out, and sew up the hole left in the bowel after its excision.

4. *Ileus and Intestinal Obstruction.*—If the patient continues to vomit, and gets distended, it is wise to remove the drains; and if no benefit ensues, the wound should be re-opened, and any kinks or adhesions removed. Generally the best thing to do is to draw the nearest piece of the ileum into the wound and open it. It is wise, if expecting trouble, to leave a piece near the wound so that there will be no trouble in finding it.

5. *Septicæmia.*—This is treated just in the same way as in any other septic case. Quinine, antistrepto-coccic serum (if the case seems to be suitable), and stimulants being the best.

6. *Pyæmia.*—Treat on ordinary surgical lines. Open every abscess as it forms, and keep up patient's strength. Quinine is the best drug. Strychnine and digitalis and alcohol are required.

7. *Pylephlebitis.*—This is noted by the patient becoming jaundiced wildly delirious, comatose, with widely dilated pupils, and death. I do not know what can be done.

8. *Abscess of Liver.*—There is generally tenderness over the liver, some increase of dullness on percussion, jaundice, sweats, and hectic temperature. An incision must be made into the abscess. Exploration with needle first is justifiable here.

9. *Subphenic Abscess.*—Increased dulness of the liver, the latter being pushed down; perhaps pain and cough, and sometimes jaundice, and sputa, and diaphragmatic pleurisy. It is best opened by removing a part of one or two ribs directly over where the pus has been got on exploration. The diaphragm is stitched to the skin, and then the abscess is opened and a large drain placed. There is often a great deal of bile drains away, and plugging the cavity with gauze is the best treatment for it.

10. *Pleurisy and Bronchitis.*—Treat on ordinary lines.

11. *Empyœma.*—This is not common, but is sometimes met with, particularly in cases of abscess behind the colon. It is treated by open drainage.

12. *Tubercle.*—This is a serious condition, as it is generally only in tubercular ulcer of the bowel and after a considerable time that the appendix is affected sufficiently to form tuberculous abscess. There are, generally, a large mass of glands, too extensive for dissection. One case went delirious after operation (apparently from general tuberculous dissemination), and died much in the same way as I have seen cases do after operations on tubercular hips, &c., many years ago.

13. *Foreign Bodies.*—Personally I have yet to find a foreign body in the appendix, unless fæcal concretions of all shapes and sizes are counted as such. The treatment is not altered.

14. *Pyosalpinx.*—This should be removed at the same time as the appendix. The treatment is the same, whichever organ started the mischief.

15. *Hernia of Appendix.*—When found in a hernial sac the hernia is usually of the sliding variety, and the utmost care must be taken in tying off the sac. If not, some of the blood supply of the colon may be taken up in the ligature, and death of that part of the bowel occur. If the appendix is unhealthy it should be removed; otherwise I do not think it necessary.

16. *Hydatid.*—I have seen hydatids growing in the serous coat of the appendix in a multiple abdominal case, and have removed the appendix for appendicitis caused by pressure of hydatids.

17. *Kidney Complications.*—Perinephritis is the commonest of these, and is usually seen in the retrocolic cases. Some degree of nephritis and pyelitis may occur.

18. *Malignant Disease* may occur here as elsewhere. The symptoms are the same as for cæcal cancer, and the only treatment is excision.

19. *Cystitis.*—Its best treatment is the removal of the abscess, and the administration of urotropin.

20. *Ventral Hernia.*—If the patient is very obese, and the hernia large, a well-fitted belt may be used; but, as a rule, it is best to completely excise the scar and suture the wound in layers. If it cannot be well brought together, a " grid " of hardened silver wire may be used to stiffen the wound.

21. *Pain in Wound.*—Patients at times complain of not being able to straighten themselves: this is generally due to adhesions re-forming inside after operation. They usually stretch or absorb, and the patient gets well. Occasionally a second operation is necessary to get rid of the condition.

22. *Omental Abscess.*—This seems to be formed by the omentum being affected from contact or adhesion with the appendix, and rolling up in itself in the same way as it occasionally rolls itself around the appendix. I have

treated it both by complete removal with the appendix, and by incision into the abscess through a new incision. In the latter case the abscess was adherent to the anterior abdominal wall.

23. Injuries caused by retractions to blood vessels and bowels.

The above remarks are gathered from my operative experience of the last eight years.

APPENDICITIS: A CLINICAL REVIEW.

By Robert Scott, M.D., Ch.M.

In bringing under your notice the subject of my paper, and traversing ground that has already been so well and ably exploited by distinguished members of the profession all over the world, my only excuse must be that appendicitis, in one or other of its varied forms, is a disease of such wide significance, and one with which we are all—whether physicians or surgeons—brought so frequently face to face.

Being then of such frequent occurrence, there is more likelihood of an interesting discussion following; and, in that discussion, let me express the hope that we may gain some useful information that may enable us to combat the disease more successfully, and still further lessen the mortality in the acuter forms—which still persists, despite the splendid triumphs of modern aseptic surgery. There is no disease in the acute inflammatory stage which permits less of the "festina lente" method of treatment. Timidity must be put aside; diagnosis must be rapid, and a definite and determined line of treatment carried out. Many of us, who have been in practice for a number of years, have reason to look back with regret on the line of treatment which has for its refrain "He seems a little easier; we will see how he is in the morning," and the morning brings with it a perforative appendicitis with an acute peritonitis, which, in so many cases—even with an immediate operation—means death to the patient.

I had hoped to be able to collect all the cases treated by medical men in our city and district, whether operated on or treated without operation; but, finding it impossible to get anything like a complete list, I abandoned the idea, and give a list of all cases operated on in the Ballarat Hospital during the last three years, which was kindly supplied by the Resident Medical Officer (Dr. Langmore). I also give a list of cases seen by me in private—some of them in consultation. These tables cannot, however, be put forward as being of any statistical value in showing the actual prevalence of appendicitis in our district.

Date.	Total Number of Cases Operated upon.	Catarrhal Cases.		Suppurative Cases.		Remarks.
		Recovered.	Died.	Recovered.	Died.	
1902 .... (9 months)	Males, 3; females, 2	—	—	4	1	Peritonitis following operation
1903	" 5 " 4	8	—	—	1	Complicated with abdominal hydatid
1904 . ..	" 12 " 8	10	—	9	1	Peritonitis following operation
1905 .... (5 months)	" 3 " 4	3	—	3	1	Subphrenic abscess, which burst into lung

In my case-book I have notes of twenty-one cases during the last two years, of which fifteen were operated on. Of these nine were catarrhal, and all recovered ; four were suppurative, and all recovered ; two were cases of general peritonitis, and died from toxæmia a few hours after operation. Of the six cases treated without operation three were catarrhal and two suppurative, and all recovered ; one was a case of general peritonitis, which died from infection following perforation of the appendix. Of the twenty suppurative cases operated on, there were only two in which the appendix was removed, and these two both died.

Although some of the cases had prolonged convalescence, they all made a good recovery, and not a single case has come under observation again for any relapse or symptom necessitating further operation for removal of the appendix.

I do not propose to enter fully into the functions of the appendix. It appears to be a matter of conjecture as to its true use in the economy of nature. Sir William McEwan, in a Huxley lecture delivered by him, holds that the appendix has a distinct and definite function in digestion ; and instances the case in which the anterior wall of the colon had been blown away by an explosion, enabling the condition of the mucous membrane of the cæcum and the orifice of the appendix to be carefully noted. It was observed that, shortly after food was taken, an exudate of glairy alkaline mucus was poured out from the orifice of the appendix, as well as from the mucous surface of the cæcum. It was noted that, after the reception of "bad news," which brought on an attack which he called "indigestion," the secretion in the cæcum became drier, causing smarting round the edges of the wound due to the acid chyme which was poured into the cæcum not being sufficiently neutralised by the "succus entericus," which, under normal conditions, was secreted by the cæcal and appendical glands.

A man 56 years of age was operated on by me for an appendical abscess, which was opened and drained without removing the appendix. On the twelfth day, on removing the gauze drain, about 4 ounces of glairy alkaline mucus welled up from the wound. This condition persisted for about ten days, sometimes being more profuse than at other times. Eventually the sinus healed up, and he has had no further trouble. Evidently this secretion came from an unobliterated part of the appendix which gradually became sealed up, for at no time was there any fæcal fistula.

Etiology.—Much has been written, and many theories advanced, as to the causation of appendicitis, and the alarming increase in the number of cases occurring of late years. Heredity, enteroliths, gallstones, rheumatism, gout, influenza (responsible for so many sequelæ), ectopic gestation, trauma, errors in digestion, and many other causes, have all their advocates.

I believe there is no more potent factor than digestive disturbances. MacEwan says that "present day man seems to act as if food should be thrown into the stomach, as a sandwich into a pocket, and the lid closed. Then he wonders that he has indigestion and appendicitis. Though, in a sense, digestion is independent of the direct control of the brain, yet powerful mental impressions exert an influence over the glands concerned in digestion, from the salivary down to the glands of Lieberkühn in the cæcum and appendix. The mental influence is such that, when the mind is engrossed in other affairs than the taking of food, those gland secretions are apt to be scanty, and indigestion is apt to follow. The standing lunch, eaten against time, with the mind otherwise occupied, is one of the best ways of not only producing indigestion, but of ultimately causing cæcal and appendicular mischief."

It is a recognised fact that appendicitis is most frequent between the ages of 10 and 30. It we contrast the amount of work put into those

twenty years now with what was expected of an individual fifty years ago under like conditions, we will see that the greatly increased strain put upon the infantile and adolescent tissues must leave a weakened condition, less able to withstand the onslaught of any microbic invasion, and the first organs to suffer would be those of the digestive system. Now, if the appendix take an active part in digestion, would it not be one of the earliest to suffer, when we remember its anatomical construction—a cul-de-sac. What more likely than prolonged neglect of the ordinary laws of digestion in causing inhibition of the functions of the cæcum and appendix with subsequent stasis and inflammatory changes?

What is the ordinary condition of life in our States? Almost as soon as the child is able to think for himself, he is sent to school. The infant comes home from school, partakes of a hearty meal, and returns to school, where he sits—often in a constrained attitude—for two hours. All this time his brain is engrossed with the school work. Until about the age of 14 the child absorbs as much mental pabulum as possible, and if he shows any aptitude in a particular subject—such as music, or recitation—probably enters for some local "competition," which is a further strain on his mental capabilities. Then comes office work, or a university career, when the conditions of life are still strenuous; and, if anxious to succeed, has often to forego outdoor exercise for sedentary work, and has probably irregular meals. Even if exercise be taken, the boy often acts unwisely, in his ignorance—rushing from the office, or home, after a hasty meal, to play a game of tennis or football, or—worst of all—go for a long bicycle ride. All these conditions must, sooner or later, bring serious consequences in their train; the digestive system will probably earliest suffer, and the part that will soonest become disorganised, owing to its anatomy, is the appendix. I have gone fully into this somewhat homely picture, because, from a clinician's point of view, a timely warning to parents would do much to help physique and lessen disease. *Mens sana in corpore sano* never more urgently required impressing on parents. If the child were allowed to "run wild" till the age of 7, we would hear less of "post nasals" and appendicitis, and one would also find an improvement in the physique of coming generations in Australia. It is well known that appendicitis is rare among the black races, the reason being due to their mode of life.

Trauma is, no doubt, a very frequent cause of appendicitis; and possibly this is the reason that males are more frequently attacked than females.

A case bearing on this was a man who was thrown out of his buggy, and was so caught between the wheels that the back wheel pressed deeply into the iliac region. This happened about ten years ago. He was not laid up, nor did he complain at the time; but, on consulting me some weeks later, he stated that after a long drive or continued standing he had pain referred to McBurney's point with a sense of uneasiness and fulness over the cæcal region, which was always relieved by a night's rest. This condition has persisted up to the present. The accident probably caused a slight local peritonitis, and anything causing stasis of the bowel sets up a local hyperæmia and pain due to adhesions.

To enumerate the various factors which may have a bearing on the etiology of the disease would be tedious, but I may here cite the case of a woman admitted to the hospital under my care, with a history of pain in the right side for a fortnight, when she noticed a swelling in the right iliac region; otherwise the symptoms presented nothing of urgency. An incision was made over the swelling, but finding the parts matted together, a second incision was made in the mid-line. I then found the condition to be a tubal

pregnancy adherent to the parietal peritoneum, with a matting of omentum and the tip of the appendix firmly glued into the mass. These were all removed, together with a parovarium cyst of the same side, and the patient made a good recovery. Apparently the ectopic gestation had been the exciting cause. Here I may remark that it should be a rule always to examine the uterine appendages of the right side when operating on a female, involvment of these organs being such a common occurrence.

From the clinician's point of view the various forms that appendicitis may assume fall under three great heads:—(1) Catarrhal appendicitis; (2) suppurative appendicitis; (3) acute toxæmic appendicitis.

The amount of literature that has been written about the treatment of these various forms rather appals one, and even yet no hard-and-fast rules can be laid down. Every case must be a law unto itself, and every medical man must exercise his own judgment and formulate his own rules for guidance, based on past experience and aided by knowledge gained from the experience of others.

In (1) catarrhal appendicitis, when one is called in early, and the patient complains of pain in the umbilical region, with a temperature of 99° to 100°, slightly furred tongue and nausea, with perhaps vomiting, but no thickening in the cæcal region, and no great rigidity of the abdominal muscles, the treatment consists of a saline purgative or calomel with small doses of morphia to allay the pain, and hot foments over the painful area, with liquid diet consisting of chicken or mutton broth and albumen or barley water. I always instruct the nurse to take the temperature and pulse every two hours, and at once report any rise above 100·5. At the same time the friends are informed that an operation may be necessary at any time during the attack, and, if not during the attack—which may prove mild—then during the quiescent period an operation should be performed. In mild cases I always keep the patient in bed until all tenderness has disappeared—a variable length of time. If operation then be declined, the responsibility rests with the patient himself.

(2) Suppurative appendicitis.—One is called in and finds the patient complaining of pain in the right iliac fossa, pulse and respiration increased, temperature 101 to 102. Tongue coated with a creamy fur, and the right leg flexed on the thigh; vomiting usually, and nausea always, present; on palpation, a rounded swelling is felt in the cæcal region, painful on pressure, but the rest of the abdomen fairly flaccid.

When confronted with these symptoms, what is the line of treatment to adopt?

During the time I have been in practice, no class of case has given greater anxiety. Before these cases were so well understood, and before asceptic surgery had attained its present brilliant position, one hesitated before advising operation, and waited for nature to effect a cure. How many lives have been lost by thus temporising! Some years ago I waited for four days, and if the symptoms did not improve, advised operation. Now I am firmly convinced that the only treatment admissible is *early and immediate operation*. If the case is being attended by a physician, he should call in a surgeon. When the case has reached this stage, it is no longer one requiring palliative treatment. When operation is decided on, what form should the operation take? On this point opinion is still at variance. Many leading American and Continental surgeons contend that the incision should be on the inner side of the swelling; others, just as eminent, that the first opening should be over the most prominent part of the abscess. It has always appeared to me that the incision should, in the first place, be made over the swelling. As the peritoneum is reached the incision is gradually deepened, using the finger to

indicate by sense of touch whether the pus is nearly reached. When pus is reached, the opening into the abscess is very carefully enlarged, taking every precaution not to break down the retaining abscess wall. The contents are then very carefully swabbed out; if the quantity is great, I do not hesitate to use a saline douche, taking care not to exercise any great pressure. If the appendix is lying in view, I ligature and remove it, but waste no time looking for it. An iodoform gauze drain is then loosely packed in the cavity, and the ordinary dressings applied. Should the incision be larger than is considered necessary, one or two sutures are inserted.

Murphy, of America, in a paper published some months ago, advises that in these cases the operation should be directed to opening the abscess and draining, the appendix being amputated only if it is readily accessible, the greatest care being taken to avoid separating the agglutinated intestines. This appears the more rational course; the patient is not in the best condition for sustaining the shock of a prolonged operation. There is decided danger in breaking down the wall of the pus cavity, and in some cases the appendix will not be found lying in the abscess cavity. If the operation has taken place into the meso-appendix, the organ itself may really lie outside, or form part of the wall of the abscess sac, and in such cases any attempt to remove it would almost surely prove disastrous to the patient. In the majority of cases the appendix becomes a necrotic mass, which comes away in the discharge, or becomes so glued down and obliterated that it gives no further trouble. Should the appendix give further trouble, it can be searched for and removed at a second operation at a very slight risk to the patient.

(3) Acute toxæmic appendicitis. I have used this nomenclature because it expresses the condition of the patient; although it may not be strictly classical, still the appendicitis is the exciting cause. These are the cases which Mayo Robson aptly describes as being of "a peculiarly tragic nature." The attack comes on, in many cases, with startling suddenness. From being in a condition of perfect health the patient is, in a few hours, fighting for life against an army of malignant microbes. The diagnosis is comparatively easy if there is a history of prior attacks of right-sided pain, but in many cases not even this is present, and one is called in to find the patient taken suddenly ill with abdominal pain and vomiting, rapid pulse and temperature of 101 or 102, which, with the dusky color and anxious expression of the face, and the general rigidity of the abdominal muscles, make up a picture characteristic of acute infective peritonitis. In such cases there must be the least possible delay. Even if the diagnosis cannot be clearly made as to whether the case is one of acute intestinal obstruction, or appendicitis, or other cause, the treatment must be the same—early and immediate laparotomy. The mortality in these cases is very high; but a surgeon's duty is to give the patient the "only chance," even at the risk of incurring the unpleasantness that "a death from operation" always brings. Certain death without operation, and a "possible" chance by operating, must be the prognosis

This outlook, coming suddenly on patient and friends, makes it difficult for them to realise fully the gravity of the case, and after the almost certainly fatal result of operation some are sure to cavil at the wisdom of the surgeon. A case in point came under my care a few months ago. I was called to see a lady in consultation with a brother practitioner. She was 29 years of age, and in perfect health until thirty-six hours previously. When out in the evening she was seized with abdominal pain. Vomiting shortly after began, and was incessant. She was seen early in the morning by her medical attendant, but she steadily became worse. She was seen by me at noon the following day, when she presented all the symptoms of acute general peritonitis. At the urgent request of her husband, I operated three hours later, when I

found the abdomen full of most offensive sanguineous fluid, and found also a perforated appendix. The patient only lived a few hours after the operation.

From a clinician's point of view, it matters little in these cases whether the infection be due to streptococci, staphylococci, or the colon bacillus; early diagnosis and prompt operation is the only possible hope, and this hope should, even in cases appearing almost desperate, be given to the patient, if consent be obtained. Irrigation with hot saline solution appears often to rally the patient, and gets rid of the offensive fluid more speedily than dry swabbing. I cannot see that the former method is more likely than the latter to harm the surface of the peritoneum, and so open up new avenues for infection. I have not tried continued irrigation after laparotomy, as recommended by La Place, but should be inclined to give the method a trial in a desperate case.

The after treatment of these cases hardly comes within the scope of this paper, for it falls under the heading of the general treatment of post-operative laparotomy.

As one writer remarks, the surgeon must (in these cases) always remember that even if he can save one life in fifty it is worth while making the attempt.

The method employed in making the incision is a matter of individual choice. The ideal method is the "gridiron" incision—there is less risk of subsequent hernial trouble; but I never hesitate to divide muscular fibres if desirous of more room, and if thorough technique is carried out and carefully applied, and buried sutures are introduced, the scar appears to be as firm in one case as the other.

I have not touched on the various methods adopted for amputating the appendix, for, like the incision, this is purely a matter of choice. The American method most adopted consists in ligaturing the appendix, and then inverting the stump into the cæcum, burying it there by a purse-string suture reinforced, if necessary, by a second row of Lembert sutures. Personally I have never found any trouble result from the amputated stump left in the abdomen after touching it with pure carbolic. The advice given by Treves is all-important—that the amputation must be made as close to the orifice as possible, so as to leave no cul-de-sac that may form a focus for future infection.

To sum up. Though many causes contribute toward the production of appendicitis, one of the most frequent is due to an aberration of the function of the appendix brought about by continued abuse of ordinary conditions of digestion. Therefore it is the duty of medical men to warn patients, and especially parents, to take all possible prophylactic precautions to ensure avoidance of attacks. When an attack comes on, it may assume a mild non-infective course which may pass off with palliative treatment, and give no further trouble, or may, either during this or subsequent attacks, pass into the more serious stage of suppuration. When it reaches this stage, either the condition known as suppurative appendicitis may be present, where the abscess forms slowly, owing to perforation being, for the most part, in the meso-appendix; or the fulminating attack known as acute perforative appendicitis may present itself, due to an overwhelming attack of microbic invasion and the bursting of the appendix into the general peritoneal cavity before there is time or (from the position of the appendix) opportunity to shut off the invading infection from the peritoneal cavity. But the treatment of these cases is the same, viz., *immediate operation.*

The ethics, as between physician and surgeon, in these cases may be summed up shortly. In purely catarrhal cases either physician or surgeon may treat the patient; but in suppurative and acute perforative appendicitis, the physician *never,* the surgeon *ever.*

SOME OBSERVATIONS ON APPENDICITIS.

By R. Steer Bowker, M.R.C.S., &c.

Mr. President and Gentlemen—When I started to write this paper I thought I would be able to put all I had to say on a few pages, but by the time I had finished it I found that it was of such length that I could hardly expect you to hear me out, more especially as there are others to be heard, and the time at our disposal is not great, so I have asked to be allowed to read an epitome of what I have to say, and will mention the points I wish to bring before you without going further into detail than is necessary, and will only quote my authorities without giving their arguments.

My excuse for bringing so well discussed a subject before you is that there are so many erroneous views enunciated concerning it, such deep interest taken in it by the laity, and so many stupid and misleading statements made about it by the public press.

One constantly hears the question asked, in a more or less scoffing manner: "Why is appendicitis so common now?" And it is freely hinted, even by some members of our own profession, that appendix operations are too frequently and unnecessarily done. Gentlemen, the error is on the other side: even now, with our present knowledge of the disease, delays are too frequent, and operations too often deferred.

Causes.—Foreign Bodies.—The general public, and even many medical men, have been accustomed to look upon fruit seeds and other foreign bodies as the chief cause of appendicitis, whereas it is a very rare thing to find any such in an appendix. It is an accident when any such body gets into an appendix, and due to some accident if the appendix does not, by its peristaltic action, get rid of it again.

Anatomy.—With regard to the anatomy, I will not take up your time, except to ask you to remember that the blood supply is a poor one: that, in the ultimate distribution, the vessels ramify between the mucous and submucous coats, surrounded by a fairly dense muscular layer, and that the organ is one which nature, in her economy, is endeavoring to rid herself of: also the longitudinal muscular bands (or tæniæ) on the cæcum leading to the appendix, and the fact that, at the cæcal orifice of the appendix, the mucous membrane is thrown into folds: one crescentic in shape—just above it—is known as the valve of Gerlach, and another on the opposite side, but within the lumen, is that of Nanninga, more marked—the acuter the angle at which the appendix comes off from the cæcum, and that on distension of the cæcum these folds or valves are liable to close the orifice (Kelly, 1); that the nerve supply is derived from the superior mesenteric plexus of the sympathetic, which supplies also the small intestines, and to this fact is attributed the wide distribution of pain in early appendicitis (G. R. Fowler, 2); and that the veins empty into the superior mesenteric and on into the portal. Its lumen is narrow, the adenoid tissue encroaching on it, any catarrhal condition liable to still further lessen its calibre. Ribbut, Kelly, and, more recently, Woods-Hutchinson (3), have described how nature, in trying to destroy the organ, does so by obliterating the lumen—often only in parts, and so leaves strictures; and that the lumen is the habitat of various micro-organisms which may, on slight provocation, become virulent.

Definition.—What is appendicitis? It is as well to give a definition before going into the causes; and, for this purpose, I cannot do better than quote R. T. Morris (4). He says:—"Appendicitis is an infective exudative inflammation of the vermiform appendix of the cæcum, originating in any local cause for the production of an infective 'atrium,' or gateway in the

tissues of the appendix, and progressing by bacterial invasion into the layers of connective tissue and the layer of lymphoid tissue, all of which are partially or completely disabled by interstitial exudate compression within the narrow muscular and peritoneal sheath of the appendix. The principal cause of appendicitis is mixed bacterial infection from the lumen of the appendix: the chief cause of bacterial infection from the lumen of the appendix is the formation of an 'infective atrium' in the *mucosa*, by force applied in any way."

So that appendicitis is the result of an "infective atrium" in the mucous lining of the appendix, the result of any cause.

Now, Van Zwallenburg (5) shows that the chief cause of infective atria is the cutting off of the blood supply; that the chief cause of the cutting off of the blood supply of the appendix is distention of its lumen, causing pressure on the vessels in the submucous coat, compressing them against the unyielding outer walls, de-vitalising the mucous membrane and allowing it to be attacked by the contained pathogenic organisms; that distention of its lumen is caused by any obstruction to its outlet, a constriction more or less narrow, with a contained concretion of some sort—not necessarily a hard body, a little fæcal matter (or even a plug of mucus) may suffice to plug the opening. Given these conditions, an excess of fluid may be forced into the appendix by some muscular effort past the obstruction, and, by its recoil, forces the obstructing material (whatever it may be) against the mouth of the stricture, blocking the opening, and so acting as a ball-valve; distention takes place. So that it may be said that anything which interferes with the blood supply is a direct cause of appendicitis. Given a good blood supply, the bacteria are inert; de-vitalise the tissues, and immediately the bacteria attack them. This supply may be cut off from the base by a twist, twisting the artery, by pressure of some other part, &c., or an obliterative endarteritis starting peripherally may occlude the main vessel, and cause sloughing of the entire appendix. But no doubt the description given by Van Zwallenburg (5) is the most common cause; and I will now trace the various factors which lead to that condition.

First and foremost, we have some former slight attack recovered from, the result of injury, typhoid ulceration, or in fact anything causing an abrasion of the mucous membrane, which has healed, with the formation of connective tissue, in which contraction has taken place, leaving a narrowing or stricture of the tube; or this may be caused in the process of natural obliteration which goes on from birth until death, causing destruction of the lymphoid tissue and connective tissue replacement—or even a thickening of the lymphoid tissue, or an increase in it, may block the lumen, or some peri-appendicular peritoneal adhesion may cause a kink in the appendix, and so arrive at the same result.

Now for the *causæ causarum*. The causes of stricture have been mentioned.

Lymphoid tissue in some people is excessive.

Catarrhal condition may be caused by any intestinal irritation, typhoid fever, influenza, some of the exanthemata—any hereditary malformation of the appendix, narrowing its lumen or distorting its shape.

Kinks caused by peritoneal adhesions from inflammation of the other organs. Kinks caused by peri-appendicular peritonitis, the result of intra-abdominal muscular traumatism, as described by Byron Robinson (6)—chiefly by the action of the psoas. Distention of the cæcum, the result of flatulence, may cause obstruction; and, if the appendix is already a maimed one, may precipitate an acute attack—and, to my mind, at any rate, accounts for those attacks of colic or appendicitis attributed to the eating of green fruit or some indigestible food, but not on the grounds stated by Rubin (7), of

Rush Medical College, who attributes appendicitis to our civilisation causing man to retain gas in the large intestine for long periods of time; that he is most of his time in places and under conditions which render it undesirable that he should rid himself of it; that this gas distends the cæcum and appendical opening, and allows foreign bodies to get in—attributes to this the fact that savage people seldom suffer from the disease, and quotes Bland Sutton and J. H. Campbell, who have made a study of the appendix in anthropoid apes, as saying that they never came across an inflamed appendix in these animals, and in proof of his contention gives laboratory experiments which he conducted; but forgets that these were on a cadaver with no blood supply to the mucous membrane, and consequently no turgid valve of Gerlach or Nanninga to shut off the lumen: besides which, Wienberg (8), in the annals of the Pasteur Institute, relates a case of appendicitis in a chimpanzee. Distention of the cæcum is controlled, to a certain extent, by the muscular bands and the abdominal wall, especially behind, and so distends in a lateral direction more than antero-posteriorly, and approximates the edges of the opening; and where the appendix comes off from the cæcum at less than a right angle —as it generally does—the valves of mucous membrane very efficiently close it.

Now, to follow up the theory of Van Zwallenburg (5), some extra strain—a bicycle ride, some athletic exercise, or even a change of the position of the body during sleep—may set up this ball-valve action in the appendix, by causing an excess of fluid to distend it; peristaltic action is set up, which may overcome the obstruction, or may not—if not, the appendix still tries to get rid of the obstruction, and painful contractions of the organ take place, giving rise to "colic" (just in the same manner as we get hepatic colic). The obstruction, by the excessive effort of the peristalsis, may yet be overcome, and the attack end in colic only, for the time being; or the obstruction may persist, more fluid be exuded from the walls of the appendix, and exudation take place into its tissues, rendering it still more tense: distension increases until circulation is arrested, pain diminishes by reason of paralysis of the nerve endings, and perforation or gangrene takes place. Now to interpret these signs: when the pain lasts one or two hours, with sudden cessation, then the obstruction has given way. If there is no tenderness left, the attack has passed and left no material further damage; if tenderness is left, then some inflammation has been caused; if the pain lasts several hours, with vomiting with abrupt cessation, perforation has taken place; if the pain gradually diminishes after six hours to twelve hours, gangrene has resulted; and if it reappear after twenty-four hours, peritonitis has commenced. This account very accurately describes an ordinary acute attack; but there are other factors at work, which will have to be considered, causing the disease to run different courses; some to be acute or fulminating, some to run a chronic course, some to end in resolution, and others to go on to suppuration, some to be localised, others to cause general peritonitis, pyelophlebitis and portal pyæmia, vomito-negro, empyæma, or acute toxæmia.

The nature of the attack will depend on (1) the position of the appendix; (2) the nature of the micro-organism set free.

There are two chief positions of the appendix governing the nature of the attack—*Colonic* and *Enteronic* (Byron Robinson) (6)—*Colonic*, or benign area of peritonitis, being the outer side of a line drawn from the surface of the liver along the psoas muscle over the right pelvic brim to the pelvic floor: *enteronic* area, to the inner side of this line towards the centre of the abdominal cavity; an appendix pointing here will be amongst the enteronic coils, where the stomata vera of the peritoneum are vast in numbers, whilst in the colonic area they are limited.

The enteronic area is the area of absorption (septic). Perforation in the enteronic area is liable to be followed by absorption and acute toxæmia before a limiting membrane can be thrown out. Whilst in the colonic area, the peritoneum has more time to throw up a defensive barrier, absorption being slower. The most constant micro-organism in the appendix is the B.C.C. Bacillus pyocyaneus is occasionally found, and streptococci more frequently.

All authorities are fairly unanimous in that the nature of the infection depends on the virulence of the organism which predominates, that the infection is generally a mixed one, that the B.C.C. is the only organism ever found alone, and that the streptococcus pyogenes leads to the most severe infection.

Though constipation, *per se*, has little to do with the causation of appendicitis, it has, I think, a deal to do with the numbers of the bacilli, for it would cause stagnation and favor the multiplication of germs, giving them a quiet corner to breed, surrounded by heat, moisture, and food.

An infection caused by the B.C.C. alone is generally of a mild form, the resulting peritonitis more limited and less severe, with the production of pus of a characteristic fœtid odor: whereas in a streptococcal infection in the enteronic area, highly virulent toxines are thrown into an area where absorption is very rapid, and the probability of death is very great. The question is not as to whether complete gangrene or perforation has taken place, for probably, as it would take longer for the whole appendix to become gangrenous than it would for it to perforate, the omentum would have a longer notice of impending disaster, and so have a better chance of coming to the rescue.

I do not think the nature of the micro-organism has anything to do with whether gangrene or perforation shall result; that is determined by three other factors—(1) nature of obstruction; (2) position of appendix; (3) number of former slight attacks.

Where the obstruction is caused by a hard concretion, and the distention and œdema is cutting off the blood supply, the organ will give way first where the pressure is greatest, viz., at the site of the stercolith; perforation will result, relief from tension will take place, and gangrene be averted. Again, where there have been many attacks of peri-appendicular peritonitis, causing the appendix to be buried in adhesions, sloughing seldom takes place on account of the greatly increased blood supply—more particularly where the appendix is attached to muscles—to the outer side of the colon. I have called this the region of "chronic or constant appendicitis," as distinguished from "recurrent appendicitis"; a sloughing appendix is generally found free in an abscess cavity. Complications from this class of cases arise from (1) absorption of pyogenic organisms into the circulation; (2) displacement and migration of venous thrombi; (3) infection through the medium of lymph channels; (4) direct extension of abscess.

There is another class of case where only slight symptoms are followed by an acute attack. Lejar and Menetrier (9) describe, in the *Revue de Chirurgie*, various diverticulæ of the appendix—small outgrowths from the lumen, having thin and easily ruptured walls, connected with the lumen by a small neck, the result of some abscess in the appendiceal wall; an adhesion between one of these and a muscle, or some other organ, could be easily ruptured by any exercise or strain, and set up an immediate and acute attack, and no doubt account for some of the attacks following immediately on exertion. These diverticulæ are prone to take place at the site of the meso-appendix, and may account for some cases of thrombo-phlebitis.

Recurrent Appendicitis I class those sub-acute attacks, either of catarrhal appendicitis or of obstructive appendicitis, from which a patient recovers and which recur from time to time until an attack with perforation occurs.

Then there is another class of case of which I wish to speak, more common than is generally thought, and which I divide into two groups :—(1) Chronic, or constant appendicitis, the source of definite trouble and easy of diagnosis (as a rule); (2) unrecognised appendicitis, causing general ill-health with but vague symptoms, in which the diagnosis is difficult and conjectural. I think, in the latter group, there are more cases than in any other class.

Now to consider the milder attacks of the disease which prepare the way for perforation, or for chronic appendicitis and perpetual ill-health.

Appendicular colic will be severe, or otherwise, according to the difficulty the appendix experiences in ridding itself of the obstructions. This having been accomplished, the attack will subside. If there remains no inflammatory trouble there will remain no tenderness on pressure shortly after the attack, but if there remains for a couple of days tenderness on pressure, then there has been some damage done—a small infective atrium formed, probably, or some exudation into the tissue of the appendix, which will almost certainly lead to future trouble; so that the fact as to whether, after an attack of colic, there is or is not a resulting tenderness becomes a most important sign, and should guide the surgeon or physician in the advice given to his patient.

In the case of a smooth foreign body (viz., one which, by its sharp edges, does not perforate the appendix) its position in the lumen will have a great deal to do with the result; if it gets to the distal end of the appendix it will probably become fixed there, as peristalsis will not have much power over it, and the appendix will, after a time, cease to try to get rid of it; mucin and salts will be deposited on it, and it will in time become a veritable stone, and others may form above it: and if the appendix is situated to the outer side of the cæcum, as in these cases it generally is, their presence sets up a mild irritation, and peri-appendicular peritonitis—which will cause it to become adherent to the tissues around, probably the iliacus (or the sequence may be reversed and fixation of the appendix by peritonitis, the result of traumatism of the iliacus, be the first step), a chronic irritation will be set up, with but little pain, only occasional aching, interfering with the action of the intestine and nerves of the part, causing indigestion, flatulence, constipation, &c., without any very definite symptoms—only occasionally vague discomfort: the person is never very ill and never well, often only " that tired feeling " of which so much is made by the quacks. These appendices never slough and seldom perforate, on account of the extra blood supply that they receive from their situation and condition. They are generally large appendices with their mucous membrane more or less destroyed. They can generally be diagnosed—if thought of—and carefully searched for.

By *Unrecognised Appendicitis* I mean that class of case written about by Gibbons (10) in the journal of the American Medical Association, chiefly in children, and more recently I think by Fowler (11) in adults.

Gibbons says :—" Ailments referred to the intestinal tract in infancy and childhood, and misinterpreted as 'gastric difficulties,' 'cholera infantum,' 'colic,' 'gastro-intestinal,' 'intestinal and nervous dyspepsia,' 'liver ailment,' 'constipation,' or even diarrhœa—such 'difficulties,' 'disturbances,' and 'ailments' are nearly, if not always, due to a diseased appendix." He says :—" Watch carefully children who are pale at certain times without cause, who are yellow or of an earthy color, especially those who vomit easily without cause, or after driving or walking." There are also cases, in adults, in which it is difficult to realise that the appendix is the source of their trouble. My attention was first called to them by a couple of cases I saw in consultation with the late Dr. Angel Money, a few years ago, in which the operation proved the diagnosis and cured the patients. These patients are always in ill-health, though never very ill—constantly complaining—and have consulted in-

numerable medical men, and have taken almost all known medicines, and gone through a course of what Gibbons calls "dietary juggling," and are told eventually that their trouble is "nervous," or the result of "excessive uric acid," and know all about "purins," &c.

Bearing this in mind one must look with suspicion on such a case—complaining of dyspepsia, flatulence, and constipation; no signs to lead one to a definite diagnosis, never any marked pain, perhaps an occasional attack of colic, forgotten until inquiry is made; no tenderness on pressure anywhere, but probably having a dilated cæcum. This condition of cæcum, together with a flabby whitish tongue, is often the only sign of chronic appendicitis; and if careful palpation of the appendiceal region is made some tenderness may be found, enough to warrant the probable diagnosis. Remembering that the appendix in these cases is generally to the outer side of the cæcum, and pretty firmly fixed, probably more or less obliterated and containing calculi, and interfering with the nerve supply to the cæcum by irritating the peripheral nerves—never, or rarely, ending in an acute attack, but always remaining a source of chronic intestinal irritation.

The symptoms of an acute or sub-acute attack are so well known that I need hardly go into that subject, except to mention that the pain may be complained of at the onset almost anywhere over the abdominal wall; but, as a rule, tenderness on pressure will generally be found over the seat of the appendix, or the pain will generally eventually settle there: though, in a case I had recently, the pain was complained of over McBurney's point, and there was resistance and tenderness there, but, on making my usual incision in the iliac region, only coils of small gut were found and no cæcum, but I could feel the cæcum higher up in the region of the kidney, and I removed through an oblique incision (as for a kidney operation) a large appendix on the point of rupture—a case of non-descent of the cæcum, the appendix being behind and in the kidney pouch. Another point to be emphasized is that during even a moderately mild attack the cessation of pain—the temperature and pulse dropping or remaining throughout but little above normal—may not mean that the appendix is well, but that it is dead; a fact I could illustrate by many cases had I the time. This is a condition which has lulled many into a false sense of security, with disastrous consequences.

"Why *is* appendicitis *so* common now?" It is due no doubt, in great measure, to more accurate diagnosis, and so the increase is more apparent than real; but there is no doubt that since 1889 there has also been an absolute increase in the number of cases, and Marvel (12), in a very able article, puts this down to what I consider the correct cause—a conclusion I had come to before reading his paper. He attributes this increase to the recent epidemics of influenza, which began in that year, and quotes the statistics of three large American hospitals for the last three five-year periods, also Continental opinions, among which are Winternitz, Pever, Faisans, and Sonnenberg, showing that the Pfieffer bacillus has been found in the appendix, in support of his views; and there can be no doubt that influenza, more especially the intestinal type, is responsible for many cases of appendicitis,

Now, as to treatment of these treacherous cases. All up-to-date physicians and surgeons the world over are in accord in thinking that all pus cases should be operated upon at once—except, perhaps, those cases in which the dose of poison absorbed seems too great to allow of the patient rallying from an operation; also, that if a patient can be tided over the acute attack the best time to operate is during an interval. But can we always tell when pus has formed, or shall we diagnose the formation of pus only when pyelo-phlebitis, or portal pyæmia, is setting in? For the signs by which we have been supposed to diagnose pus formation are more usually the signs of commencing

toxæmia, and the case is probably then too far gone for operation to save it. Then, again, we are taking a very great risk—an unjustifiable risk—in the present state of our knowledge, in trying to tide a patient over the acute attack when we see it at the beginning of such an attack. How can anyone tell what is going to be the ending of even an apparently mild attack ? Why wait for the interval when that waiting subjects the patient to absolutely unknown risks, of which the physician can judge no better than can the patient ?

People are so educated to the dangers of appendicitis—each individual almost in the community knows of friends lost by waiting, though they may not realise that the waiting was more the cause of death than the disease—that when they know the name of the disease from which they are suffering they are prepared, in most cases, at once for operation, and only wait for their physician to suggest it for them to agree to it. The patient puts his trust in the judgment of his medical man more in these cases, perhaps, than in any other, and he is indeed a very foolish or very unscrupulous man who unaided will take upon his shoulders the unknown risk of "tiding a patient over" an acute attack to the interval, when he can obtain the services of a competent surgeon.

Now, what have we on the other side ? If the appendix has perforated, then it is admitted an operation is urgently needed ; if it has not perforated, and the trouble is still yet within the appendix, then, in the hands of a surgeon used to the cases, an operation is almost devoid of risk and should not be dreaded, and would in most cases—if both sides of the question were candidly put to the patient—be eagerly asked for, and free the medical man from an amount of responsibility that he has no right to assume.

It is not even as though a patient, having faced the unknown risks and come haply through with his life, is any better off than he was before ; he still, in the great majority of cases, cannot avoid an ultimate operation, for no one can tell which individual case will be one of the lucky few which will get no further attacks ; for there can be no argument against the fact that an appendix which has once been attacked—even mildly—is, by that attack, rendered more prone to future attacks ; one attack does not give immunity, but the reverse. Why give a restive horse a chance of bolting ? Besides which, an appendix at the commencement of an attack is in a better position for operation than one recovering from an attack, and the operation much more simple, for later there will be adhesions, &c., to complicate the case.

In support of these views I cannot do better than quote Barrett (13), of Chicago, who expresses himself most happily. He takes for his text an explanation a surgeon so often hears—or, I am glad to say, "used to hear"—"I was treating the case medically until I could detect pus," and says, "There is no greater fallacy than the idea that an unruptured appendicitis deserves medical treatment, but that the advent of pus outside the appendix is the indication for the insistence of an operation. A physician harboring this opinion—and there are not a few—should remember that so great an authority as Osler has said, 'There is no medical treatment of appendicitis.' Knowing the virulence of appendiceal infection, its tendency to break through the wall of the appendix, and occasion a fatal peritonitis, the man who deliberately sits waiting for pus and peritonitis can only be likened to a fireman who would stand, hose in hand, and contemplate a beginning conflagration in your house, and withold the stream for fear of soiling the rugs." He continues, "The much talked of *interval operation* is a wise thing to advocate for a patient *during the interval*, but it is sometimes a most dangerous thing to advocate and wait for *during an attack ;* any treatment that goes sparring for a diagnosis during the first thirty-six hours, and manœuvring for the advantages of an

interval operation during the next few days, and then closes for a life or death struggle on the fifth or sixth day, will largely increase the death rate. There *is* an interval—but not the one so often alluded to—that should interest the patient and the physician; this is the interval between the onset of infection in the appendix and the time when a dangerous amount of the infection would be found outside the appendix if it were not removed. The interval between pus in the appendix and pus everywhere. This is the interval which, had a surgeon's knife cut short the possibility of pus everywhere, would have saved the lives of most of these patients."

"A patient going through an attack (suppurative) without operation is saved by adhesions—adhesions are life-saving at the time, but death-dealing afterwards. Waiting treatment favors adhesions; early operation avoids them."

I think, therefore, that no one should argue against the early operation, *before* serious trouble has set in. The risk of operation at that time is so little, the wound need be so small, the convalescence so speedy, the chance of post-operative hernia so slight—in fact, the operation which saves is less painful, less irksome to the patient, than the attack which may kill. It all hinges on the diagnosis; if the case is diagnosed early, then operate at once. Of course if an error has been made, and a correct diagnosis not arrived at until the disease has ceased to be limited by the appendiceal walls, then no hard-and-fast rule can be laid down for guidance; but it must be considered a misfortune that the case has not come for operation sooner. This is where judgment and experience will tell, for it is on these that our decision as to operation must now rest. When an acute case comes first under notice, the attack having lasted a day or two, at the Sydney Hospital, the case is placed at once in a surgical ward, and if it is manifestly getting over the attack operation is not postponed, but delayed from hour to hour, blood counts taken, and convalescence from that attack hoped for, and then the operation done. On the other hand, operation is at once done if thought advisable; but one has to be very sure that the improvement is absolute, and not only "a lull before the storm."

Now come another class of cases—those that are no longer cases of appendicitis, or inflammation of the appendix, but have gone on to peritonitis or toxæmia. Mere removal of the cause will now do no good, and one has to judge of the amount of poison absorbed—that is, of the result of the poison on the system of that individual patient—and this depends on the virulence of the germ let loose, and on the power of that patient of withstanding the poison. In other words, will the patient stand the anæsthetic and operation on the top of his toxæmia, for in even the worst cases it is better to open and drain, if possible, the region where the poison is manufactured; but, in those cases where our judgment tells us that the patient will not stand the operation on top of his other troubles, then we must rely, not on medical treatment, but on "masterly inactivity"—the method of Ochsner being the best to follow—in the hope that the patient will overcome the toxæmia and allow of later operation.

Ochsner himself has not proposed his method as one of treatment of appendicitis—but, unfortunately, some medical men have allowed themselves to be misled into looking upon it as such—but only as aiding a desperate case in their fight with toxæmia, the result of neglected appendicitis. The method consists of keeping the patient at absolute rest, giving nothing by the mouth and little by the rectum, so as to check as much as possible the movements of the intestines; if necessary, washing out the stomach to check vomiting; in fact, the method consists of rest absolute, but to drain the part is the first essential in every case where it is practicable.

Now, as to the removal of the appendix in pus cases. I think this should be done in every case, if possible, without adding too much to the risk or length of operation, and it is generally possible to a surgeon experienced in these cases. I know that such an authority as Treves has stated that after a severe suppuration most cases never have further attacks, but I think he goes rather far in that statement. Only the cases which result in sloughing of the entire appendix are likely to be free from further attacks. Those caused by a perforated appendix are more likely than ever to have further attacks. I have a specimen removed at a second operation. There was an acute abscess well walled off: I could not feel the appendix, so left it; subsequently I removed in one piece the old cicatrix with the appendix attached by its middle, the free end pointing to the enteronic area—distended, and looking as though it were again on the point of rupture. I was astonished to find that, after the lapse of nine months, all or most of the adhesions had become absorbed.

There is no class of case in which the experience and practice of an operator are more called upon than in this, so that in recommending early and frequent operation I only do so when it is possible to secure the services of a trained operator; for, treacherous as the disease is, I am not sure it would not be safer to trust to it rather than to the inexperienced judgment and surgical indiscretions of some self-appointed operator.

If we are to press early operation upon our patients, we must be able to tell them that in all probability the shock of the operation will be slight, that they will not be laid up in bed long (probably not as long as by waiting for the interval), that they will not be disfigured by a great and unsightly scar, with the tendency ever afterwards to a hernial protrusion and the necessity of wearing an irksome abdominal support. If we are, by our operation, to leave the belly wall so maimed that a hernia is a likely result, then it is questionable if we should ever advocate an operation except when our hand is forced; but it is quite an exception that a long incision is necessary, or even desirable. Of course the first requirement of an operation is to be life-saving, but if we can achieve that object and be artistic as well, then so much the better. I will go farther, and say that with a short incision we can do better work than with a larger one, when that incision is placed in the right position. I seldom fail to find and remove the appendix (I may say I have never failed except where I have thought it undesirable to try) with the least amount of shock and disturbance of the intestines, a speedy convalescence, and no tendency to hernia.

When I first began to use the one and a half inch incision I followed R. T. Morris, of New York, but since becoming more experienced I have modified somewhat his method. I have made the short gridiron incision in every case I have operated upon since 1900, whether acute or chronic, suppurating or not; and at times, to show that it was possible, I have made the incision considerably under one and a half inches. And my statistics are good; for though holding an appointment at a large hospital where we get all sorts of cases, I have not lost a case for three and a half years, and only one in private for a longer period than that: and I attribute this to the slight amount of traumatism necessary. With experience, the sense of touch becomes more accurate and more useful than the sense of sight, the appendix being more readily recognised, more easily freed, less damage done to surrounding parts, than when a full view is necessary to do the work. Howard Kelly quotes an operation of Professor Dandridge (14) of Cincinnati. He could feel a mass towards the middle line—cut over it, at the right border of the rectus, freed the mass, but before he could remove it had to tie a somewhat large vessel; the mass proved to consist of enlarged mesenteric glands (inflammatory), but

he could not find the appendix ; afterwards discovered it to the outer side of the colon. The wound healed, and then broke down, and about 15 inches of small intestine and cæcum sloughed away, leaving a fæcal fistula (which took four serious operations to close). He had tied off the blood supply of part of the ileum and cæcum by means of this sense of sight ; whereas, had he depended more on his sense of touch, and started from the right place, he would at once have found and removed the appendix, and left the mass to the mercy of time and the peritoneum, and have avoided the disaster.

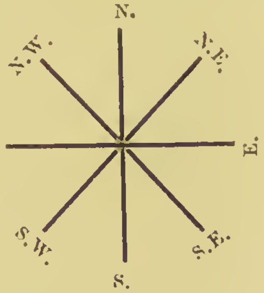
Of course this is an extreme case ; but I quote it as showing the importance of *not* cutting over an inflammatory mass (for, never mind how large the incision, it is the way to find trouble), but beginning the search, not from where the appendix *probably ends*, but from where it *more than probably begins ;* for the inflammatory mass is of little import, it is the cause of the mass which is our chief concern—remove that, and the peritoneum will attend to the inflammatory mass, whether it be composed of inflamed glands or thickened omentum. I have demonstrated this on many occasions. To cut down near the mid line, in the hopes of coming across the unknown position of the distal end of the appendix, even though there may be a mass, is a somewhat hazardous proceeding when one can keep to the colonic side of trouble and probably with ease find the proximal end, with a certainty of finding the cæcum, with its tæniæ waiting to act as a guide. It savors somewhat of setting a " black tracker " to pick up the tracks of an escaped criminal anywhere he can, rather than letting him trace them from the place where he was known to have been ; besides, who would engage in a frontal attack when he can take the enemy from the rear ?

The commencement of trouble is generally some little distance from the base of the appendix, and we should begin from there, where the parts are probably healthy. By using this method I have often dug an appendix out of the midst of an inflamed mass, put a drainage tube into the cavity I had left, and in a few days the mass had disappeared—and this with the smallest of incisions. This cannot be done by sight ; it has to be done by touch. To use sight, a very large incision would have to be made, and probably the task given up ; whilst, by keeping in touch with the appendix, it can be stripped out, or the mucous lining brought away, leaving the outer coats, but generally the whole organ can be brought away without disturbing the outer walls. If there is pus in the cavity, this is even more easy and quite safe, as one works only in the shut-off cavity, with the healthy intestines packed off with gauze. If the attack is fairly recent there is a natural cleavage line, which gives way easily. Experience has taught me that bleeding need not be feared, as we can tie the principal appendical vessel at the base, and if there is any oozing, can pack gauze around the drainage tube and so control it.

If the operator starts over the mass he will probably have to open up to the inner or enteronic side, with the added danger of contaminating this region, and when he has " come on " the mass he has then to open it up from above ; he does not know what he is going through, in spite of his vision (perhaps the ileo-colic artery), and he has a large incision in the dangerous area crammed full of sponges, pads, and other paraphernalia. He says he wants lots of room —as it is safe—and at once proceeds to minimise his room by stuffing his wound chock full of pads, &c. ; and, perhaps, after pulling his patient about for an hour, scrubbing the tender surfaces by ever-changing pads, finds that his appendix is not there, and is lucky if he has only to give up the search without having done any irretrievable damage. What I want to emphasize is, that it is not necessary to remove or open up the inflammatory mass, but the cause of that mass—leave the house untenanted, and nature will demolish it. Of

course if it were necessary, or even desirable, to remove all the inflamed surroundings of an appendix, then a large incision would become necessary, and no one could do the operation without it; but, to remove the appendix, it is not necessary, and it is astonishing how much can be seen through a small opening if desired. In ordinary cases, when the cæcum is drawn up, it acts as a plug to the wound, and there is seldom occasion to use even one small pad; the operation often is an extra-peritoneal one, with the exposure of a piece of the gut the size of half a crown.

I have seen some appendix operations done when it was hard to realise, except for the pads and blood, that it was not a *post-mortem* examination of the abdominal contents that was in progress; and have often wondered how much pulling about the human body *would* stand, and whether it was advisable to operate quickly, and whether the peritoneum did not improve by being scrubbed by the pushing in and pulling out of innumerable pads, and whether long continued æther administration did not improve the patient's vitality and kidney function. And yet people will argue in favor of big incisions. The operation I advocate is really the operation of R. T. Morris, of New York—a small incision, splitting the muscles, &c.; but I make the skin incision, not at McBurney's point, but further out—further out, I think, than any other surgeon. I start about half an inch above the level of the anterior superior spine of the ilium, and about 1 inch (in thin people, a little more) to the inner side, and directed *not* towards the rectus but almost parallel with its border, towards Poupart's ligament. This I have found the best place to make the incision, and, in ordinary cases, one will at once come on the appendix; if one does not, the cæcum at least will appear, and the tæniæ act as a guide; if we have to search for the appendix, the nature of the attack will aid us somewhat as to the location of it when the base is hidden, as it sometimes is. Guided by this information, I feel for it first in one direction, then in another. If the attack is an acute or sub-acute one, it is probably situated in the S.E. direction, less often N.E., and least often E. If the attack is a chronic one, it is probably S.W. or N.W., going by the compass on the descriptive plan suggested by G. R. Fowler some years ago: taking the points of the compass and fixing the N. upwards, S. downwards, E. towards the middle line of the body, and W. pointing outwards. But generally we can fix the base and feel for the tip, which can generally be felt and recognised by its thickened and bulbous extremity, and can be freed and hooked up to the surface with the finger; if too fixed for that procedure, it can usually be dug out from its bed; if surrounded by omentum, the whole mass can be often coaxed out of the wound: in these cases the appendix is generally pointing E., is a perforating or perforated one, and superficial. I wall off the enteronic area with gauze, shell out the appendix, and ligature off the omentum. If there is pus, I leave the gauze and put in a drainage tube. Sometimes the appendix is buried in the wall of the gut, or in the muscles to the outer side, or behind the cæcum, the cæcum having also become attached, hiding the base of the organ; in these cases sight or the size of the incision will not help one in the least (though the position of it will), and it can only be found by the sense of touch. In these cases I do not try to see. There are cases in which, though we locate the organ, it seems next to impossible to remove it; but, by perseverance and the aid of finger nails, we can generally get it either with or without its external coats; in fact, I have yet to come across a case where I have located and tried to get an appendix that I have failed.



In cases with acute abscess, if I feel and recognise the appendix, I remove it, as I think it better always to do this where one can ; but in cases of acute abscess, where I cannot feel the organ, I do not search too long, and hope that it has been a case of gangrene, and that it may have become destroyed. I simply swab the cavity well out and drain, making a counter-opening, if I think it advisable ; an abscess containing a sloughing appendix is generally walled off. If I could locate the base and could not remove the appendix, I should be inclined (though I have never had such a case) to tie the vessel at the cæcal angle, and ligate the appendix, and leave it in the hopes that it would slough.

In acute fulminating cases the appendix is usually easily found and removed ; if there is general peritonitis I make free counter-openings, and put in drainage tubes in the loins and above the pubes, but seldom wash out. Practice has taught me, purely on empirical grounds, not to do so, except in those cases without any limiting membrane and no adhesions, with a lot of thin fluid, like tea, with flakes of cream in it.

Dudgeon and Sargeant say, in their Erasmus Wilson lecture on the "Bacteriology of Peritonitis," that the surgeon has friends as well as foes amongst the bacteria (and so give scientific reasons for facts arrived at by experience), that the first bacterium to make its appearance in irritation of the gut is the staphylococcus albus ; it is a bacterium of but little virulence, and its *role* is to call to the part many phagocytic cells which are able to overpower the Bacillus Communis Coli, which appears a little later on the scene, when the gut has become more damaged. To wash out, besides washing out also the defenders, may mean washing the Bacillus Communis Coli to healthy parts not prepared by the staphylococcus albus for their reception ; and, therefore, to wash out is a bad policy. But that the phagocytes have no power over the more virulent organisms, such as the streptococcus pyogenes, or the bacillus pyocyaneus, so that in infections where these organisms predominate the only chance lies in a thorough washing out in the hopes of getting rid of them.

They also describe as harmful the practice of peeling off lymph covering the bowels, so damaging the underlying endothælium and leading to the formation of adhesions.

Conclusions.

Foreign bodies are not often the cause of appendicitis.

Factors causing irritation of the mucous lining of the intestines are frequent causes.

Pain generally commences at or near the navel, but may start anywhere in the belly.

Any pain in the belly should have immediate and skilled attendance.

Pain continuing for six hours, and tending to settle in the right flank or groin, should call for immediate operation. Operation at this time is attended by little or no risk when compared with the risk of waiting.

Recovery from an attack does not mean recovery from the disease : it means only a respite ; each succeeding attack increases the danger and difficulties of operation.

Any attack may end in death, or in chronic ill-health.

Chronic appendicitis is very often the unsuspected cause of ill-health.

There is no medical treatment for appendicitis, any more than there is for a broken leg.

"Waiting" on the doctor's part, neglect on the patient's part, or occasional unavoidable error in diagnosis is almost the sole cause of death.

No one individual can expect (on the ordinary laws of chance) to be one of the very few who will get no further attack.

REFERENCES.
(1) Howard Kelly. "The Appendix and its Diseases," 1905.
(2) G. R. Fowler, "Annals of Surgery," 1894.
(3) Woods Hutchinson, "Appendicitis," as an incident of development, "American Medicine," 1903.
(4) R. T. Morris, "Lectures on Appendicitis," 1899.
(5) Van Zwallenburg, "Annals of Surgery," 1905.
(6) Byron Robinson, "The Appendix in its relation to the Psoas," "Annals of Surgery," 1901.
(7) Rubin, "Annals of Surgery."
(8) "Medical Record."
(9) "Medical Record."
(10) Gibbons, "Journal of American Medical Association," 1903.
(11) G. R. Fowler, "Medical Record." (?)
(12) Marvel, "Journal of the American Medical Association."
(13) Barrett, "Journal of the American Medical Association."
(14) Dandridge, "Annals of Surgery."

DISCUSSION ON APPENDICITIS.

DR. STEER BOWKER: I was very surprised to hear the statements made by Dr. Hamilton Russell.

To set out with the intention of removing the appendix in *all* suppurating cases was good; but there are cases where the wisest course is not to try to remove it, but simply to drain, and wait for a more favorable time. I am sure that to always remove it would, in some cases, turn the balance against the patient.

In certain cases the mere pulling about, and length of time necessary to do this, would destroy the patient's chance of life. I seldom have had to leave an appendix, and always feel much happier when it is removed; still I recognise the fact that there will be an occasional case where it is wiser not to try. My own practice is, when I can feel the appendix, to remove it; but do not waste time in hunting for it, as I believe speed in operating in these cases to be the chief factor governing the result.

With regard to his statements concerning the length of the wound, I can hardly think he meant what he said, for a huge incision is most undesirable, and never mind how accurately the incision is sutured, the drainage pack is not the only place where a hernia may result, for in a large incision nerves and vessels supplying the muscles are divided, and atrophy of the muscle prone to result in consequent hernia; besides which, every inch added to the incision renders more liable the formation of adhesion to the wound surface.

If the pus is thick, I may use a little more endeavor to get the appendix; but where it is thin and watery, I am more careful. If the appendix comes readily to hand and I can remove it without wasting time, I always do so.

CRITICISM ON PAPERS BY H. L. MAITLAND.

(1) *Appendicitis.*—The chief debatable points that have arisen in these very excellent papers on appendicitis are—(1) when to operate; (2) should attempts be made to remove the appendix in abscess cases; (3) the length and location of the abdominal incision.

First of all, when to operate.—I am of opinion that every case of appendicitis should be operated upon preferably in the interval.

If a case be seen in the first ten or twelve hours, then the appendix should be removed immediately, because the condition of the appendix and surrounding structures is not far removed from the normal; but quite another condition exists if the case is seen during the full blast of an acute attack. Then

the appendix and the surrounding structures are loaded with inflammatory products and bacteria, and operation is better postponed till after the acute attack is over.

Of course, if special indications arise pointing to the necessity of operation, then it should be done. If the pulse rate becomes high (120), or a rigor, or the patient look ill, or if there be any doubt as to whether the patient will recover from the attack or not, then it is better to operate. I object to a hard and fast rule of operating immediately in every case.

With regard to the removal of the appendix in abscess cases, I am firmly of opinion that if the appendix is easily found, without separating adhesions, then it *should* be removed, but not otherwise. It is better to wait till the abscess has healed, and then at a later date remove the appendix, if necessary.

The incision that Dr. Bowker uses is the one I have adopted. Dr. Bowker removes his appendices through a very small incision, and by the sense of touch; and I know his results are excellent. I prefer to see as well as feel when I am removing the appendix, and use a slightly larger incision, but in the same place and on the same principles as he does.

SOME POSSIBILITIES OF NERVE CROSSING.

By Dr. Kelvington.

Some three years ago I commenced a series of experiments on crossing the popliteal nerves in dogs, with the object of accurately determining the result. These experiments are not yet quite complete, but certain conclusions can be drawn which seem of sufficient interest to bring before members of the Congress. I think they warrant the trial of nerve-crossing in certain suitable—and I should like to emphasize this limitation—cases of infantile paralysis, and some rarer forms of paralysis due to destruction of portion of a nerve. This particular branch of surgery is not quite new, as it has been applied in certain cases of facial paralysis by Ballance and Purves, Stewart, and others, and, in one case of facial spasmodic tic, by Robert Kennedy. The facial was in most cases implanted in the spinal accessory, though the hypoglossal has also been used, and is possibly the more suitable.

It is needless to discuss the unsettled question as to whether regeneration takes place in the peripheral end of a divided nerve autogenetically, or only by the growing down of nerve fibres from the central end into the peripheral trunk. It may be mentioned that the latter is now regarded as the more probable method.

Any nerve we can experiment on contains several kinds of fibres, for even a so-called purely motor nerve like the facial carries sensory fibres, which allow the state of tension of the muscle to be known. After dividing the radial nerve, Head found the de-nervated parts of the hand, which had been rendered totally insensible to cutaneous stimuli, still remain sensitive to pressure that deformed the sub-cutaneous tissue. This, which he designated "deep sensibility," was due to fibres running with the motor nerves.

The various kinds of nerves, after being divided, regenerate at different rates. After section of the popliteals in the dog, motor power commences to return in from two to two and a half months: sensation commences to return probably somewhat earlier. I have found it completely absent six weeks after division and immediate suture of the sciatic in the dog; but the so-called sensory nerves contain separate fibre for touch, pain, and tempera-

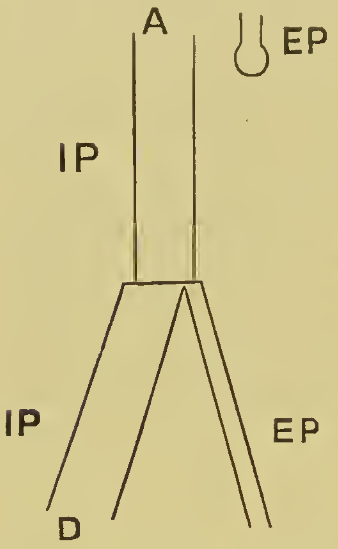
ture. Head, in the Marshall Hall address of this year, stated that regeneration occurs first in those fibres which convey the sensation of extremes of temperature and of pain, while the ability to appreciate light touch and accurate localisation of pressure appears later. The regeneration of vaso-motor fibres is a much slower process. Professor Osborne and myself have found that vaso-constriction commences about 200 days after division and immediate suture of the dog's sciatic, and gradually improves. In a dog, whose external popliteal was implanted into a gap in the internal popliteal, regeneration of the vaso-constrictor fibres was well marked after an interval of 293 days. About the regeneration of the vaso-dilators, which of course run separately from the vaso-constrictors, nothing is yet definitely known.

In performing nerve suture it is important to use some absorbable material. If silk be passed through the substance of the nerve it is never absorbed, and, from its mechanical irritation, leaves an area of scar tissue around it which interferes with the growth of fibres from the central to the peripheral end of the nerve. Silk can be used in the larger nerves if it merely be passed through the nerve sheath, and does not penetrate the nerve substance. Even in these large nerves I prefer to add another suture of absorbable material, which goes through the nerve substance, taking a good hold of both ends. With small nerves like the facial only absorbable sutures are permissible, and even in large ones, like the popliteals, are preferable. Chromicised catgut, prepared by Lister's method, is the material I have used. Ordinary catgut is absorbed before the two ends of the nerve are at all firmly united, and they are apt to pull apart on movement of the limb with very disastrous results. Strict asepsis is essential in nerve suture. Suppuration in the wound causes the stitches to give way, and the ends of the nerve to come apart. Even mild infection of a suture, without any suppuration, may cause an area of scar tissue about it which gradually strangles the nerve fibres, leaving a very unsatisfactory function. I have had this occur in a dog in which the skin wound healed by first intention. No suspicion of anything wrong was expected till the function of the limb gradually deteriorated. On cutting down, a fairly dense fibrous area was seen where the suture had joined the central and peripheral end of the nerve

Before describing the details of the experiment, the anatomy of the nerves used may be briefly mentioned. The sciatic nerve divides, at a variable point on the back of the thigh, into the two popliteals ; but even in the sciatic trunk, for some distance, the fibres of the two nerves are distinct : the two are bound together by an areolar tissue to form the main sciatic. The external popliteal, which is much the smaller, passes over the neck of the fibula, and supplies the peroneii and all the muscles on the front of the leg which extend the toes and flex the ankle : it also supplies skin sensation to nearly the whole of the front of the leg and foot. The larger internal popliteal supplies the calf muscles and the muscles of the sole of the foot, which flex the toes and extend the ankle : it also gives skin sensation to the sole of the foot. The experiments were arranged so as to allow of one nerve being implanted into the other. The central end of the implanted nerve was cut off short, so as to permit it to affect no junction with its own peripheral trunk After some time it was found a very useful function was obtained after this operation. The following are the details of some of the actual facts :—

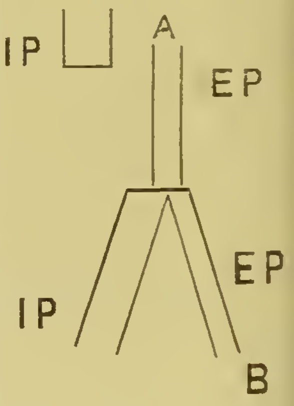
Dog 1.—Both the internal and external popliteal were cut completely across in the left hind limb. As much as possible of the central end ($\frac{3}{4}$in.) of EP was excised. The central end of IP was sutured to the peripheral end of both IP and EP. After recovery from the anæsthetic, the animal walked on the dorsum of the foot owing to the preponderating action of the muscles at the back of the leg. This, unless carefully guarded against, results in the

formation of an ulcer on the soft skin of the dorsum of the foot, which is easily abraded, and is now quite insensitive. The wound healed by first intention, function gradually improved, and at the time it was killed (139 days after the operation) the animal had a very serviceable limb. The finer



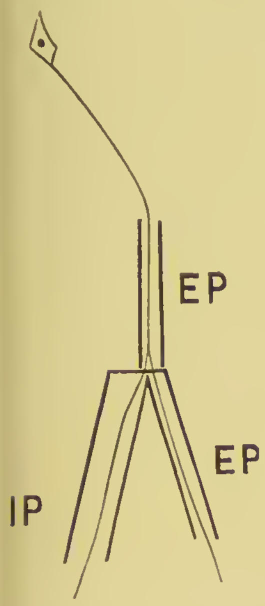
co-ordinated movements such as scratching a particular spot, were the last to be recovered, and were not at all perfect at the end of four and a half months. On walking and running, however, it was not easy, even at this time, to tell the affected limb. On anæsthetising the animal, and cutting down on the popliteal space, the place of suture of the three nerves showed a slight swelling. The central free end of EP had a bulbous termination, but no strands of nerve fibres extended down to the place of suture. This was always carefully excluded, and, in some cases, the fibrous tissue below was examined microscopically to show the absence of nerve fibres. The stimulation of the nerves was performed with the Faradic current, IP was cut across at A, above the line of suture, and electrical stimulation here resulted in vigorous contraction of both flexor and extensor muscles. The peripheral part of IP was then cut across at D. Stimulation at A now resulted in movement of the extensor of the toes only. These are, of course, supplied by the EP. Thus the muscles normally supplied by the EP nerve are now innervated through IP fibres which have grown down the peripheral EP trunk.

In dog 2 a similar operation was performed, but the smaller EP was fixed to the peripheral ends of both nerves, about an inch of IP being excised. In this case it was impossible to prevent two small ulcers forming on the dorsum of the foot; but these subsequently healed. After nearly four months the use of the leg had been considerably restored, though the finer co-ordinated movements were not so good as where the larger nerve had been used. The animal walked fairly well, but occasionally the left foot would double over on walking. On opening up the popliteal space, and stimulating the nerve, it was found that EP innervated both flexors and extensors: thus stimulation at A produced vigorous contraction of all the muscles of the leg. After cutting through the EP at B, stimulation resulted in action of the flexors of the toes alone. In another animal, where

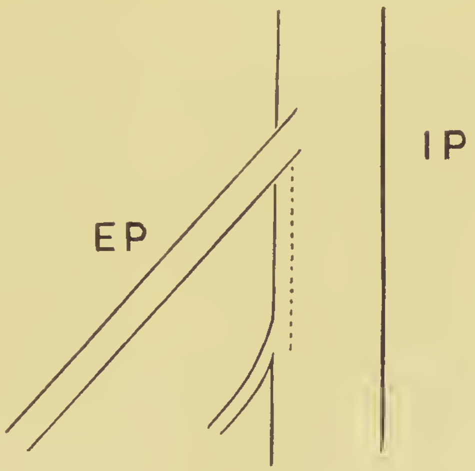


the same operation was performed, stimulation after four and a half months of the EP at B produced remarkable results: contraction of the flexor muscles through the distal end of IP resulted. This is known as the axon reflex phenomenon, and its significance I intend to discuss later. In this last animal it was shown that no vaso-motor regeneration had taken place. The nerves of these two animals were imbedded in celloidin and stained by Pal's method, and the fibres counted. In one case the central end of EP contained 4,435 fibres, and the distal end 2,786: the distal end of IP had 7,196. In the second case there were 5,826 in the upper and 8,868 in the lower nerve. In other words, there were more fibres in the lower two than in the upper nerve. This leads to the remarkable conclusion that where one nerve is sutured to two distal ones the axis cylinders of the

motor nerve divide, and a single fibre in the central end may join with two or more fibres in the distal nerve. The difference in number of the two is still more startling, if we remember that there are no vaso-motor fibres in the distal nerve while they are present in the central nerve. We may look on this as an attempt to compensate for the smaller number of nerve fibres which supply a muscle after this procedure. When a nerve is divided, and immediately sutured, the fibres of the central end are described as dividing and forming a kind of brushwork: these have a wavy course, and ultimately one branch from each axis cylinder joins with or grows down the fibre in the distal nerve. The rest of the branches from the central fibre gradually disappear. But if a more than corresponding number of fibres be present in the distal end, some of these branches, instead of disappearing, remain permanently, and have a definite use.



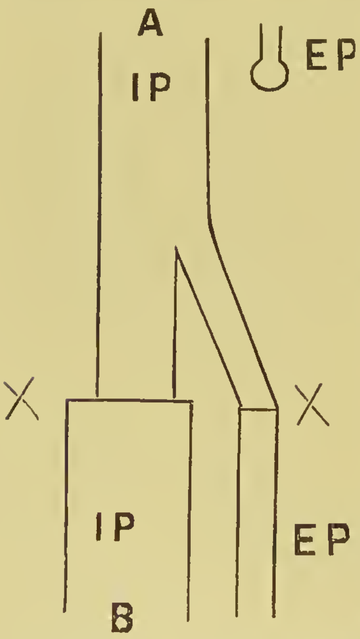
The axon reflex phenomenon shows that a single fibre from a motor horn cell in the spinal cord cannot only divide, but can divide and supply two different—and in this case antagonistic—muscles. (See Fig.) This can be the only explanation, as the electric stimulus can only be spread by continuity of a nerve fibre. I do not suppose that every dividing axis cylinder behaves in this way: probably is it exceptional. As any discharge from such a motor cell results in contraction of the two sets of muscles, it is evident that accurate co-ordination would be impossible with this method of suture. At the same time, some of the muscular contraction would be wasted, and dissociated movements impossible—or, at any rate, only possible by ceasing to use those muscular fibres which are thus innervated. Ballance and Purves Stewart's cases of implantation of the facial into the spinal accessory confirm this. The method of suturing the facial into a gap in the spinal accessory allows of this axis cylinder splitting. These authors say, from their experience of seven cases, "So far in our series of facial accessory cases we have not observed any independent movement of the face unassociated with that of the trapezius and sterno-mastoid (which are innervated by the spinal accessory nerve), though in several cases a minimal innervation of the trapesius is sufficient to cause facial contraction. Körte reported a case where the facial was implanted in a gap in the hypoglossal of the same side. He found that, after movement had been restored, any contraction of the facial muscles was accompanied by movement of the tongue. When the tongue was moved facial contraction also took place. This implantation of one nerve into a gap in another (as shown in the diagram)



at first seems to offer a considerable advantage over the methods of completely dividing the nerve. It is obvious that the shock of the operation is less, and that the paralysis resulting from division of the nerve will not be complete, as some of the trunk has not been injured. But there are di -

advantages which more than counterbalance the good effects of this procedure. In the first place, it does not do away with the associated movements owing to the peculiar form of splitting of the axis cylinder I have described. Secondly, it does not utilise to the utmost the capacity for division of the nerve fibres which occurs when one nerve is sutured to two distal trunks. An uninjured active cylinder cannot multiply in this way. I am unable to say into how many branches a single fibre can divide, except that it can establish connection with at least two fibres in the distal nerve. It seems also impossible to find this out even by the method of counting, as any nerve contains not only motor but sensory—and probably also vaso-motor—fibres: these may equally as well divide. There is no reason why the number should not be more than two. Possibly, too, the number of branches may be greater when there is a greater number of fibres in the distal nerve compared with those in the central nerve. From the less satisfactory result obtained when the small EP was used, it would appear that a limit for this proliferation is soon reached. Thirdly, with this method, some of the branches coming from the nerve on the side where the other is implanted in it may be mechanically or otherwise deprived of all fibres from the central end. This actually happened in one case I studied. A muscular twig (see Fig.), as shown by stained sections, contained no fibres whatever. This is a very serious objection, and I think of itself puts this particular operation out of the question.

My next attempt lay in the direction of abolishing or reducing to a minimum the associated movements due to the splitting of a nerve fibre, so that one branch goes to one set and the other to another group of muscles. The following experiment was actually proved to effect this object, and from a functional result was the best I have ever obtained. The left leg was operated on, and about 1 inch of EP excised. The IP was separated into two parts: the separation was made between the fascicular bundles, and as little damage as possible was done to the actual nerve tissue. These were then cut transversely across, and the smaller part was sutured to the distal end of EP. The larger one was then fixed by a single suture to the di tal end of the entire IP (see diagram). After about five months the function was almost perfect. Scratching, which I look on as the best test of accurate co-ordination in the dog, was done nearly perfectly. Professor Watson saw the dog, and was hardly able to say which leg had been interfered with: the animal could stand on and jump with the hind legs alone perfectly well. The dog was anæsthetised, and stimulation performed. The IP was cut across at A, and stimulation produced vigorous response in the muscles of the front and back of the leg. The distal end of IP was then divided across at B.

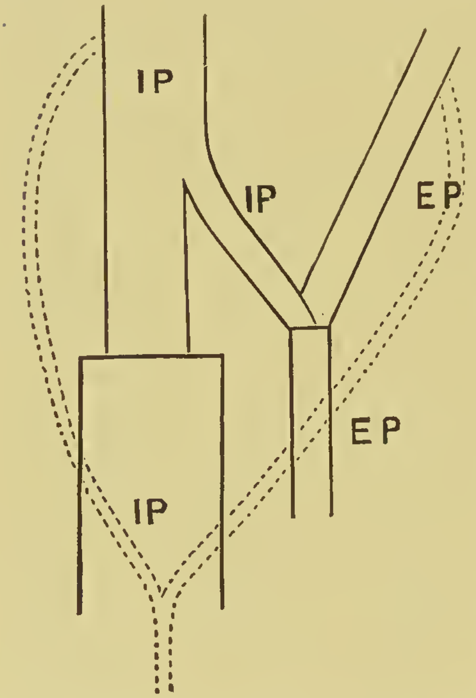


Stimulation at A now produced contraction of the extensors of the toes alone: stimulation of IP at B gave no contraction of the extensor muscles. This proves that no single fibre in the central nerve was supplying antagonistic muscles by the distal nerves. The muscles of the front and back of the leg were now dissected. They were well developed, and nearly as bulky as on the healthy side. I may mention, as a confirmatory proof of this device, that in another dog, where the same operation was performed, it was noticed after two and a half months that the co-ordination was getting rather worse. The wound was

re-opened, and it was found that the nerve at XX had fused considerably in a common mass. There was also a good deal of scar tissue at this spot, as the catgut sutures had evidently been mildly infected. The two bulbs at XX were then separated, and the wound stitched up again. In a few days it was noticed that the function had greatly improved, notwithstanding that a good many fibres must have been cut in separating the fused nerves. The animal is alive at the present time ; but the result is not a great success, for I believe the scar is contracting and strangling many of the nerve fibres. It will often be impossible to avoid injury of some few nerve fibres in this splitting operation, especially where small nerves such as the facial and spinal accessory are used ; but even if we do this, it is only a few fibres at the angle of separation that will supply antagonistic muscles, and the associated movements will be reduced to a minimum. I believe it will be of great service in facial anastomosis, for here any associated movements are especially objectionable. It is also evident that, at first, associated movements will take place even with splitting until the co-ordinated mechanism has adapted itself to the new conditions. But this will probably disappear as the cortical or other centres become re-educated.

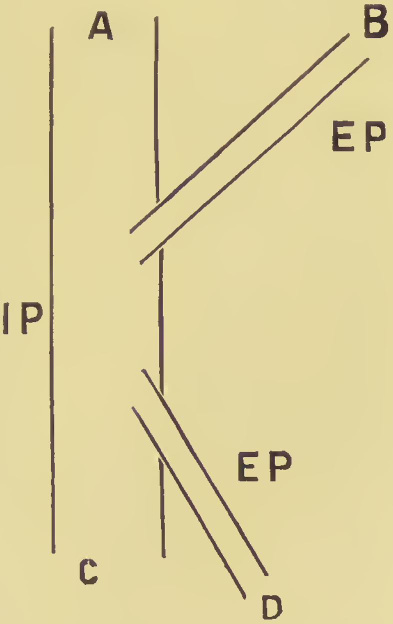
Having now decided on the best method in which the central end of one nerve is able to innervate the distal ends of two (and there is no reason why there should not be more than two), we may discuss its application in a very common form of paralysis. In infantile paralysis we have a loss of power in particular muscles as the result of destruction of a certain number of motor cells in the spinal cord. Skin sensation is quite unaffected. The foregoing experiments prove that a certain number of these motor cells can be dispensed with, and yet all the functions of the limb be efficiently retained. This suggests the idea that it is probable a good result could be obtained by suturing the nerve supplying the paralysed muscle on to the cut face of the proximal end of an adjacent healthy nerve. I think nerve-crossing would be especially applicable in the common form of infantile paralysis, where the extensors of the toes and peroneii are affected. The external popliteal being small compared with the internal, experience with the dog would lead us to expect a good functional result. Where the calf muscles were paralysed we should expect a useful though not so strong a limb as in the former case. There are three classes of cases where it is probable nerve-crossing would be inadvisable : First, in those where a large amount of recovery takes place naturally, with a fairly useful limb, it is very doubtful if we could produce a better result, even if as good a one, by nerve suture : a good many cases of infantile paralysis go on to almost complete recovery in the course of time. Secondly, in very severe cases, where only a few muscles remain intact (as, for example, a case where the peroneii are the only muscles of the leg to retain their function), it is very questionable if the fibres of the musculo-cutaneous nerve could innervate at all efficiently the whole of the muscles of the leg. In these cases arthrodesis, with anchylosis of joints and tendon transplantation, are our only resource. In a case with loss of power in the whole of the leg muscles it might be possible to bring the sciatic through the adductor muscles, and fix it on to the anterior crural ; but, very probably, just as much good would result from anchylosis of the knee and ankle joints, rendering the limb a firm and rigid support in progression. Excellent results are obtained from the transplantation of tendons and muscles. It is to be noted that tendon transplantation merely distributes to advantage muscles remaining healthy. Nerve-crossing aims at *restoring* the affected muscle. Thirdly, nerve-crossing is useless in old cases of infantile paralysis. It is known that when a muscle has been cut off from its nerve supply for some time (about two years), little or no recovery follows suture of the separated ends of the

nerve—that is, as far as motor functions are concerned, for sensation can apparently be recovered after a much longer interval. This is usually put down to complete atrophy and degeneration of the muscle fibres, but it may be caused by changes in the delicate end plate of the nerve to the muscle. Somewhere about two years would appear to be the limit in which nerve implantation can be of use. It should be possible to come to an opinion as to whether a particular case merits trial of nerve-crossing in about six months at the outside limit, and often in a much shorter time. Meanwhile the nutrition of the muscles should be kept up by electricity and massage, so that if suture be performed they should be as healthy as possible. The electrical reaction of the muscles gives an idea as to how the case is progressing, and allows of an exact localisation of the injury to be made. The operation I suggest, in a suitable case, where the peroneii and extensors are affected, is as follows:—The internal popliteal nerve is first split into two parts, which are divided transversely across. The external popliteal is cut through at the same level, and the central end of the smaller separated part of IP sutured to the peripheral end of EP. At the same time the central end of EP should be fixed in the same place, as usually some motor fibres have escaped, and it is advisable to use them if possible. Even if these be all destroyed, the EP still contains sensory fibres, which will regenerate along their old paths. The other central part of IP should now be sutured to the whole of its own distal trunk. Theoretically, we wish to interfere with the motor fibres only; but for anatomical reasons, this is obviously impossible. If any purely sensory branches can be spared they should be kept out of the way during the operation. In the present case, two sensory branches—the nervous communicans tibialis and fibularis—come from the IP and EP, respectively, and join to form the external saphenous, which give sensation to a part of the foot. It is all the more important, as the insensitive limb is liable to ulceration from pressure on the insensitive part: this is probably the origin of the so-called "trophic" ulcers, though the term "anæsthetic ulcers" would be a better one. The exact form of procedure will, of course, vary with the distribution of the paralysis, and can be only worked out for each case by carefully noting the individual muscles affected. There is a great deal of shock after any operation involving nerve-cutting, and several of my dogs have died from this within the first few days. All precautions against shock must be taken; and I believe cocainisation of the nerves central to where they are to be divided would be of the utmost value. This is done by injecting a small amount of sterilised 2 per cent. solution of cocaine within the nerve sheath: this produces a block, preventing afferent impulses reaching the vaso-motor centres, and causing a great fall in blood pressure. For the first three months there will be absolute paralysis in the parts supplied by the divided nerve, and great care will have to be taken to avoid sores from pressure. After about a fortnight—or even

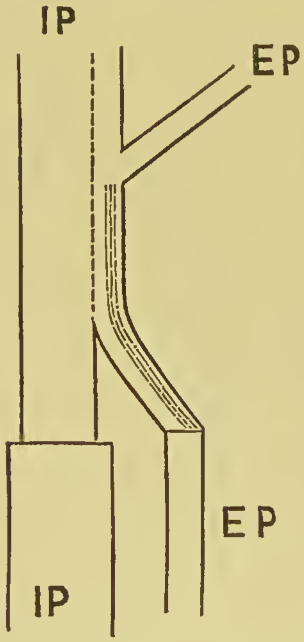


from the first—massage should be practised daily ; but we should be careful not to put any strain on the sutured nerve for some time. In fact it would be safer to put the child's limb in a splint to avoid this. The amount of function returning will depend on the size of the healthy nerve available, and, probably, on the duration of the paralysis. In no case will it be quite as good as in the normal state. I have examined a good many nerves microscopically, before and after nerve-crossing, and find that the nerve is always smaller than in the healthy state. If a nerve be divided and immediately joined, the finest movements are never quite so accurately under control as before. It is probable that if a pianist had his median nerve severed, and shortly after sutured, he would never be able to command delicate effects on his instrument. But this would be of little consequence in the lower limbs, though in the hand and arm it may be a drawback. When a nerve is divided and sutured it is unlikely that exactly the same axis of the trunk is maintained, and many of the fibres in the central end will not grow down their old paths. Thus re-education will be needed to effect movement, and all such processes take place much easier in young people. In children the nervous tracts are not so fixed as in the adult, and alternative routes are comparatively easily developed. Though these experiments are not yet complete, enough has been done to warrant the trial of nerve-crossing in selected cases of infantile paralysis. If performed at an early date, it may prevent much of the deformity which results from this distressing condition.

The results of nerve implantation suggest that it will be a means of treating cases where a considerable length of nerve is destroyed by injury, involvment in callus, or tumor, &c. ; provided there were a companion nerve available for re-inforcement. But it would be preferable to use the central end of the injured nerve in some way, as the motor cells in this case are intact. The following experiment was performed on several animals to test this. Very interesting results were obtained. About 1 inch of EP was excised. The IP was cut nearly half way through, in two places, and into these gaps the central and peripheral ends of EP were fixed by single sutures. After a time very good use of the leg was regained, and after about six months the function of the leg seemed almost perfect. The dogs were anæsthetised and the nerve stimulated. IP and EP were cut completely across at A and B. Stimulation at A with the Faradic current resulted in contraction of the muscles at the front and back of the leg : stimulation at B gave the same result, but less vigorously. After cutting IP at C, stimulation here, caused by the axon reflex contraction of the extensors and peroncii. In a second dog stimulation of EP at D resulted in movement of the calf muscles. It is to be noted, too, that the response in the extensor muscles from stimulation of IP at C is more vigorous than when the central end of EP is stimulated : in other words, the associated movements (which prevent co-ordination) are more active than the movements produced by EP alone. The condition of the nerves is evidently a very complicated one. After 300 days Professor Osborne and myself obtained regeneration in the vaso-constrictor fibres in the peripheral part of EP.



A very important point is that it is presumably a matter of indifference what length of EP be excised. After loss of a short part of a nerve trunk in a limb, the two ends can be brought together by stretching the nerves and relaxing the limb. An interval, of not more than 2 inches at the most, can be bridged over by procedures which offer an absorbable scaffolding down which nerve fibres can find their way. This is supposed to act as a guide to the fibres coming from the central end; but I am not aware that this has actually been proved. It seems immaterial what absorbable material is used for this purpose. A piece of nerve excised from some animal apparently does no better than several strands of catgut. But none of these methods can offer the inducement that a living nerve *in situ* does, for this exerts a definite chemiotactic attraction for nerve fibres to grow down it. When this interval of about 2 inches is exceeded, the resources of surgery are at present very inadequate. Probably even at this limit the functional result would be unsatisfactory. The very extensive operation of shortening the limb by re-secting the bone, to allow of the separated ends of the nerve being approximated, is advocated, but this results in mutilation and in a serious operation. Nerve implantation in these cases is almost ignored. Cheyne and Burghard (2) mention it, only to condemn it. The cause of failure seems to be due to the fact that only the sheath of the sound nerve is opened, and few, if any, of its fibres are divided. When the injured nerve is implanted in such a way it has no chance of getting any fibres to grow down its trunk. It is essential that a considerable gap dividing many fibres should be made in the sound nerve, and into this gap the central and peripheral ends of the injured one should be fixed. It is a very dubious point as to whether one ought to use the central end of the injured nerve, as in the above experiment. The hind legs of the dog do not allow one to judge accurately of the finer movements. As far as can be decided from these cases, it would be better not to use the central end in this way. Dog 7 showed that the contraction resulting from stimulation of the central end of the injured nerve was less vigorous than that resulting from the axon reflex; or, in other words, the fibres actually proceeding from the central to the peripheral part of the injured nerve produce less result than those which are coming from the sound nerve and splitting.



The alternative method allows of splitting of the healthy nerve, and does away with the axon reflex. It can be easily seen that certain strands in nerve occupy a particular position in the nerve trunk. For example, the external popliteal fasciculi occupy the outer part of the sciatic trunk, and can easily be dissected for a considerable distance up the thigh: in fact they seem to be anatomically separate up to the pelvis, and it is not uncommon to find the sciatic nerve coming out of the sciatic foramen in two trunks, the external and internal popliteal separated by some of the fibres of the pyriformis muscle. The same separation can be made for a shorter distance with the muscular twigs coming off the nerve trunk. To apply this to the condition in question, the piece of IP sutured to the distal end of EP can be separated up along the dotted line (see diagram). If a short cut be made in this for the central end of the EP to be inserted, some of the EP fibres would now reach their own distal trunk: at the same time none would

run into the rest of the internal popliteal going to the flexor muscles, and no associated movements would result. Part of the split internal popliteal here plays somewhat the role of a graft (part shaded to right of diagram) *in situ*, while the rest of the split consists of IP fibres going now by means of the EP trunk to the extensors of the toes and peroneii. The actual condition of the nerves can be seen by a glance at the diagram. This form of nerve implantation would be chiefly applicable to the arm and forearm, or for anatomical reasons.

I have to express my thanks to Dr. Bird for taking charge of my paper in my unavoidable absence from the Congress.

THE TREATMENT OF CLEFT PALATE BY BROPHY'S METHOD.

By Dr. M. Morton.

Although descriptions of the methods originated by Brophy for the treatment of congenital defects of the palate have been published in America since the year 1893, its merits have very tardily been recognised in English surgical literature. In all probability this might be explained by the fact that Brophy addressed himself rather to dentists than to surgeons; and, in America, the dentist very commonly undertakes the treatment of oral as well as of dental diseases. At length, however, the merits of this excellent operation are beginning to receive the recognition at the hands of English surgeons which they deserve; and I can now refer you to a description of the operation in the *British Medical Journal* of June 24th, 1905, where Sir Thornley Stoker, of Dublin, records three cases successfully operated upon after Brophy's method, and to Mr. Edmund Owen, who recorded a successful case of his in the *Lancet* of December 19th, 1903, and recommends Brophy's operation in his recently published monograph on cleft palate.

Having recently delivered a patient of a child suffering from a complete cleft palate and hare-lip, I had much trouble in obtaining a good description of Brophy's operation. With the idea rather of assisting members of this Congress to a description of Brophy's methods than of recording my successful experience of them, I have thought that this paper might be useful. I shall not hesitate, therefore, to quote largely and freely from Brophy's published descriptions of his methods, because these lie, for the most part, in dental literature, not easy of access to surgeons.

The attitude of English authorities in the treatment of congenital palatal defects is, unfortunately, very conservative. A strange ignorance of Brophy's work still besets our standard authorities upon operative surgery, even in their latest editions. Probably, therefore, it is only fair to bear in mind that their opinions have been expressed in ignorance of Brophy's method, and with reference only to operation by the union of mucoperiosteal flaps.

Jacobson (*Operative Surgery*, 1902) says :—" With regard to the argument that cleft palates require operating upon in infancy, because of the difficulty in nourishing the patients, I would reply that this difficulty can be met by persevering care; and, when this is not the case, the little patient is not likely to be in a fit state to meet what is one of the severest operations in infancy."

With regard to the voice, while there is no doubt that the earlier a successful operation is performed the better will be the voice, it is possible that this has been too strongly put forward, to the exclusion of the other side of the

question. Thus the possibility of an unsuccessful operation, with the inevitable loss of tissue and scarring of what remains, has been too much kept in the background, and children under 2 years of age cannot have contracted a habit of speech so bad as of itself to call for operation before this date.

Jacobson quotes the opinions of various English surgeons as to the most favorable period for operation, viz., Davies-Colley, who preferred the age of 14 months; E. Owen, for a soft palate, in the first six months, for a hard and soft palate together, in the second year; Clutton, before the second year; Arbuthnot Lane, during the fifth week; G. A. Wright, of Manchester, between the fourth and sixth years. Owen, for one at least, has probably been converted since to Brophy's opinions, as in the *Lancet* of December, 1903, he recorded a successful case operated upon by Brophy's method, and in his recent monograph expresses his complete accord with Brophy's principles.

Jacobson concludes: "Writing, as I do, for the guidance of many of my younger brethren, I consider that the *end of the first year* should be reached before a cleft of the soft palate should be operated upon, and then only under favorable conditions, and that the patient should be 2 *years old at least* before a *complete cleft* of the palate is operated upon. As experience is gained, operations may, no doubt, be performed successfully at an earlier date; but at any time during the first year of life the risk of failures is great, owing to the effects of hæmorrhage, the readiness with which convulsions are excited, the liabilities to bronchitis and diarrhœa, and, lastly, the delicacy of the soft parts and the readiness with which they tear."

Cheyne and Burghard (*Surgical Therapeutics*, 1901) say:—"The operation for a cleft palate demands more time than that for hare-lip, and entails greater loss of blood and more shock; and, for these reasons, it cannot be done with safety at so early a period of life." They advise, in slight simple cases, operation upon hare-lip within a few weeks—or even a few days—after birth; but in wide clefts, extending up into the nostril, that operation should be delayed until at least three months. They admit, however, that "it is essential that the cleft should be closed before the child learns to talk."

"Unless there are very strong reasons against it, the operation should not be delayed beyond the third year of life—the most favorable periods being from eighteen months to two and a half years. In some cases, where the cleft is not very bad, the operation may be performed before the eruption of the teeth—from the fifth to the eighth month. If both hard and soft palates be cleft, it is well, if possible, to unite both at the same operation."

Even the *International Textbook of Surgery*, 1901—although room for a brief and inadequate description of Brophy's operation is found—says:—"The operation for cleft palate and uvula is much more difficult and dangerous than that for hare-lip alone, *and should not be undertaken before the completion of the third year.*"

As shown by reported debates of American medical societies, even American surgeons are not unanimous in their adoption of Brophy's principles, and Dr. Willy Meyer, at the New York Surgical Society (*Annals of Surgery*, February, 1904), said that he did not advocate operation before the end of the third month. Dr. Richard H. Harte, of Philadelphia (*Annals of Surgery*, June, 1904), says that the most desirable time to operate is about the time the child is beginning to talk, as the ability to make sounds will be more easily acquired. Operation upon children under 1 year of age is followed by a large mortality. Dr. James J. Young (*idem*) says that closure at the age mentioned is preferable to operation on the very young. In double hare-lip and cleft palate it is best to operate between the ages of 7 and 14, doing the plastic operation of the French.

The first principle of Brophy's method is operation at as early an age as possible after birth. Brophy says (writing in 1901), " I am convinced—after making 570 operations for the cure of cleft palate, of which 211 were upon children younger than 6 months, the others being from 6 months to 52 years of age—that *the best age to select for operating is within three months after birth.* Of the children under 6 months, no deaths occurred. Two deaths followed operations upon children 3 years of age, one of which I am confident resulted from failure of proper care. Of the whole number of operations, two died, as above stated."

Brophy operates in early life for the following reasons :—

1. " The surgical shock is less, because the nervous system of the child is not well developed ; that young children re-act better, and mental apprehension is eliminated."

All of us who have had any experience of midwifery work know in what an exceedingly " dilapidated " condition (if I may use the word) infants are very commonly ushered into the world, and how, even after prolonged, vigorous—and often violent—measures for resuscitation, the child in the course of an hour or two is in as good a condition as if born in an easy and rapid manner.

2. " Before the bones are fully calcified they may be bent or moved without fracture. Bone, at birth, is almost one-half organic matter ; hence the injury is really less in closing a cleft than it would be if the calcification were more complete."

3. " The muscles are early brought into action, a good velum is secured, with consequent establishment of normal articulation when the child begins to talk."

4. " When the palatal processes of the maxillæ are united, it will be observed that the development of the bones of the alveolar processes of the upper jaw assumes a form nearly or quite normal, and when the teeth are erupted they will properly occlude with the lower ones, or nearly so."

Any resultant deformity of the teeth Brophy claims can be easily corrected by orthodontial methods ; and he makes the important point, backed by very convincing evidence, that in nearly all cases there is sufficient tissue in both hard and soft palates, but the parts are not united.

5. " The existence of the hare-lip gives more room for work." Referring to the practice of early hare-lip operation and late cleft-palate operations, Brophy says :—" It is a great mistake to commence at the oral opening and partially close the only aperture through which a subsequent palate operation must be made. The surgeon needs all the space that can be secured."

For the following description of Brophy's method of closing clefts of the palate in the main I am indebted to my friend, Mr. R. J. B. Yule, D.D.S., of Melbourne, who was associated with Dr. Brophy in Chicago in the treatment of a great many of these cases. I have embodied a few points that my limited experience has suggested to me, and that I think will be found of advantage.

In addition to a skilled chloroformist, a good assistant is of great advantage. Two nurses will be required ; and it is of immense advantage to operate in a well-appointed theatre of a hospital—public or private.

The child, having been anæsthetised, is wrapped up and pinned in a sterilised towel. If the weather be at all cold he is laid upon a hot water cushion throughout the operation. He is placed with the head hanging over the end of the table, with the face towards the light. A nurse undertakes the important duty of supporting the head with the palm of her hand. She sits, or stands, most conveniently to the right of the patient. The operator should be seated.

After preliminary anæsthesia with the mask, the chloroform is continued with a Junker's inhaler throughout. As the tender skin of a young infant is easily excoriated, any strong antiseptic solution should be rinsed off the hands after sterilisation, in plain sterilised water or weak boric acid solution. The instruments should be taken out of sterilised water or weak boric acid lotion.

When operating upon the hard palate alone, a gag is unnecessary, and is more likely to be in the way: better exposure is obtained by a tongue depressure with a good curve. The tongue is thus kept out of the way, and sufficient force may be easily applied to keep down the lower jaw if the patient is well anæsthetised. I have not found Brophy's oral speculum necessary. For paring and suture of the soft palate, a Whitehead's gag is most useful.

After a preliminary cleansing and sponging of the face and mouth with boric acid lotion, raise the cheek, and—well towards the posterior extremity of the hard palate, just back of the Malar process, and high enough to ensure its being above the palate bone—insert, by means of a needle, a large braided silk suture, carrying it through the substance of the bone to the central fissure. When the needle appears in the cleft, pick up the thread which it carries with a tenaculum hook or forceps: withdraw the needle, leaving the thread *in situ*, and catch the ends of the thread in an artery forceps. One of the difficulties of the operation is the picking up of the thread from the eye of the needle in a small narrow space filled with blood. I think that the use of silk dyed black should render this part of the operation a little easier.

Through the corresponding part of the opposite bone pass a suture in same manner. Pass the one loop through the other and pull it out, carrying the other with it. Nearer the front portion of the maxilla, anterior to the Malar process, insert another silk suture in like manner. These can then be substituted by silver wires of No. 18 or 20 American gauge. The next step is to make lead plates, of No. 17 American gauge, and moulded to the convexity of the buccal surface of the bones. Provide these with eyeholes to correspond to the position and number of wire sutures. The protruded ends of wire are then passed through the lead plates upon each side: the plates are then placed gently in position. Trim up the edges of the fissure and also the edges of the palate bones. The wire sutures may then be drawn tight and twisted together, *i.e.*, the right end of the anterior to the right end of the posterior, and the same on the left side. With the fingers forcibly press the two maxillary bones together until the cleft is completely closed, and then the wires can be tightened to hold the parts together. If the edges of the cleft do not approximate, a further step may be taken. After the cheek has been well raised, divide the mucous membrane and bone through the Malar process. Carry the knife in a horizontal direction, and when well inserted sweep the handle backward and forward, thus dividing the maximum amount of bone and a minimum amount of mucous membrane. This done on both sides, the wire suture can then be tightened and the edges approximated.

Brophy adds —"The incision of the mucous membrane must be made as small as possible, as this membrane must serve to retain the bones in proximity, or to hold them nearly together . . . . . . Separation of the bones is attended with very little hæmorrhage, which is easily controlled."

I would add myself that this division of the Malar process should be thorough—more free, probably, than Brophy's description would lead one to believe.

After approximation of the hard palate, the premaxilla will be dealt with as required to bring it into line with the alveolar border. The edges of the soft palate, which will have been refreshed at the same time as the hard

palate, should now be closed by fine silver wire or horsehair sutures, and coaptation sutures of similar material inserted here and there along the hard palate, if required.

The after-treatment consists in absolute cleanliness, and careful spoon-feeding with sterilised milk. Frequent gentle syringings with weak boric acid lotion are required, and in the after-treatment a good nurse should always be obtained where possible.

Brophy says that the plates may be left in from two to four weeks. But, as they seem to cause little or no trouble, there is no reason why they should not be left for a longer period where much strain has been put upon the retention of wire sutures.

F. P., the subject of my operation, was operated upon June 11th, 1905, being then 14 days old. He suffered from a complete cleft of hard and soft palate and hare-lip on the left side. He was an otherwise healthy, well-nourished child, and weighed 9lbs. at birth. The child was anæsthetised with chloroform by Dr. H. M. Hewlett, was wrapped in a sterilised towel, and laid on a hot water bag. He was placed with the head hanging over the end of the table; the head being supported by the hand of a nurse throughout. A Whitehead gag was inserted, and the whole cleft was pared on each side. The silk sutures were then passed as above described, and stout silver wire substituted, No. 00 English gauge. The two lead plates, No. 17 American gauge, were then adjusted. The maxillæ were then forcibly pressed together, and the wire sutures tightened by twisting.

It was found that complete apposition was not possible, so Brophy's procedure of division of the Malar process was attempted. Better apposition was thus obtained, but not actual contact; a narrow cleft of barely one-eighth of an inch remained. I believe that a more thorough division of the Malar process would have given us complete apposition. The soft palate, having been freed at its junction with the hard palate, was then sutured with horsehair sutures; the smallest English semicircular surgical needle obtainable being used. The premaxillary bone was then forced into position, horizontal incision above the alveolus being necessary. This was retained in position by a silver wire suture. There was not much hæmorrhage, and the patient stood the operation, which occupied about three hours, very well.

I am obliged to Dr. Yule for giving me his skilled assistance, and the benefit of his valuable experience in the case.

The child had been spoon-fed from birth; partly with breast milk, which had been drawn with the breast-pump, partly with cow's milk. During the week following operation the child gained 7½ ounces, and during the subsequent week, 8½ ounces. A few days after operation the soft palate had completely broken down, but the anterior part of the hard palate united solidly.

On August 5th, a second operation was undertaken, the retention plates being still *in situ*. There being thus so little space available, it was impossible to use Brophy's method of approximation of mucoperiosteal flaps by means of lead plates. The whole of the cleft was pared, and mucoperiosteal flaps were raised from the hard palate, after longitudinal incision just within the alveolar border on each side. The whole of the hard and soft palate was united with fine horsehair sutures, threaded on Brophy's small needles. The hare-lip was then pared and closed. More blood was lost at this operation, but the child stood it well, and still continued to gain in weight during the succeeding week. The soft palate unfortunately broke down again, but the hard palate healed perfectly. The plates loosened, and were removed on August 19th.

The child is now 3 months old. He has a good lip, a good alveolar border to his maxilla, and a solid hard palate. He is in perfect condition, and weighs 13½ pounds although still spoon-fed. The soft palate still remains to be closed on some future occasion.

I cannot imagine how such an excellent, although, as yet, partial, success could have been obtained otherwise than by Brophy's operation. Although the eruption of the teeth is still two or three months distant, the child's maxilla has been restored to a nearly normal condition, and there is still ample time to restore the soft palate long before articulation is required.

I think that there can be little doubt that Brophy's principles are sound, that the operation is a safe one, and that Brophy's operation is destined to be, in the future, the operation of selection in all cases coming under observation in early infancy.

For any who are anxious to find a full and particular description of Brophy's operation, inadequately sketched in this brief paper, I can refer them to the following, viz. :—

BINNIE, J. F.—" Manual of Operative Surgery, 1905," where a short but clear description of the operation is given.

BROPHY—*Dental Cosmos,* April, 1901, where an excellent description of Brophy's methods of treating clefts of both hard and soft palate is given.

BROPHY—Reprint of paper by Brophy, " The Surgical Treatment of Palatal Defects," read at Columbian Dental Congress, Chicago, 1893.

BROPHY—" Radical Cure of Congenital Cleft Palate," *Dental Cosmos,* September, 1899. *Boston Medical Journal.*

OWEN, EDMUND—*Lancet,* December, 19th 1903.

OWEN, EDMUND—" The Treatment of Cleft Palate," Monograph Series, 1904.

STOKER, THORNLEY—*B.M.J.*, June 24th, 1905.

EXTROVERSION OF THE BLADDER: ITS TREATMENT BY EXTRAPERITONEAL IMPLANTATION OF THE URETERS INTO THE RECTUM.

BY H. SIMPSON NEWLAND, M.B., M.S., ADEL., F.R.C.S., ENG., Adelaide, South Australia.

At the last Intercolonial Medical Congress held in Hobart, Dr. Lendon, when introducing his original operation for the treatment of this distressing deformity to the notice of the profession, made the following prophecy : " I am inclined to think that it will prove to be the method of the future, subject, possibly, to some slight modifications." This prediction was made a comparatively short time ago, and it is too soon to say whether it will prove to be true. Maydl's intraperitoneal operation, or some modification of it, such as that of Borelius, is certainly more commonly performed. That very few cases have so far been treated by the extraperitoneal method is not surprising when the rarity of the deficiency is considered.

With a view to calling the attention of surgeons to the advantages of extraperitoneal implantation of the ureters (uretero-proctostomy), this record of a successful case is published. Cases which have been operated upon by this method are collected in an appendix, which, however, lays no claim to completeness.

The following are the notes of the case which is the subject of this paper:—

H. G. W., a boy aged 7, fairly well developed for his age, was brought by his parents in March, 1904, for an opinion as to the advisability of an operation. The deformity of the bladder had been present from birth. At the age of 3 an operation had twice been unsuccessfully performed in the Adelaide Hospital. He had always enjoyed good health, apart from his deformity.

The family history revealed nothing of interest.

On examination, a pyriform area of bright red mucous membrane was to be seen in the pubic region. The stalk lay downwards and was formed by the penis, which was in a state of complete epispadias. Above and at the sides the junction of the skin and mucous membrane were well defined. The exposed surface of the bladder ended below in the cleft urethra, in the proximal portion of which the vera montanum could be recognised. A transverse sulcus, due to the folding of the urethral on the vesical mucous membrane, existed. When this was opened out by gently drawing the penis downwards, the vera montanum and ureteral orifices were more distinctly exposed. The latter lay at the summits of two papillæ, from which urine gushed forth intermittently, but seldom simultaneously. There was no ulceration of the vesical mucous membrane nor irritation of the surrounding skin. The cleft prepuce was very large. The scrotum was well developed. The testicles were incompletely descended; they lay below the external ring, and could be pushed into the scrotum. No hernia existed.

The pubic bones were widely separated. The gap could be felt distinctly, and was very obvious in a skiagraph. The pelvis was consequently flatter than normal, and the inner surface of the thighs, instead of meeting at a narrow angle at the perineum, were separated by a wide interval. The gait was therefore rather waddling in character.

The urine collected from the uncleansed surface of the bladder was slightly alkaline (phosphates); that collected from the ureters direct was acid, and free from albumen and pus. The thoracic and abdominal organs were healthy.

Dr. Lendon courteously granted me the use of one of his beds in the Children's Hospital, and kindly assisted me at the operation.

First Operation.—On March 29th, 1904, the following operation was performed after anæsthesia had been induced by the chloroform-ether sequence. The anus was dilated and the rectum washed out with saline solution. The large bowel had previously been emptied by a purge followed by an enema. To better expose the ureteral papillæ a silk retraction suture was passed through the glans penis and given to an assistant to hold. The vesical ends of the ureters were then freed according to the method employed by Peters. A No. 3 Jacques catheter was passed into each ureter for about 2 inches and fixed to the ureteral papilla—the one with a fine silk, the other with a fine catgut stitch. A circular incision of as great extent as possible was then made through the mucous membrane around each papilla. This was deepened until the bladder wall was completely cut through. The separation of the ureter from the loose cellular tissue, in which it lay along the wall of the pelvis, was easily effected. Each ureter was exposed for 1½ to 2 inches. With the left forefinger in the rectum and the right in the suprapubic wound, the

anterior and lateral walls of the rectum were defined. A Lister sinus forceps was passed into the rectum, and its fine point thrust through the lateral wall about 1½ inch above the anus. The small wound was dilated by opening the blades of the forceps. The catheter in each ureter was grasped by the sinus forceps and drawn through the wound in the lateral wall of the rectum on the corresponding side. The ureter followed the course of the catheter, the rosette of vesical mucous membrane passing through the wound in the rectum, much as a button passes through a buttonhole. With a large rosette of mucous membrane and a small rectal wound there is no danger of the ureter slipping out of the rectum, and there is no necessity for a restraining suture. The catheter in the left ureter was forthwith removed. The rosette on the right side was not a large one, and it seemed better to leave the catheter in the ureter. The remainder of the vesical mucous membrane was not interfered with. The wound left by the transplantation of the ureters was gently packed with iodoform gauze. The free end of the catheter in the ureter was passed into a bottle containing boracic lotion. The operation, though tedious, was well borne, and the patient did not suffer from shock. A mixture containing salol and urotropine was ordered to be given every four hours. Urine commenced to flow from the right ureter immediately after the operation. Seven hours later the patient passed some blood-stained urine by the rectum, and during the night 1 ounce was drawn off by catheter passed into the rectum. The catheter in the right ureter was removed the day after the operation, and the rectum washed out with boracic lotion, and this was ordered to be repeated daily. Two days after the operation he could retain his water from two to three hours during the day, but was troubled with incontinence at night. He gradually acquired more and more control over the flow of urine, until at the end of the month he was able to remaiu dry all day, and only occasionally wet his bed at night.

On May 6th he developed a temperature of 102°, and for the next three weeks his evening temperature ranged between 101° and 104.8°. The child lost his appetite, he became apathetic, and his weight diminished rapidly. The tongue was red and dry, and the abdomen full. A swab from the throat and a "Widal" were negative. The temperature reached normal on May 27th, and from that day he rapidly gained flesh and strength, and was discharged from the hospital on August 31st. He was still troubled with occasional nocturnal incontinence. I believe the pyrexia to have been due to an attack of ascending nephritis.

Second Operation.—He was re-admitted in September, and on September 30th the remaining mucous membrane of the bladder was dissected away. The raw surface was diminished in size by the insertion of transverse Halstead sutures of catgut. The skin was then freed all round and extensively undermined. As the skin could still not be sutured across the raw surface without tension, a curved incision, with the convexity upwards and outwards, was made on each side from the upper limit of the raw surface left by the ablation of the vesical mucous membrane to the groin. The wonnd, when closed, presented a triradiate appearance. At the same time an attempt was made to close the proximal portion of the urethra. An incision was made along the junction of skin and mucous membrane. A flap of the latter was dissected up on each side and sutnred the one to the other. The skin was undermined and sutured over the urethral mucous membrane. The whole wound healed well, with the exception of the small portion lying over the urethra.

Third Operation.—A month later another attempt was made to establish a urethral canal. On this occasion the penile urethra was treated as well.

The mucous membrane was dissected up on each side and sutured. The skin was joined over this. There was considerable tension, and union again failed. I therefore determined to use the redundant prepuce to remedy the condition of epispadias. In the previous operations on the urethra the attempt had been made to form a canal lined by mucous membrane for the possible future escape of prostatic and seminal secretions. This idea was now regretfully abandoned. On December 21st the whole of the urethral mucous membrane was dissected away. An incision was made through the prepuce at its junction with the glans, and it was then stripped from the under surface of the penis. A large flap of skin was thus obtained. It was next buttonholed and the glans penis passed through the opening. The flap was then sutured along the dorsum of the penis. On this occasion the wound healed soundly.

I have collected eight cases which have been operated upon by the method of extraperitoneal implantation of the ureters : two died, giving a mortality of 25 per cent. Orloff has collected fifty-six cases of Maydl's operation with eleven deaths, or a death-rate of 20 per cent. These figures, however, only represent the published and not the real mortality of the operation. I know of several unpublished cases of both operations in which death has ensued. Consequently these statistics are of little use in gauging the relative merits of the two operations. We must, therefore, be guided by other considerations. In Maydl's operation and its modifications it must always be an objection that the peritoneal cavity is opened. The ureter is anatomically an extraperitoneal canal. In Orloff's statistics, of the eleven deaths four were due to peritonitis, and in the forty-five cases which recovered seven suffered from fæcal fistula. Peritoneal infection does not occur in the extraperitoneal operation, and fæcal fistula has not been recorded.

A risk common to both operations is that of ascending ureteritis and pyelonephritis. In the eight cases I record the two deaths were both due to this cause—one five days and the other two months after operation. Of the forty-five cases of Maydl's operation that survived more than three weeks, five died from pyelonephritis. It is argued that ascending infection in Maydl's operation is rendered less likely because the ureters are transplanted without disturbing their terminations in the bladder. As a matter of fact, in the extraperitoneal operation, if as large a rosette as possible of mucous membrane be isolated around each ureteral orifice, very little injury is done to them. Pyelonephritis is therefore no more likely to occur in the extraperitoneal than in Maydl's operation. Borelius has modified the latter operation by performing a lateral anastomosis of the limbs of the pelvic colon and then grafting the trigone of the bladder and the ureters into the end of the loop. It is considered that the lateral anastomosis will prevent the passage of the intestinal contents over the ureteral orifices, and diminish the liability to kidney infection. This passage, however, will occur unless, as has been suggested, the lumen of the afferent limb of the sigmoid loop be obliterated just distal to the anastomosis. Experience will show whether pyelonephritis will be prevented by this manœuvre, which certainly adds to the severity of the operation.

At best the intraperitoneal operation is a tedious and difficult one, and attended as a rule by shock. The extraperitoneal operation, though at first sight difficult, is in reality not so, and in my case at least was not attended by shock.

It is surprising how easily the ureters are separated from their surroundings when once the layers of the trigone have been divided around the ureteral papillæ.

The ultimate results of the two operations are similar. The majority of cases can hold their water for several hours in the daytime. Some cases of nocturnal incontinence occur. The urine causes no irritation of the bowel. My patient, during his journey down to Adelaide to be shown at the Congress, held his water for six hours. He occasionally has nocturnal incontinence; the urine he passes then is clear and not discolored by fæces. The sigmoid flexure is said to form a better reservoir than the rectum. It must be remembered, however, that fæces are more constantly present in the sigmoid flexure than in the latter. When the development of the rectum is considered, it would seem to be a more natural reservoir for the urine than the sigmoid flexure. The weight of evidence, therefore, seems to favor the extraperitoneal operation. When either is successful, there can be no question that a comfortable existence replaces what has hitherto been a most distressing one.

From the patient's point of view the most potent arguments in favor of intestinal implantation of the ureters are :

1. Complete control over the evacuation of the urine is afforded.

2. An irritable and tender mucous surface is ablated.

3. The odor of ammoniacal decomposition of urine, so distressing to himself and to others, is abolished.

Intestinal implantation of the ureters also does away with the possibility of cancer supervening on the mucous membrane of an ectopic bladder. True, the gain in this direction may be balanced by a loss in another, for sufficient time has not elapsed to show whether intestinal implantation tends to cause cancer of the bowel. The following cases show that the supervention of cancer on ectopia vesicæ is no remote contingency :—

Case I.—Keitler* exhibited before a German medical society a woman, aged 55, with ectopia vesicæ and deficiency of the symphysis pubis ; the right ureteral orifice could be distinguished, but not the left. A year and a half previously some tuberous masses were seen on the border of the bladder, and they had developed into a tumor the size of a fist, breaking down at several points. The case was quite inoperable.

Case II.—For permission to use the notes of this case I am indebted to Dr. Poulton, surgeon to the Adelaide Hospital. The patient, a man aged 60, it is interesting to note, had always earned his living by cracking stones. He first noticed a lump forming on the extroverted bladder nine weeks prior to admission. For a fortnight it had been very prone to bleed. He complained of considerable pain, especially down the legs. With the exception of the mucous membrane of the epispadiac penis, the whole of the extroverted surface was replaced by a prominent cauliflower-like mass the size of an egg, which bled freely when touched. The openings of the ejaculatory ducts could be distinctly seen, and exuded glutinous fluid. The ureteral orifices were lost in the growths. Both testicles were completely descended. The inguinal and external iliac glands were enlarged and hard. The condition was quite inoperable, and the patient died a month after admission, exhausted by pain and hæmorrhage. My microscopic sections of the growth and of one of the iliac lymphatic glands proved the tumor to be a carcinoma

In conclusion, I should like to say that abroad sufficient credit has not been given Dr. Lendon, of this city, for having devised this new method. It is usually known as Peters's operation. It is, however, only fair to state that Dr. Lendon performed his operation two months before Peters operated upon his first case in Toronto.

---

* Epitome, *British Medical Journal*, December 16th, 1905.

Cases of Extraperitoneal Implantation of Ureters for Ectopia Vesicae.

No.	Surgeon.	Date of Operation.	Age.	Sex.	Previous Operations.	Complications after Extraperitoneal Implantation.	Results.
1	Lendon, *Transactions Aust. Med. Congress*, 1901	May 12th, 1899	10	Male	Twenty-fivo previous operations	Right ureter slipped out of rectum, and was subsequently grafted into the sigmoid flexure	Recovery. In perfect health six years later. Perfect control over evacuation of urine.
2	Peters, *British Medical Journal*, June 22nd, 1901	July 15th, 1899	5½	Male	—	—	Recovery. Alive two and a half years later, and in perfect health.
3	Lendon, loc. cit.	July 21st, 1901	16	Male	Thirty plastic operations. Castration and radical cure of hernia on left side. Right nephrotomy, right nephrectomy, and later right ureterectomy	Anuria twenty-three hours. The ureter being kinked it was freed and re-inserted into rectum, July 28th. Acute parotitis. Vomiting	Death from pyelonephritis, October 3rd, 1901, two months after operation.
4	Peters, *Canadian Journal of Med. and Surg.*, April, 1902	—	13	Male	—	—	Recovery.
5	Peters, loc. cit.	—	1	Female	—	—	Recovery.
6	Peters, loc. cit.	—	4½	Male	—	—	Death five days after operation from pyelonephritis.
7	Jacobson, *Journal of American Medical Association*, January 3rd, 1903	—	29	Male	Repeated operations	Urine contained blood and pus at operation	Recovery. Alive six months later.
8	Newland, *Australian Medical Gazette*, November, 1905	March 29th, 1904	7	Male	Two operations of uncertain nature	(?) Nephritis after operation	Recovery. In perfect health seventeen months later. Can hold his water for six hours.

TREATMENT OF LARGE VASCULAR TUMORS BY THE INTRODUCTION OF ARROWS MADE OF MAGNESIUM-METAL, AFTER THE MANNER OF DR. PAYR OF GRAZ (AUSTRIA).

By Gavin McCallum, M.B., C.M., Glas.

Mr. President and Gentlemen—It gives me much pleasure to introduce to your notice a case which has been of considerable interest to myself, and I would fain hope to all of you, partly from the apparent hopelessness of the case and partly from a suggestion as to a treatment, of which, I must confess, until recently I was ignorant.

The case is one of a large vascular tumor (*Cavernous angioma*) occupying half the face of a young man.

There seems a general consensus of opinion that these are among the most difficult lesions with which the surgeon has to deal.

The suggested mode of treatment I refer to is that of Dr. Payr of Graz, *i.e.*, the introduction of little arrows made of magnesium-metal into the substance of the growth. The reason for using magnesium-metal depends upon its peculiar chemical properties.

I now show you a photograph of the case. The history is as follows :—

R.G., *aet.* 29 years, bank clerk. When about 18 years of age he first felt a tingling sensation at left half of upper lip, extending to the ear at that side. Later, the cheek swelled slightly, and the trouble was put down to a decayed tooth in the left upper jaw. This tooth was removed, but the swelling did not appear to be affected. The swelling continuing to extend, in about a year an attempt was made to remove it, but, owing to excessive bleeding, only a portion was removed. He was told by the doctor that it was an angioma. The growth continued to extend until within the last two years. During the last two years it has not extended, except for some puffiness at root of nose; and certainly there has been some shrinkage as regards the parts within the mouth. For some time after the operation severe bleedings took place from the nose, and, later, from the inside of the cheek. These bleedings used to take place with only a few days' interval, but have been very rare within the last three years. He has no notion of how it began, unless it was brought about from bad teeth in upper jaw. He did not receive any injury.

On examination there is seen a well-marked swelling of the whole left side of the face, from the lower jaw to the left temporal region, and from the left side of the nose to the left ear. Over the lower jaw there is seen the scar of the operation mentioned above. There is well-marked projection forward of the eyeball, with total blindness. The tumor itself is diffuse and smooth. A puffiness at the root of the nose is evidently due to enlarged veins, and there are seen large veins passing down to the neck from the tumor. In the left temporal region there is a well-marked pulsation, with puffiness. Blue veins can be seen over the anterior portion of tumor, but the pulsation is not so marked, and the skin is fairly natural in color. The tumor extends into left nostril, and blocks it up. It also extends into the roof of the mouth, nearly to middle line. The tumor formation would seem to have occupied all the region occupied by the superior maxillary bone, and to have bulged outward from it in all directions. The bones themselves have been displaced by the tumor in its growth, as can be readily detected on palpation. It would seem as if the growth under zygoma had pushed it outwards, making it occupy the superficial part of the tumor. At the roof of the mouth the left side of hard palate is replaced by the soft mass, except for a ridge along near the line of the teeth.

Hearing is lost to air conduction, but not to bony. The drum membrane is tightly indrawn. The Eustachian tube would appear to be blocked.

The tumor formation would appear to occupy the position of a portion of the distribution of the external carotid artery, and more particularly the internal maxillary branch: There is a well-marked venous thrill to palpation just below the left ear, and a marked venous hum all over growth on auscultation. In the region of the zygoma and cheek the projecting bones prevent the tumor varying in size, but above and below that region great variations take place. Here the dilated blood vessels, which evidently constitute the mass of the tumor, appear at times to empty themselves, and at other times to become quite engorged.

In a reference notice to a German journal I observed that Dr. Payr of Graz, in Austria, reported a similar case, which has been very much reduced in size by his method of introducing magnesium darts or arrows. He inserted forty-two in six sittings. Pieces of this metal introduced into the living tissues produce certain definite chemical changes, which affect the absorption of the metal. Under its action the watery constituents of the tissues are reduced to the original elements, oxygen and hydrogen. The gases evolved accumulate around the metal, the oxygen combines with it, and forms a soluble magnesium oxide. The hydrogen is absorbed. The arrows are absorbed in from two to three weeks.

After injection the swelling gradually loses its softness to the touch, and in the course of a few days gaseous crepitation can be elicited around the foreign substance on pressure. This is apparently due to hydrogen, which is gradually evolved and then absorbed. Obstruction to the circulation takes place apparently over a considerable area.

Gradual cicatrisation follows in the area, with consequent diminution in size. The treatment is practically painless when done undel local anæsthesia, and seems to cause no ill effects, local or general. The patient can go about as usual.

By successive introductions large swellings may be thus reduced.

I noticed that the scar from the operation wound along the lower jaw in my case prevented the spread of the tumor downwards. On thinking over it, I determined to adopt this method of Dr. Payr's, and first of all introduced strips of magnesium ribbon half-inch long. I found the effects just as described by Dr. Payr. I placed myself in communication with him, and received the following reply :—

" Dear sir—I am glad to hear that you are using my method in the treatment of disease of blood-vessels. I am using myself only small needle-like arrows, pointed at one end and blunt at the other, which are inserted into the tissue by means of a tube and small rod. In case of need one can insert also arrows cut out of a magnesium ribbon, but they are far less efficacious, because the ribbon form of magnesium represents but rarely an absolutely pure form of the said metal. Arrows of magnesium, as I am in the habit of using, are to be got at Messrs. Rohrbeck Wien, 1 Kartnerstrasse 59. I enclose a copy of an article written by me for the *Centreblatt fur Chirurgie*, from which you will infer all the necessary further details of my operative *technic*. I have treated, from the time of those publications to this day, eleven cases of cavernous angioma, the results of all of which are excellent. In cases of a very large tumor there remains the spongious tissue, being shrivelled, sometimes a rather considerable nest of fibrous connective tissue ; cosmetic motives may at times lead to the excision of it. The largest angioma treated by me was about two fists in size, and occupied nearly half of the face of a young man. The recovery was nevertheless complete."

I obtained from Dr. Payr his own apparatus, and have since been using it, injecting about half a dozen at a time. There is already well-marked improvement in the case, testified by appearances and by the patient's own feeling, both inside and outside the mouth. The patient himself is quite enthusiastic about it, and I injected half a dozen the day before I came away.

The length of treatment depends solely on the area involved. Any part of the tumor may be reached from the small opening made by trochar and canula.

The arguments in favor of this method would seem to be :—

1. It may be a gradual means of cure of large and otherwise inoperable tumors.

2. There is practically no scarring.

3. It seems to be a means at one's disposal of reversing the method of growth of those tumors, and causing a gradual cicatrisation.

4. By rendering the tissues less vascular, it may at a later date be operated on. It will thus be seen that this treatment neither excludes or forms a barrier to any other means of treatment. Excision becomes easier, less dangerous, and does not cause the same loss of blood. Galvano-cautery, thermo-cautery, electrolysis, and other methods of injection may all be employed without interfering with this treatment.

5. Only sub-cutaneous cavernous tumors are suitable for this treatment. I have referred to the skin of my patient having a fairly normal appearance, indicating its sub-cutaneous nature.

6. Dr. Payr considers the following vascular tumors unsuitable for this treatment :—Cutaneous angiomas and telangiectases, mixed vascular tumors, angiolipomas, angiofibromas, &c. These, as a rule, can be treated by other means.

7. Dr. Payr is of opinion that for cases such as my patient his method is the only one, and, at the same time, a very hopeful means of treatment in an otherwise hopeless case.

8. Dr. Payr has never had any trouble from embolism, or from the thrombotic process extending into the neighboring tissues. He has made a great many experiments on animals, and made a careful examination with regard to these possibilities. In the great number of patients whom he has now treated, he has never had any trouble. The bubbles of hydrogen gas in the tissues never caused the slightest anxiety.

CASE OF PAPILLOMA OF THE KIDNEY.

By A. A. Lendon, M.D., Lond.,

With Pathological Notes by W. R. Cavenagh-Mainwaring, M.B., Adel., F.R.C.S., Eng.

Mr. H., a pastoralist, was first discovered to have albuminuria in November, 1898 ; he was then 75 years of age, hearty, and well in most other respects. The urine had a specific gravity of 1020 ; it was acid and free from sugar ; in the slight sediment deposited, on standing, were seen, under the microscope, a few pus cells and some fine oxalate of lime crystals ; the quantity of albumen was found to vary from a faint trace to a marked ring by Heller's test. On February 24th, 1899, blood was found in the urine for the first time, but in small amount ; the smoky tint of the specimen suggesting a renal rather than vesical origin for it. The patient was noticed to be sallow-looking, but his

weight was inclined to increase from its minimum of 9 stone 9 pounds, his usual weight being 10 stone 4 pounds. In the November of that year there was an attack of hæmaturia, spread over several days. It was attributed by the patient to a severe bout of influenza ; no pain accompanied the bleeding, nor was there any frequency of micturition ; rest in bed did not seem to appreciably diminish it, but it appeared to be best controlled by the internal administration of turpentine in capsules. From this date the bleeding became of a fairly frequent occurrence ; a mild attack lasting a couple of days, and a severe one a couple of weeks perhaps. The intimate admixture of the blood with the urine, and the absence of clots, still supported the suggestion that it had a renal origin, and an endeavor was made, by sounding the bladder, to exclude the possibility of its being derived from that viscus. An interval between the attacks was chosen for that purpose, but nothing was detected, nor was any fresh hæmorrhage provoked. There was no renal enlargement nor tenderness. Sometimes in former years the patient had suffered from attacks of lumbago without hæmaturia, and now he often complained of a little pain with tenderness over the lower dorsal vertebral spines. In October, 1901, the correct diagnosis was thought to have been established, because the patient had a severe attack of left renal colic, which required hypodermic injections of morphine, and the continuous administration of chloroform for its relief. The bladder was again sounded after his recovery from the attack, but no stone found, so that it was assumed to have remained behind in the renal pelvis. This attack and its accompanying bleeding were supposed to have been precipitated by a ride on a rough horse on the previous day. The temperature was not raised, nor did the patient have a rigor.

When just 79 years of age, Mr. H. had an attack of suppurative appendicitis, and after he had recovered from his operation he began to put on weight, and finally reached 10 stone 12 pounds—a heavier weight than he had been for many a long year. In spite of his general and progressing weakness and the frequent hæmaturia, the weight had not materially decreased when he was last weighed in February, 1904.

In June, 1903, a correct pathological diagnosis became possible, because for the first time a piece of papillomatous growth was found amongst some blood-clots in the chamber. The renal colic two years previously further pointed to the passage of a piece of the growth down the ureter. Since that event, too, the bleeding had been observed to be much freer in character, and sometimes the passage of clots from the bladder gave rise to pain.

In May, 1904, an attack of bleeding, more profuse than ever, was followed by cystitis, with decomposition of the urine, and death ensued from exhaustion. A partial *post-mortem* examination was made, and the left kidney and the bladder were submitted to my colleague, Dr. Cavenagh-Mainwaring, for examination and report.

One remarkable feature about the case is this, that when in town the patient resided in a suburban street where the only other cases of papilloma of the urinary organs which I have met with during twenty-two years of practice in Adelaide also lived—one next door to him, and the other further up the street. Both cases were reported in the transactions of the South Australian Branch of the British Medical Association (*Australasian Medical Gazette*, 189_, pp. 171 and 174 ; also 1894, p. 300).

The question naturally arises as to whether an accurate diagnosis—anatomical and pathological—might not have been arrived at sooner. At the outset it might be said that the reported cases of this disease are but few, Dr. Drew in 1896 being only able to find records or specimens of eight instances. Again, neither Dr. Murchison nor Mr. Campbell de Morgan, in 1869, diagnosed their cases during life, nor was Mr. Barker's case in 1896 detected,

even after a nephrotomy; Mr. Knowsley Thornton's was only found out after nephrectomy. On the other hand, Mr. Jones's case and Mr. Battle's were both diagnosed by means of an exploratory nephrotomy, followed in each case at a subsequent date by removal of the kidney.

But it may be said that with cystoscopes, urine segregators, and X-rays we have advantages over the men of the nineteenth century. This is true; and all these means should have been employed, had it been possible. But the patient was elderly, and objected to manipulations. He experienced no pain, except during the one attack of colic, and he argued that he was too old to be operated upon even if a stone had been found in the kidney. When the piece of growth was passed he was undoubtedly too weak for such a thing to be contemplated. In a younger man the cystoscope might, and probably would, have anticipated the discovery that the bladder was free from disease, and that the lesion was situated in the left kidney: the separator might, and probably would, have given us the same information. Private patients, however, and especially those who are elderly, cannot be treated quite in the same imperative manner as hospital inmates, greatly to their own disadvantage. An early diagnosis in such a case (had the patient been younger) would have been all-important: the kidney might have been removed, as was done successfully by Mr. Battle in a man 53 years of age.

PATHOLOGICAL NOTES BY DR. CAVENAGH-MAINWARING.

The left kidney was increased to about one and a half times its normal size, and consisted of two portions. The upper portion, forming about two-thirds, was dilated into a thin-walled sac, with thin walls somewhat resembling the capsule of a hydatid, and about $\frac{1}{4}$ inch in thickness. Its interior was filled with a bloody mass of mixed fibrin and débris, and scattered over the inner surface were numerous shaggy masses of papillary growth, infiltrating the wall of the sac at their base, the whole being divided by incomplete partitions into three saccules. The lower third comprised a single saccule, the walls of which were formed of healthy kidney substance enclosing a somewhat dilated pelvis, into which four flattened papillæ projected. The internal wall of this portion was smooth, except at one spot above where the cavity communicated with the sac of the upper portion, where was placed a single compound outgrowth similar in nature to those found above. The two portions were drained by separate ureters; the upper one was dilated, and with thicker walls than the lower one, whilst scattered all the way down were numerous implantation papillomata. The lower one was somewhat smaller than normal, with unthickened walls and a smooth lining. Three ureters had been torn away from below from their entrance into the bladder, so that it was impossible to tell whether they had a double entrance there or not; but it appeared more probable that they coalesced close to that viscus.

The bladder itself was somewhat thickened and contracted; its mucous membrane presented some general thickening and roughness, most marked in the neighborhood of the opening for the left ureter, but the walls beneath presented no evidence of new growth. Posteriorly the bladder was adherent to the rectum by a thickened mass, apparently inflammatory in origin.

Microscopical sections through the wall of the upper dilated portion of the kidney showed that it was composed almost entirely of new growth, the proper kidney substance being represented only by a narrow fibrous stratum outside this. The growth itself showed externally an abundant fibrous stroma, arranged in the form of alveoli, containing solid columns of cubical cells, resembling the glandular epithelium of the tubules, but more internally the alveolar structure became less marked, and the character of the cells

became changed, in places becoming elongated and flattened, and somewhat resembling endothelium, and in others round and oval, with a general resemblance to sarcoma elements. When the actual relation of the stroma to the cells was carefully observed it was found that though the general appearance was an alveolar one, similar to that met with in carcinomata, still in nearly every instance the cells were not entirely separate and lying loose in the alveoli, but were at some point in intimate relation with its walls, at which point the stroma appeared to run into the mass of cells and to become developed directly into them.

From a pathological standpoint the most interesting question in this case is the nature of the growth; that is to say, whether it should be placed amongst the carcinomata or epithelial malignant neoplasms, or amongst the sarcomata or mesoblastic malignant tumors. The kidney is apparently partly developed by an outgrowth from the Wolffian duct, which is sometimes double, and partly by the development of glomeruli and tubules in a portion of mesoblast known as the intermediate cell mass, the two portions subsequently becoming connected with one another.

The ureters, pelvis, and collecting tubules are probably developed from the former (and when that is double there will be two ureters); the glomeruli, convoluted tubules, and rest of the kidney from the latter. Embryologists differ as to the exact origin of the Wolffian duct. It appears at a very early period of development as a thickening growing on the one hand in connection with the intermediate cell mass and with the epiblast on the other. This thickening soon obtains a lumen, and grows down along and at the expense of the epiblast to open posteriorly into the cloaca.

According to some this tube is derived primarily from mesoblast, whilst others think it is derived from epiblast, and becomes connected with the intermediate cell mass secondarily. Under the first theory the epithelium of the pelvis, ureters, and collecting tubules of the kidney would have a mesoblastic origin; under the second, an epiblastic one. This difference of opinion gives a special interest to the morphological study of the epithelioid tumors of the kidney, as possibly throwing some light upon the disputed question.

A carcinoma may be roughly described as an abnormal proliferation of epithelium in some part of the body, which is atypical in nature and situation and of unknown causation; and a sarcoma as a similar overgrowth of the connective tissue, the tendency of the new-formed tissue being to revert to the embryonic form and to become cellular rather than fully formed. In the carcinomata the stroma is not affected primarily, though a secondary irritative overgrowth may occur. The epithelial cells are the intrinsic and distinguishing features of the growth, and groups of these cells are found in close approximation with the intervening connective tissue, the groups lying in, but separate from, spaces in the connective tissue known as alveoli, the walls of which may, however, become infiltrated with the epithelial elements at a later period. The blood vessels formed in connection with such tumors have fully-formed connective tissue walls, and are only infiltrated secondarily.

In the sarcomata, on the other hand, the cells are directly derived from the connective tissue that forms the stroma of the neoplasm, and, consequently, the individual cells have a much closer relation with that stroma, and even when the alveolar structure of a carcinoma is closely imitated do not lie free in the alveoli, but are in places in intimate relation with their walls. The same tendency to reversion to the embryonic form is seen in the walls of the blood vessels, and these are, consequently, frequently actually composed of tumor substance, and the cells composing it gain a ready and early access to the blood stream, more especially the thin-walled veins. Consequently metastasis and secondary growths in sarcomata are frequently through the

blood stream, especially to the lungs; whereas the carcinomata have to wait till they have penetrated into the vascular system before becoming disseminated; and as the lymph vascular system has the closest relations to the elements of the tissues, the path of dissemination is more frequently through those than through the blood vessels, though the latter may occur. The general opinion of pathologists as to the nature of these growths has varied from time to time. Previous to the work of Waldeyer the accepted opinion was that they arose by the proliferation of the connective tissue elements of the part; but since his researches the opinion has been widely accepted that they originated primarily from an abnormal proliferation of the epithelium. In a very able and interesting paper on the subject, delivered at the Brisbane Congress by Professor Allen and Dr. Cherry, the earlier view was upheld, and the histological features of this case are, I think, distinctly in support of the views expressed by them.

The first impression received on examining sections of the growth under the microscope is that it corresponds to the carcinomatous type of tumor; but, on closer examination, it is seen that though the cells be in definite alveoli, and free from the stroma wall in a large part of their circumference, still, generally, they are in contact with it in some part, and at that spot fibrous prolongations extend from the stroma wall into the midst of the cell masses, and are so intimately related to them that in many instances the cells seem to be developed directly out of the stroma. The type of cell varies considerably in different situations, and all gradations can be observed between those typically epithelioid down to those resembling ordinary sarcoma elements. The structure of the blood vessels inclines towards that met with in sarcomata, and in places the walls are composed of cells similar to those met with in the tumor proper.

The question of secondary growths was not settled at the autopsy, but it is an established fact that in similar tumors these frequently occur by the medium of the blood stream rather than the lymphatics, thus conforming to the sarcomatous rather than to the carcinomatous type of tumor; and, on the whole, it is my opinion it is in the former class rather than in the latter that such growths should be included. The condition of double and two separate portions of the kidney is an interesting one; and the extensive involvement of the upper portion and its ureter, and practical freedom of the lower portion, points strongly to the probability that, whatever be the causation of malignant growths—whether parasitic or a reversion to a germinal condition of the tissue—local conditions play an important part in the actual site of the growth.

REFERENCES:

Australasian Medical Gazette, 1891, pages 171 and 174.
Australasian Medical Gazette, 1894, page 300.
Path. Soc. Trans., vols. xxi. and xlviii.
Clin. Soc. Trans., vol. xxviii.
Trans. Intercolonial Med. Congress, Dunedin, 1896.

SECTION OF GYNÆCOLOGY AND OBSTETRICS.

PRESIDENTIAL ADDRESS IN THE SECTION OF OBSTETRICS AND GYNÆCOLOGY.

By William S. Byrne, M.D., M.R.C.P., London, Brisbane.

Gentlemen—First of all, on behalf of the Executive of this Congress, allow me to welcome you to this the section of Gynæcology and Obstetrics; branches of our science and art which, during the past ten years, have made advances unexcelled by any part of the glorious profession of Medicine and Surgery.

The honor which has been conferred upon me by electing me President has been accepted with a full sense of my own shortcomings; but, at the same time, as a compliment paid not alone to myself individually but to the medical profession of the State to which I belong. When I look back upon the names of past presidents of this section I confess to a feeling of pride that I am considered worthy to follow a train of such eminent men.

Gentlemen, the history of Gynæcology is the history of Surgery, and the history of to-day depends upon the alphabet and language of yesterday. The practice of medicine is giving way largely to the art of surgery, and judging from the trend of comparatively recent events an elderly practitioner of the present day would be tempted to remark that the modern practice of medicine is surgery. And how true it is. I suppose most of us remember, in the old days when we were boys, being informed that Mrs. or Miss So-and-so was ill, or, in the words of that day, "delicate"; and we noticed that the lady in question was continually on the sofa, or in bed—being taken out for a drive on fine days, possibly—and that this sort of thing went on for years, and then our recollection of what happened grows faint and the case of Mrs. or Miss X has quite faded from our memory. What happened in those days to all the cases of pelvic adhesions, diseased tubes, pus cases, intractable retroversions and tumors? How often nowadays do we hear the remark from elderly ladies that in their time there were no operations, inferentially meaning that as none were performed in their day none ought to be done now? But our critics quite forget the years of chronic invalidism suffered by women in former times, and measure the ills of others by their own good health. The gynæcologist has been the pioneer in the forest of abdominal surgery since the time when Ephraim McDowell performed the first successful ovariotomy; but progress was slow, until the discoveries of Lister and Pasteur enabled surgeons to perform their work without fear of septic peritonitis. All our sympathies go out to Baker Brown in the early days of the Samaritan Hospital, when he abandoned sections on account of septic poisoning; and it is difficult for us now to appraise, at its true value, the work done, the courage displayed, the indomitable determination and the enduring perseverance shown, in spite of all obstacles thrown in his path, by that surgeon of surgeons, Spencer Wells. When we consider that not alone the public but even our own profession were violently opposed to the new procedure in the method of saving life by the removal of ovarian tumors, and that he was even threatened with a court of inquiry if a patient died after operation,

his marvellous determination to succeed and his disregard of the bitterest attacks made upon him point him out to be not alone a surgeon amongst surgeons but a man amongst men.

Whilst acknowledging the brilliant gifts of Spencer Wells, let us not for one moment forget the still greater achievements of Pasteur, and our own countryman, Lister. Had it not been for Simpson and chloroform there would never have been a Wells; so, without a Lister and a Pasteur surgery would have remained simply the handmaiden of medicine, instead of becoming the virile and aggressive—but withal the kind—master.

Medicine and surgery are wedded, and the greatest triumphs of recent years have been won in the field of pelvic surgery.

Many of those amongst us will remember an article published not so many years ago entitled "A plea for the early removal of ovarian tumors"; and although we may look, with our present day experience, with some amusement at the title of that paper, yet it is an index to the feeling of the profession at that period. At the present time one is almost tempted to write "A plea for the early removal of uterine fibroids." It is still the tradition amongst physicians to view myomata as innocuous, harmless, and productive of but little discomfort; but the sooner this feeling is dispelled the better for our patients. Tradition is a good thing to have if we can only afford it, but in the treatment of fibroids it is apt to prove expensive. About five years ago a London specialist, in charge of the women's department at one of the large hospitals, published a paper emphasizing this tradition, and showing statistically that deaths from fibroids were most exceptional, and that removal except in urgent cases was not justifiable. I am glad to say that professional opinion is slowly veering round to surgical interference, and personally I strongly advise early removal. That uncomplicated myomæ can be removed safely at an early stage is beyond all question: the mortality is little more than that of an ordinary ophorectomy. The longer a tumor is allowed to develop, to contract adhesions, to block up the pelvis, to lessen resistance by continuous hæmorrhages, the more difficult and dangerous the surgical procedure becomes.

Who amongst us has not seen a wan, pallid woman—emaciated, unable to move unless it is to a couch, exhausted by continuous hæmorrhage, suffering from all the concomitants of the disease, a face drawn and disfigured by long and abiding discomfort, who has had drugs *ad nauseam* likely to alleviate the symptoms, and who, as a last resort, is handed over to the surgeon for operation? What are the probabilities of recovery from a long and difficult operation on such? We may view such a case with apprehension; but our duty is clear—we must afford the possible chance. Had the case been given to us in the early stage how different would our feelings have been on approaching operative measures. This is no fancy picture, but is fairly familiar to us all. When we know the primary dangers of fibroids—namely, malignant degeneration, decomposition and gangrene, chronic nephritis, phlebitis, embolism, the dangers of pregnancy and labor, and the secondary dangers produced by hæmorrhages, debility, and all the discomforts of an invalid life, rendering the subject an easy prey for intercurrent disease—we are warranted in looking on a myoma with the gravest apprehension and advising removal at a time when such procedure is not especially dangerous. The statement that fibroids do not kill may be true enough, but one might with the same confidence say that typhoid fever never kills: of course it does not, but the unfortunate patient dies all the same—of something else—either of pneumonia, exhaustion, heart failure, peritonitis, perforation, and a host of other complications, according to the fancy of the physician; but never of typhoid.

CURETTAGE.

Amidst all this comparative safety in abdominal work, owing to the perfection of aseptic surgery, let us for a moment consider a minor operation which is performed daily, namely, curettage. I must confess I am always more uneasy and anxious after this so-called trifling operation than I am after a clean abdominal section. Salpingitis, pelvic abscess, chronic pelvic peritonitis, and a host of other ills follow in the wake of this procedure; and we are all cognisant of cases in which the cause of severe pelvic disease is the simple operation of curettage. The reason is not that the operation was improperly performed, nor that asepsis was not thoroughly carried out on the operator's part, but that in the majority of cases where it is necessary the interior of the uterus is already intensely septic; and as there are no means by which it can be made clean, the curettage opens up new avenues for infection, and a septic inflammation which was at first limited to the uterine interior extends to all parts of the pelvis. In another class of case there has been an old inflammation about the appendages, and the operation lights up the smouldering fire afresh, and much damage is done before the flames are extinguished. Curettage is not an operation to be lightly thought of or lightly undertaken—particularly in cases of old pelvic trouble—but rather to be considered with a full apprehension of its after effects; and, while not restricting its use to the specialist, I would warn the general practitioner that he may be called upon to perform a difficult vaginal or abdominal operation as a direct result of the primary interference.

RETRO-DEVIATIONS OF THE UTERUS.

The operations for the cure of retro-deviations of the uterus are becoming narrowed down to Kelly's ventro-suspensions, or fixation, and the Alexander Adams. Each procedure has its own especial advocates; but I cannot help thinking that, for the great majority of cases, a section is the better course to pursue. It is a great responsibility to condemn a woman to pessary-life; and I do not think I have inserted one of those awful implements six times during the past two years. The Alexander Adams operation is suitable enough for simple uncomplicated versions where there is no pelvic disease; but it is wonderful what a lot of unconsidered trifles there are in a pelvis when one has a good look at it. It does not quite lie with the operator whether a suspension or a fixation is to be the result; for I have meant to suspend on certainly three occasions, and years later, on having to open the abdomen for some fresh condition, I have found the uterus firmly fixed to the abdominal wall. In retro-deviation during the child-bearing period, when a fixation is a somewhat risky proceeding to adopt, I have been in the habit for some years of doing the following operation:—After the parts have been well exposed and any extra uterine trouble attended to, I free the round ligaments from the uterine attachment for about 2½ to 3 inches on each side, bore a hole with a pair of forceps through each broad ligament, pull the loops of round ligaments through, tie them together with linen thread at the back of the fundus, and insert a few catgut stitches through the now doubled ligaments and uterine peritoneum. This draws the uterus well forward, the ligaments are shortened at their strongest parts, and the risks of a future pregnancy diminished to a minimum. The operation is a little more difficult than a suspension or a fixation, but it seems to be more rational during the child-bearing period. I do not wish to claim priority for this little addition to the already great number of procedures in the treatment of versions, as a paper appeared in the *Australian Medical Gazette*, from the pen of a gentleman in West Australia, some months ago, in which much the same operation was described.

LINEN THREAD.

As I have mentioned linen thread as a ligature and suture material, let me say that I regard it as superior to silk. It is finer, stronger, easily sterilised, can be boiled over and over again without diminishing its strength, it is inelastic, and does not break easily. In the Lady Lamington Hospital it has quite taken the place of silk for ligature material; in fact we have not used silk for a very long time. Of course catgut is the ideal material for buried suture, but where silk used to be always in evidence I now prefer linen.

ESERINE AND ATROPINE.

I have found the hypodermic injection of one two-hundredths of a grain of atropine just before an abdominal section, and one-fortieth of a grain of salicylate of eserine immediately after, to be of great comfort to patient and surgeon. It starts the peristaltic action of the intestines, subdues vomiting, and lessens the difficulty of procuring an evacuation on the following day—a great desideratum in abdominal work. Some months ago I wrote a short paper on the subject, which appeared in the *Australian Medical Gazette*, and further experience in the use of those two drugs has confirmed me in my opinion of their efficacy.

PUERPERAL AND ECLAMPSIA.

I was about to direct your attention to the treatment of puerperal eclampsia, but as I see there is a paper promised on this subject we shall be able to discuss it later. It has been aptly termed the disease of theories, and we are unfortunately no nearer the solution of the actual cause than we were when the connection between albuminuria and puerperal convulsions was first observed. Pathology does not lighten our darkness very much. In all cases we find parenchymatous nephritis with hæmorrhagic necrosis in the liver, thrombosis being primary and necrosis secondary; thrombi in the kidneys and lungs, pneumonia being common; hæmorrhagic extravasation between the muscle fibres of the heart, hæmorrhages in the stomach and intestinal walls and central ganglia. How much these extravasations depend upon the violence of the convulsions, and how much upon disease of the vessels arising from the circulation of highly toxic blood, it is impossible to say; probably both conditions favor their occurrence; but the main point is that the *post-mortem* appearances do not help us toward the solution of the *fons et origo* of the disease—they are only the effects. Can a chronic nephritis in a pregnant woman set up a puerperal nephritis, and can a puerperal nephritis cause a chronic nephritis? The general opinion answers these two questions in the negative, and I think that for our purpose we can consider puerperal nephritis a disease, apart altogether from the ordinary kidney inflammation. If recovery ensues after eclampsia the albuminuria disappears in the course of two or three weeks; and clinical experience shows that if the fœtus dies, as a rule the convulsions cease—that is, if the dose of the toxin has not been too large to be readily eliminated. Again, as in the acute nephritis of men and non-pregnant women, the so-called uræmic convulsion is not very common, whilst in the acute nephritis of pregnancy it is the rule; it is strongly suggested (although the kidneys are always seriously and acutely affected) that pregnancy is necessary for those eclamptic seizures, and, therefore, the real cause of the disease must be sought for in the fœtal or material structures *in utero*, and not in the kidneys, though both conditions are necessary for the production of the eclampsia. Intoxication, with the products of fœtal metabolism, seems to be the most feasible explanation of the entrance of the toxins into the blood; though one may be met with the objection that, if so, why should all eclamptics consist of 80 per cent. of

primiparæ? I cannot help thinking that, somehow, nature after the first effort accommodates itself to conditions which have previously existed, and enables the excretory organs to eliminate the poisons.

In the treatment prophylaxis is the first consideration; and if we can prevent the onset of the convulsions our chief end is secured. The occurrence of a small quantity of albumen is by no means uncommon in pregnancy, so that although this sign by itself is not of necessity a danger signal, still it is enough to cause us a certain amount of anxiety as to future events. Purgation, a milk diet, and rest will probably tide us over the difficulty; but should a large amount of albumen be present or albuminuria, accompanied by œdema, or headache, or fainting, or visual disturbances, hyperemesis, or persistent hiccough, we may be fairly sure that eclampsia is not far off; and our duty is to terminate the pregnancy at once. It is not good practice to temporise longer; the sooner the feeding of the blood with toxins ceases the better. We should look upon the fœtus and maternal structures in this case in the same way that we look upon a decomposing placenta *in utero*—an offending substance, to be got rid of as soon as possible. Temporising where eclampsia is probable is a fatal policy; purgation, sweatings, diuretics and drugs of all kinds are useless as long as the cause is allowed to remain. It reminds one of the parson's remark when asked to pray for rain: he replied, "It's not a bit of good while the wind is in the west."

Eclampsia may occur *ante, intra,* or *post partum*. *Ante partum* eclampsia demands evacuation of the uterine contents as soon as possible, and the character of the convulsions influences the methods to be adopted for this purpose. If slight and not recurrent, one may be satisfied with passing two or three bougies into the uterus; or, if some more certainty and haste is requisite, dilatation and the introduction of de Ribes' bag. If the convulsions are very pronounced, and immediate delivery imperative, rapid dilatation by means of Bossi's instrument is indicated. Should it be impossible to dilate the canal to the required extent with a reasonable degree of safety, then Cæsarian section, either abdominal or vaginal, must be considered; and I am convinced that, in many cases of rigidity of the canal, section is safest and best for the patient.

Eclampsia *intra partum* must be treated on the same lines—extraction of the child and placenta at the earliest moment practicable, having due regard to the safety of the maternal structures.

In eclampsia *post partum* we have to deal with the results of toxæmia, and our efforts must be aimed at controlling the convulsions and eliminating the toxins. To fulfil the first condition morphine, in fairly large doses, commencing with half a grain, is to be recommended; in acute nephritis morphia can be given freely. It is in the chronic granular kidney that its use is contra-indicated. As well forty grains of chloral per rectum, repeated in an hour, is useful. Chloroform is of doubtful utility, though by its administration the eclampsia for a time is lessened; but its continuance for any lengthy period is harmful.

To eliminate the toxins free purgation is requisite, and I know of nothing better than croton oil dispensed in butter.

Where there is a bounding high-tension pulse venesection is indicated; but infusion of salt solution into a vein must be cautiously approached, as it increases blood pressure and may defeat the object we have in view. Pilocarpine has been to me a disappointing drug. I never could satisfy myself that it had any effect on the convulsions, though, theoretically, profuse perspiration ought to help in relieving the kidneys. If it is given, nothing much less than a third of a grain is of service.

In conclusion, let me say how much I deprecate the opinions sometimes expressed, that nothing is any good for established eclampsia, *intra* or *post partum;* that we know nothing about it; and that our treatment, being empirical, is not likely to be of any use; that we should not deliver, but give baths and drugs, and pursue an expectant policy.

Gentlemen, pessimism in the treatment of puerperal eclampsia is to be condemned: want of faith in ourselves is the handmaiden of failure.

TREATMENT OF PELVIC SUPPURATION.

By Ralph Worrall, M.D., M.Ch.

Mr. President and Gentlemen—The opinions which I have ventured to express in this short paper are based upon my experience for many years past in the Sydney Hospital, and in private practice; but, for the purpose of illustrating my remarks, I have analysed and brought under your notice only the cases in which the abdominal cavity has been opened for suppurative conditions in the Sydney Hospital during the year 1904. I have done this because, in the Sydney Hospital as in all other well-regulated hospitals, the hon. medical staff collectively and individually have nothing whatever to do with the compilation of the annual report, a paid registrar being kept for this purpose. In the gynæcological report this, I fear, is but too evident from the out-of-date and confused nomenclature adopted. The report is sent each year to every registered practitioner in Sydney, and the work thus subjected to enlightened criticism. Anyone interested can verify the statistics with little trouble.

2. Of all the pathological conditions for which the abdomen is opened pelvic suppuration is the most frequent, and probably also the most dangerous and difficult.

In 110 cœliotomies performed by me in the Sydney Hospital last year forty-five were for pelvic suppuration in one form or another. I include in the term "pelvic suppuration" acute purulent salpingitis, abscess of the ovary, tubo-ovarian abscess, suppurating ovarian cysts, abscess in the uterine wall, in the cellular tissue of the pelvis and the pouch of Douglass (local circumscribed purulent peritonitis). In these forty-five cases of pelvic suppuration there were—Pyosalpinx 17, tubo-ovarian abscess 8, pelvic abscess 7, pelvic suppuration 4, acute purulent salpingitis 3, suppurating ovarian cyst 2, suppurating intra-ligamentary cyst 1, ovarian abscess 1, suppurating cellulitis 1, abscess of uterine wall 1.

3. In the forty-five cases there were two deaths; and as from fatal cases we usually learn most, I shall briefly touch upon them.

(1) Virulent streptococcus infection, with high pyrexia at time of operation. Vaginal cœliotomy was done first and several ounces of pus evacuated. As there was no improvement abdominal section was done a week afterwards, and bilateral tubo-ovarian abscesses removed. There was intense pelvic peritonitis and much exudation of lymph. The bowels were well moved afterwards, and there was no vomiting, but distension continued; insomnia was marked, and the patient became delirious, and died thirty-six hours after the second operation. Her history was that she had been curetted a week before admission for pelvic pains, which had troubled her for several months. There had been an abortion twelve months previously. A *post mortem* was made without throwing more light upon the case. It is evident either that

the patient was poisoned at the time of the curettage, or that, more probably, this caused a leakage from pus sacs present since the abortion. [N.B.—By pelvic suppuration I mean a distribution of pus more extensive than in pelvic abscess.]

(2) The second death was a case in which criminal abortion had been produced ten days prior to admission. On the same day she had been curetted by one of the ostracised members of the profession. On admission to the Sydney Hospital, as an urgent case, an extensive sloughing wound of the anterior lip of the cervix was discovered. The temperature was 103, pulse 130, and resp. 36. Vaginal cœliotomy was immediately done, and about 2 ounces of pus evacuated ; at the same time a considerable quantity of decidua was removed by curettage. Six days afterwards, but little improvement having taken place, the abdomen was opened in the middle line. The appendages were found to be acutely inflamed, but not cystic ; the uterus was soft, friable, intensely congested ; the left broad ligament contained an abscess the size of a large pigeon's egg ; all the structures of the lower abdomen were congested and roughened. The uterus, appendages, and left broad ligament were removed. Death occurred in forty-eight hours. Numerous streptococci were found in the pus of the abscess cavity. A *post-mortem* examination disclosed nothing further. This was evidently a lymphatic infection, possibly from the wound in the anterior lip of the cervix. Both these cases are examples of the dangers of curettage undertaken without adequate care and knowledge.

Another case of special interest was one in which both streptococci and pneumococci were found in the pus of a bilateral pyosalpinx, and also in fluid which was twice drawn off from the right pleural cavity. The patient recovered completely, after extensive suppuration of the parieties. The origin of the trouble was undiscoverable.

With regard to the bacteriology, no note was made in twenty-one cases, which I much regret. In the other twenty-four there were no organisms found in the pus in nine, streptococci were found in five, B.C.C. in five, gonococci in four, and streptococci with pneumococci in one.

All the cases in which streptococci were discovered in the pus were clinically most serious ; but, on the other hand, one or two cases in which the bacteriologist reported there were no organisms were equally critical, and at the operation showed as much congestion, softening, and friability of structures. The discovery of gonococci in only four cases inclines one to think that gonococci pus soon becomes sterile.

An attempt was made in each case to ascertain the origin of the trouble. In fourteen there was a history of gonorrhœa ; in five of puerperal trouble ; in three there had been recent delivery (all these proved to be serious cases) : in six there had been recent abortion, and in three recent criminal abortion (these were also all serious cases) : two were traumatic infections from curettage outside hospital ; two were ovarian cysts with bowel infection ; two were tubercular ; two were uncertain origin, although streptococci were found in the pus. In the remainder the cause was undiscoverable. Pyrexia was present at time of operation in twenty-four cases, and within the week prior to operation in six others. The leucocytic count was increased in all the acute cases. One or both ovaries were not removed in fifteen cases. One tube appeared healthy, and was not removed in six cases. Two years ago I removed a gonorrhœal pyosalpinx, leaving the other tube, which appeared quite healthy ; within a fortnight there was exacerbation of symptoms, for which the abdomen was again opened and this tube found distended with pus. Notwithstanding the risk of such an occurrence, I think it justifiable to leave a tube which is apparently healthy.

In three cases only of the forty-five was there cellulitis with pus tubes. It was in the form of expansion of the broad ligaments from exudation into the cellular tissue.

There was one case of suppurative cellulitis without pus tubes, and it was cured by evacuating the pus through an incision, as for trying the external iliac artery. In every case curettage was done at the same time as the section, and in one instance the uterus was perforated by the curette; this was at once recognised, and no intra-uterine injections made. The case was one in which all the pelvic structures were much softened by septic processes, and would in any circumstances have required the removal of the uterus, which was done. The patient recovered. The uterus was thus removed with the appendages ten times: on eight occasions because it was so greatly involved in the infection as to make it appear dangerous to leave it, and twice because it was riddled with myomata.

The appendix was adherent to or incorporated with the pus sac in seven cases, and was removed.

The cæcum was once torn into where a pus collection was evidently just about to perforate. The opening was sutured with a continuous silk Lembert suture, in two rows, and no ill result followed. The parietal wound suppurated slightly once and extensively three times; in two of the latter there was a streptococcus infection.

In forty-four of the forty-five cases vaginal cœliotomy was done as a preliminary measure, either at the same sitting (thirty-three cases) or from six to fourteen days prior to the abdominal cœliotomy (six cases). In four cases only vaginal cœliotomy was done: once because it appeared to have effected a cure, both as regards symptoms and physical signs (puerperal case), and three times because the patients felt so well that they refused further operation, although at the time of their discharge from the hospital bi-manual examination disclosed the presence of the original pus foci. In my experience there is, sooner or later, a relapse in the vast majority of such cases.

On referring to my address as President of this Section at the Hobart Congress, nearly four years ago, it will be seen that the technique which I then advocated remains unaltered. Much has been written on the subject of "The treatment of pelvic suppuration"; there have been earnest advocates of the vaginal route and equally earnest advocates of the abdominal route, but I believe I have been the first to advocate and practise the systematic employment of both routes along certain well-defined lines. For the purposes of discussion I shall recapitulate these.

(1) In all cases in which it is evident from the symptoms and physical signs that pus exists in the pelvis, the treatment should be operative.

(2) When the patient is desperately ill, operation is done without a moment's delay.

(3) When it appears probable, after a careful examination and weighing of all the factors in the case, that the patient although seriously ill is likely to improve with rest and supporting measures, operation is postponed until the symptoms have subsided and the temperature become normal.

(4) The opening of the abdominal cavity is always preceded by curettage at the same sitting.

(5) The abdominal cavity is always opened first through the posterior vaginal fornix. If serious constitutional symptoms are present at the time of operation, and if pus can be evacuated from the pouch of Douglass, or from pus sacs within reach of the finger, and if bi-manual examination discloses no other collection which might be responsible for such constitutional symptoms, the pelvis is thoroughly cleansed, powdered with iodoform, and

the pus sacs lightly packed with gauze. In a week the patient's condition has usually so much improved as to allow of abdominal section in the middle line and the removal of the pus sacs.

(6) If, on the other hand, there are no constitutional symptoms, or these are but slight, or if again it be impossible to reach and evacuate the pus from the presence of which constitutional symptoms have arisen, then the final step of medium abdominal section is undertaken at the same sitting, and the pus sacs completely removed.

(7) If the uterus appears to be involved in the septic process it also is removed.

(8) The same rule applies to the appendix.

(9) Flushing is not practised unless there is general purulent peritonitis.

(10) The pelvis is carefully cleansed by gauze swabs, and lightly powdered with iodoform.

(11) Every case is drained by a *single* strip of gauze, 2 inches wide, earried from the pelvis into the vagina.

This technique is the outcome of a large experience and mueh anxious thought; it gives good results, and I have therefore thought myself warranted in again bringing it under your notice.

SOME POINTS IN THE CAUSATION OF PELVIC SUPPURATION.

By G. Rothwell Adam, M.D., C.M.

Mr. President—In bringing before this Seetion so important a subject as Pelvic Suppuration, I must crave premission at the outset to extend my title so as to include all forms of pelvic inflammation; for I need not remind an audience such as this that it depends on the dosage and virulenee of an infection, together with the resistanee of the patient, whether an inflammation will pass through all its phases to suppuration. Nevertheless, this trinity of problems is presented to each one of us when faee to face with a pelvic inflammation, and on a correct solution depends the forecast of the progress of the case. These remarks have been suggested, in some measure, by the careless use of the term "pyosalpinx," as signifying a pelvie suppuration, and not infrequently this loose diction is used to cover all forms of tubal inflammation. But, while it may be freely admitted that tubal inflammation is the most frequent cause of pelvic suppuration, there are other situations in whieh pus may be found in the pelvis.

Abscess of the broad ligament is not so uncommon; and, under certain conditions, to be discussed more fully hereafter, I believe purulent fluid will collect in the pelvie basin apart from a primary tubal origin. An unusual situation for pus to eolleet is in the uterine wall. Such an instance occurred in a lady under my eare, who, when pregnant, contracted gonorrhœa. The labor was normal, and her medical attendant stated that the puerperium presented nothing unusual. Within six or seven weeks after labor she began to eomplain of pelvic pain, and had a rise of temperature: the uterus was found retro-deviated, and fixed by an extensive exudate. Under rest and the usual treatment the acute symptoms subsided: the abdomen was opened, with the intention of removing the infected adnexa and restoring the uterus to its normal position. In taking a V-shaped piece out of one uterine eornu, in order to thoroughly remove the affected tube, an abscess cavity was opened, and about three drams of pus evacuated. This cavity did not eommunieate with the fallopian tube in any way, nor was there any eonnection with the

uterine cavity. The pus from the uterine abscess was sterile; at all events no cultivations could be obtained from it. An extended experience in operative work for purulent salpingitis has led me to note how frequently the isthmic portion of the tube is free from disease, while the ampulla may be distended with several drams of pus. This is more surprising when we consider that the infection in most cases, if not in all, has travelled by direct extension from the endometrium, through the tube, to its abdominal ostium. More than this, in many instances the remains of an infected endometritis still in existence can not infrequently be demonstrated; so that we have the two extremities of the tract bearing evidence of disease, while the middle portion, although necessarily traversed by the infection, is apparently free. It may be that the anatomical relation of the isthmus of the tube to the broad ligament, with its enormous lymphatic and vascular supply, is in a better position for recovery than either the uterus or the ampullary end of the tube. When these conditions exist, a further argument is advanced for the conservative treatment of suppurative salpingitis.

It is now universally recognised that the causes of pelvic suppuration fall under three heads, and the order in which they occur, as regards frequency, is as follows:—1. Gonorrhœa; 2. Sepsis (parturition and abortion); 3. Tuberculosis.

Since Noggerath years ago drew attention to the ravages wrought by the gonoccocus when spread to the endometrium, increasing experience has amply confirmed his views, so that it may be admitted that pelvic suppuration is chiefly due to this variety of infection. Nevertheless, a not inconsiderable number of cases owe their origin to septic infection, either after full-time parturition or after abortion. A much smaller proportion have tuberculosis as a cause, and, owing to peculiarities in pathological and clinical features. form a class apart from the two preceding groups. The object of this paper is to draw attention to the part played by sepsis in bringing about pelvic inflammation, which not infrequently proceeds to the stage of suppuration.

At the outset it is important to recognise that once the genital area is infected, by either the gonoccocus or septic organisms, the clinical phenomena are, to all intents and purposes, identical: that is to say, when confronted by a case of pelvic inflammation the only information that will enable us to differentiate between the two groups of causes will be found in the antecedent history. At first sight this might be thought to be easily obtainable, especially when the infection manifests itself shortly after parturition; but the case just recorded will serve to point out the difficulties in arriving at a correct judgment. How much greater, therefore, must the difficulties of a correct diagnosis be increased when the patient presents herself months, or it may be years, after a confinement?

Again, it is within the experience of most of us who deal with this class of case that pelvic suppuration by no means invariably, or even frequently, follows severe puerperal sepsis. In fact it has become well recognised that it is in those instances when the infection is of a mild degree that subsequent pelvic mischief is to be dreaded. I have seen a woman brought to death's door by puerperal sepsis, and yet within eighteen months delivered of a fine healthy child; and, what is more important for the purpose under discussion, convalesced with a normal puerperium.

In order to make my meaning more explicit it will perhaps be better to describe a typical case, as it presents itself to the examiner.

A married woman, the mother of several children, gives the history that all her labors have been normal, and that the convalescences, with the exception of the last (now some months ago), were also satisfactory. During the last puerperium, although not confined to bed for more than the usual time, she had for a few days some elevation of temperature, and offensive lochia.

She also says she has never been quite well or strong, as she puts it, since this last confinement. Latterly the pelvic pain, which hitherto only annoyed her at varying intervals, has become persistent. Menstruation, when it occurred (often during lactation), was profuse, unhealthy, and accompained by pain. Each recurring menstrual period is now marked by a steady decline in her general health. On pelvic examination, the indisputable signs of tubal inflammation can be discovered, but without any extensive exudate. Not infrequently the uterus will be retro-posed, and may deceive the unwary by its apparent mobility.

Thus far the case is common enough ; but a further stage, in not a few instances, is reached. Under favorable circumstances the mild, slowly developing infection will wear itself out, and the inflammatory process come to an end, leaving the tube occluded at the abdominal ostium, and perhaps adherent—or at all events fixed—in close proximity to intestine. Under such circumstances it is conceivable that it only requires something to lower the resistance of the tissues, *e.g.*, influenza, exhausting fevers, &c., in order that the colon bacillus may pass from the intestine along the line of adhesion and re-infect the damaged tube. That this does take place, when the appendix vermiformis becomes attached to the fallopian tube under such conditions, is almost unanimously admitted. Mr. Bland Sutton recently reported a case where the appendix vermiformis opened into an occluded fallopian tube, and discharged pus into the ampulla. The essential point, in the majority of instances, being the injury done to the tube by a prior infection from the uterus.

In looking over the histories of a number of chronic salpingitis cases, it was noticeable how many dated their ill-health to an antecedent abortion. Nor was this altogether confined to those patients who, either from lack of opportunity or careless ignorance, failed to obtain medical assistance ; and in not a few the information was vouchsafed that " the doctor ' curetted.' " In these it might be permissible to doubt if the cure was not at all events as bad as the disease, seeing that the tubes became affected all the same.

When it is recollected that the pathological events which accompany abortion—the hæmorrhage, tearing of fœtal envelopes, &c.—all invite sepsis, it is not to be wondered at that a mild infection ensues with its attendant sequelæ. And it is quite conceivable that a failure to maintain an aseptic technique in the treatment of an incomplete abortion will result in an infection of the upper generative tract, notwithstanding an apparently immediate good recovery. It may be asserted as a fairly general rule that the morbidity resulting from an abortion works its greatest mischief at a late date. Instances such as are here mentioned must be familiar to everyone in family practice who cares to follow up obstetric histories by a careful gynæcological inquiry. Of course it is not intended to imply that every abortion is necessarily followed by a salpingitis, but merely to point out how favorable the phenomena of abortion are to the inception of a mild sepsis. One might as well say that, in the pre-antiseptic midwifery days, every parturient was doomed to sepsis. Those of us who experienced those times will remember the frequent outbursts of " puerperal fever," and also its mortality.

While directing your attention to some of the less obvious causes of pelvic inflammation, it may be of some profit to see what lessons can be drawn from them.

To take the cases following on a mild sepsis occurring during the puerperium first. If it once be recognised that such serious lesions as salpingitis and, more or less, pelvic peritonitis do sometimes owe their origin to a mild degree of infection arising during the recovery from parturition, it is obvious that more importance will be attached to abnormalities of the puerperium, even though they be slight and temporary.

If practitioners would undertake to mould maternal opinion in the direction of submitting to systematic pelvic examination, instituted after the period of normal involution had passed, a step forward would be made in fighting so insidious a foe. Even if only those cases in which slight irregularities occurred during the puerperium were subsequently examined, much future trouble would be averted. A careful aseptic curettage, with the establishment of good drainage from the uterine cavity, at this stage would undoubtedly be efficacious in preventing many a subsequent dangerous and mutilating operation.

The second lesson, if short, is emphatic. It applies to the emptying of the uterus in abortion at the very earliest opportunity. If the principles which govern modern gynæcological practice are correct, no sound argument can be adduced in favor of waiting on nature to complete the abortion. No doubt nature is quite capable of so simple a matter as evacuating the uterus, but, as our knowledge of modern pathology tells us, she calls to her aid certain agents whose actions she cannot always control.

The third lesson to be drawn from the consideration of these few points is that an apparently quiescent salpingitis may become re-infected through adhesions to intestine. The woman with such a pelvic condition is never safe while she retains her damaged tubes.

Reflection on these cases of pelvic inflammation brings the conviction that many are in a sense preventable, and, moreover, that we possess two lines of defence. The first entails greater care during parturition of aseptic technique and closer observation of the puerperium, whereby the incidence of a mild sepsis may be prevented. The second line may be regarded as establishing the custom of *post* puerperal examination, especially where any irregularity in temperature, pulse rate, and condition of the lochia has been noted.

By these means I believe we shall help to bring gynæcology into line with other departments of medical practice, where the prevention of gross and other irreparable disease is the ideal aim.

Brilliant as the achievements of gynæcological surgery have been during the past twenty years in saving life and restoring the invalid to health, it must not be forgotten that these results are attained by methods crude in themselves and destructive of physiological function. That this is recognised in some measure by a number of pelvic surgeons the various plastic procedures on damaged tubes and re-sections of ovaries bear witness.

These so-called conservative operations must, at best, be classified as experimental; and we have to confess that at present we lack any systematised scheme calculated to restore the physiological integrity of these important organs.

Is not the time approaching when it is incumbent on us to seek out the prime causes of the gross lesion, and thereby prevent destruction of physiological function?

THE EVOLUTION OF THE ABDOMINAL OPERATION AND INCISION.

By J. Foreman, M.R.C.S., Eng., L.R.C.P., Sydney.

The evolution that has taken place in the abdominal operation since the first meeting of the Medical Congress at Adelaide, in 1886, has been so great that I thought the different stages of it might present some points of interest and mark a time from which to gauge further developments. The period, being almost twenty years, I may divide it into four, for purposes of convenience.

The first five years: Those who were then practising abdominal surgery will only too well remember the mortality was high, and patients who recovered had, as a rule, a desperate struggle—distended abdomen, temperature and pulse raised, and suppuration of the incision being too common in a large proportion of the cases.

Twenty years ago, during an operation the patient lay on her back, with all the drawbacks and inconvenience of that posture. Little attention was paid to the disinfection of the skin, which was merely washed with soap and water, and the incision was made in the linea alba. The ovarian pedicle was transfixed and tied with the stoutest silk, without any attempt being made to cover it with peritoneum. The pedicle, in cases of fibroid tumor, was still brought to the surface of the wound. The abdomen was closed by interrupted sutures of silkworm gut, leaving on the peritoneal surface a succession of spaces, which were a fertile source of adhesions. In the toilet of the peritoneum sea sponges were invariably used. The process of cleansing the sponges was crude and imperfect; but, fortunately for the patients, Keith's tubes were in general use.

I now come to the second quinquennium of the period under review. By this time our knowledge of the germ theory had become more complete and accurate, and consequently our means of combating the great evil of asepsis more thoroughly adapted towards that end. Sponges were more carefully cleansed, and, so far as it was possible, rendered aseptic; while large tampons, which could more readily be kept clean, were occasionally substituted for them. The raw surface of the pedicle was completely covered by sewing the peritoneum over it, but the method of transfixing the pedicle was still adhered to. The peritoneal cavity was more efficiently cleansed and more care was taken to arrest hæmorrhage, as it was now recognised that the presence of blood in the cavity of the abdomen offered the most fertile medium for the development of micro-organisms. In other respects the operation resembled that of the previous period; but, as a result of the changes I have mentioned, the consequent mortality was much less.

In the third quinquennium there was a marked improvement in everything connected with abdominal operations, for at this time asepsis became the watchword of our art, instead of the feebler antisepsis, the insufficiency of which was generally recognised. Now we found the real reason why cases were lost in the failure to attain complete asepsis, and the various discussions on this subject were of the highest value, as they led to constant attempts after perfection in our methods: a perfection which we have not yet attained, but which we must never cease to strive after. Moreover, the operation was greatly facilitated by the introduction of the Trendelenberg position, while the pedicle was secured by sutures instead of by transfixion and ligature. The abdomen was still more thoroughly cleansed and dried, and drainage was seldom used except occasionally P.V. In using the drain in this manner it is done by means of gauze through a rubber tube. Wick makes the best drain, then plain gauze: gauze, with any powder in it, can only interfere with the flow of fluid. I think cases have been lost through putting in gauze and leaving it alone for two or three days. When it gets saturated drainage ceases, unless there is a dry pad in contact with it; even then, when lymph forms round it, it simply acts like a cork backing up the fluid in the pelvis. My own practice is to ease the drain the next day, and only in exceptional cases does it remain in more than one to two days.

The incision was made through the belly of the rectus instead of through the linea alba, and the abdominal wall was secured in separate layers with catgut sutures. Suppuration sometimes occurred in the incision; but the mortality rate was still further reduced.

I now come to the main object of this paper, namely, to show our present position and analyse the reasons for our great progress; for we can fairly call it great when we are able to say that no surgeon in an extensive practice should have a mortality, allowing for peculiarities in patients, of more than 1 per cent. in clean cases. We must take clean cases only as a standard, for in other cases patients are so often poisoned and so near death that the question of operation becomes very doubtful. Some surgeons will operate so as to give patients a chance. Naturally many of them die; but some are saved, and, of course, cases of this kind cannot be brought under the general rule. Moreover, it is from clean cases that we learn whether there are any flaws in our mode of operating: that is to say, whether asepsis has been properly carried out, for it must never be forgotten that asepsis is by far the most important element of success in abdominal surgery.

The lessening in mortality is due to several causes, the first of which is greater strictness in aseptic methods and attention to minutiæ. Next to this I should place greater knowledge and skill in operating; then the Trendelenberg position; after that, the preparation of the patient; and, last, the closure of the abdominal wound and after care of the patient. We will consider these *seriatim.*

First, *Asepsis.*—Nowadays everyone understands the principles of asepsis, and so I shall say nothing about them; but I would like to point out that the translation of these principles into practice is not always a matter of course. You will see a man scrub his hands very thoroughly, using a nail brush with the greatest zeal, and straightway dry his hands with a towel without rinsing them either in clean water or under a tap. Manifestly in this process there is no security that the operator's hands have become germ-free. You will see another, after cleansing his hands *secundum artem* with soap and water, condy, and oxalic acid, quite unconsciously scratch his head, pull his moustache, or even put his hands in his pockets. Here, again, we must recognise a sad failure in translating aseptic principles into practice. The preparation of the skin is now very well carried out, but still is not as perfect as we hope to make it All persons present at the operation now wear white washing garments, which can be readily sterilised; and even the caps, which some have scoffed at, are valuable in their way, if only as indicating a striving after that perfection of method which it should be our constant endeavor to attain. The use of silk for ligatures has been abandoned, and in its place we use catgut, which can be rendered absolutely aseptic by one of several well-known methods. The chemical solutions employed to further the object of asepsis are very numerous, and each surgeon uses the one he prefers. Each has its particular value, and I need not dwell on them.

Second—The greater our practice in any art or occupation the greater will be the skill which we attain, and in the art of surgery that enhanced skill will be attended by a corresponding increased safety to the patient. Twenty years ago, even in the hands of the best operators, it was by no means uncommon to see the abdomen closed up without removing a growth on account of the supposed impossibility of completing the operation. Nowadays it may be said this never happens to a skilful operator. Formerly it was unavoidable, for the difficulties were immense and the knowledge but limited, and in this respect we can certainly claim great increase of skill in operating.

It would be difficult to over-estimate the advantages of the Trendelenberg position, which is now universally adopted. By it we are able to see the whole of the pelvic basin, and so to bring into close apposition the cut or torn margins of serous surfaces, and to remove blood clot, which, as I have already stated, is recognised as the most fertile source of septic infection;

while, from the greater facilities it affords the operator, it has materially shortened the duration of the operation. It must be admitted that it is not applicable in every case, for where the pelvis contains free fluid, whether serous, sanguineous, or purulent, it would be manifestly unwise to adopt a position in which the fluid might find its way into the upper part of the abdomen. In such cases as these the supine position is used, as formerly.

The principles which underlie the preparation of the patient are now fairly well understood. Rest in bed, thorough unloading of the bowels, light diet, free action of the skin, and thorough cleansing of the skin of the abdomen by some approved chemical solution, are some of the principal measures on which we depend.

I now proceed to consider the modern operation, but I must again insist upon the extreme importance of absolute asepsis and careful cleansing of the cavity, for it is chiefly to these two factors that we owe our freedom from those distressing cases of abdominal distension which formerly were so common. The evolution of the abdominal incision has been to me an object of great interest. Formerly, following the example of Spencer Wells, it was the universal custom to make the incision in the linea alba; and this, especially with the system of drainage which was then in force, was a frequent source of hernia. Nowadays we never think of cutting in the linea alba: we open the sheath of the rectus, turn aside the belly, and open the peritoneum beneath it. This is a great improvement on the method of some years ago, which consisted in cutting through the rectus muscle, for, as will be at once seen, by the present method we avoid that interference with the nervous and vascular supply of the muscle which must necessarily result from an incision through its belly. The advantage of the new incision, which came into use about three years ago, will be seen when we consider the method of closing the abdomen. The cut edges of the peritoneum are brought into careful apposition by a continuous suture with chromic gut, and the cut edges of the rectal aponeurosis are similarly joined with the same material. The skin incision is closed with the continuous suture of white gut. It will be seen at once that an incision so closed up leaves practically no risk of hernia, while the risk of adhesions is very much reduced.

The treatment of the pedicle has been greatly improved. Instead of transfixing and ligaturing it with silk it is now compressed with the angiotribe—an instrument which I now show you—with the result that the parts compressed are quite bloodless when it is removed. The margins of the serous membranes are left in accurate apposition, while the broad ligament remains free from any traction upon it. Through the compressed part a white catgut continuous suture is run so as effectually to prevent any risk of secondary hæmorrhage. I need hardly say that gentleness of manipulation is important, so as to avoid as much as possible the bruising of the abdominal organs. Gauze pads are used instead of sea sponges.

The After Treatment.—Since Lawson Tait taught us that the proper way to treat peritonitis is not to lock up the bowels, but to get them to act freely, our principal care in cases of abdominal operation has been to ensure a brisk action of the bowels within, at most, twenty-four hours. If the bowels do not act the day following the operation care is taken to bring about this result. This may be said to be the main point in after treatment in abdominal cases, and the system of free purging has saved many lives and greatly increased the patient's comfort. A little consideration will show that, for the comfort of the patient, it is very important that the bandaging of the abdomen should not be too tight. Even in perfect health a very tight bandage round the abdomen is extremely uncomfortable, and for a woman who has

just undergone a grave operation tight compression of the abdomen is very painful, and tends to restrict the action of the bowels. We must allow the patient to rest in the position most comfortable to her.

The record of abdominal surgery during the last twenty years has been one of almost uninterrupted improvement and success; but there is one point, I mean the prevention of adhesions, in respect to which our results are by no means so satisfactory as we could wish them to be. From time to time cases present themselves in which the presence of adhesions goes a long way to neutralise the success of a well thought-out and skilfully performed operation, and it is very distressing after a patient has been relieved of a dreadful disease to find her life made a burden by apparently unimportant adhesions. To prevent their occurrence is one of the principal desiderata of our art, and a conscientious surgeon must keep it constantly before him as an aim of the highest importance. Personally, I have tried aristol in large and small quantities without the success I expected, for on two occasions I have had to re-open the abdomen for adhesions and found the worst ones about the site of the powder. I have also left the abdominal cavity full of saline fluid, but, like many others, cannot trust much to it. At present I am trying the method of washing the pelvis with a sea sponge and saline, just as we wash the abdominal wound, and it is surprising what a different appearance the serous surface presents after it is done. It is too early yet to say if I shall be content with this, but in my opinion the problem to be worked out is this prevention of adhesions, and the one who is fortunate enough to give a fairly safe method will merit the gratitude not only of the patient but of the surgeon also.

CHORION EPITHELIOMA.

By F. C. Batchelor, M.D., M.R.C.S., Eng., Dunedin.

It is not my intention to read a paper dealing with the subject of Chorion Epithelioma: it has been so ably and fully discussed in recent literature that it would be absurd for me to attempt to generalise from one special case. So far as I know, very few cases have been recognised in Australia, and the one I propose to report to-day is the first that has been published from New Zealand.

M.C., aged 45, married twenty-three years; six children, the last six years ago. Two miscarriages, one twelve years ago at the third month, the last six months ago, about six weeks pregnant.

Admitted into the Dunedin Hospital on December 30th, 1904.

Patient weak and anæmic, has lost much flesh, complains of loss of appetite and thirst, with feverish attacks, stomach irritable, and occasional bouts of vomiting; some cough, with occasional slight hæmoptysis; almost constant metrorrhagia.

History.—Dates her illness from miscarriage six months ago; has never been well since; was attended by her doctor, who removed a mass from the womb, under an anæsthetic, and considered her six weeks pregnant. There was much hæmorrhage at the time.

Following the abortion had almost constant slight bloody discharge. At the end of a month a flooding came on; she was curetted, and took medicine; the flooding abated, but a blood-stained discharge continued from the vagina. She continued to get weaker, lost her appetite, complained of headache, dyspnœa, and general malaise, and occasionally spat up small quantities

of dark blood; she continued under medical treatment, but did not improve; there was persistent menorrhagia, with occasional floodings. A few days before admission to the hospital there was a heavy hæmorrhage. Pulse 104, temperature 103°.8, with remissions, a rigor preceding the rise in temperature.

On examination, at the lower part of the vagina two irregular excavating ulcerations were noticed, one on the lower lateral wall, and one on the anterior wall, just above the meatus; the ulcerations were about half an inch in length and a quarter of an inch broad; they extended through the mucous membrane, the margins were dark-bluish in color, somewhat resembling a tubercular ulcer. On the posterior vaginal wall, lying just within the caruncle, was a small dark-blue sub-mucous tumor, about the size of a bean; the cervix was thickened and irregular, blood and fœtid discharge was exuding from the os, the uterus was retro-flexed; when drawn forward it was found to be enlarged, soft, and flabby, with a nodular thickening about the right cornu posteriorly.

The nodule from the vaginal wall was excised; it was entirely beneath the mucous membrane, and enucleated readily. It looked like a blood clot; there was no active bleeding from the capsule; it was set aside for microscopic examination. The uterus was dilated. On curetting soft friable tissue came away in large quantities, accompanied by severe hæmorrhage; it was evident that it would be dangerous to continue curetting, as the uterine wall would have been readily perforated. The uterine cavity was freely douched with hot formalin solution, and plugged with iodoform gauze; further measures to be determined by examination of the vaginal growth. Chorion epithelioma was suspected.

The tumor from the vagina was found to be composed chiefly of blood-clot, with decidual-like groups of cells dispersed freely through its substance. [Section shown.]

Diagnosis.—Chorion epithelioma plus sepsis. Vaginal hysterectomy decided on.

January 7th, 1905.—Uterine cavity washed out and filled with iodoform gauze, external os closed by strong silk sutures, and uterus, ovaries, and tubes removed by the vagina. Patient was in an extremely bad condition, but stood the operation well, and for some days there was a decided improvement. On the 13th a rise of temperature occurred, with a return of septic symptoms.

On February 8th a large abscess under the deep fascia of the right side was opened, and a quantity of fœtid pus evacuated. A portion of the sac wall was excised for microscopic examination and pronounced to be granulation tissue. The septic symptoms, however, still persisted; for a day or two she would keep comparatively well, then a rigor and rise of temperature would occur.

About the middle of March, for some ten days, she improved remarkably; so much so that she was able to be removed to the sun-room for some hours daily, and had determined to return home.

On the 26th of March, however, the rigors and rise of temperature recurred, and from thence she gradually went to the bad.

A fortnight before her death it was noticed that there was paresis of the left arm and leg; this gradually deepened, and for some days before her death she was hemiplegic and comatose. She died on April 18th.

The two ulcerations in the vaginal wall healed spontaneously, and the incision for removal of vaginal growth closed by primary union; the abscess in the right thigh had also healed before her death.

No thorough *post mortem* could be obtained; but the abdomen was opened and examined before removal of the body by the friends. The pelvic

floor had united perfectly, and there was no growth in the pelvis. During her illness it had been noted that the liver was enlarged and tender ; a portion was torn away ; it had a nutmeg appearance, and, in the portion removed, two small abscesses, containing about half a drachm of thick pus, were found.

There were also a number of small, stellate, fibrous-like patches in the liver substance removed. Examined microscopically these show broken-down liver cells and fibrous tissue, which contain small deeply-stained brown cells, with marked nuclei. [Section shown.]

The spleen was soft and diffluent, and there was a small abscess in the cortical portion of the left kidney.

The uterus [specimen shown] has a large fungating mass sprouting into the cavity and excavating deeply into the muscular wall of the uterus on the right posterior wall, near the fundus ; at a portion just below the level of the right tube there was little but the peritoneal coat of the uterus left ; bluish masses of the growth could be seen extending into the uterine wall, off-shoots of the main growth. Microscopically the characteristic cubical and synthythial cells are seen invading the muscular walls of the uterus.

I was unable to obtain clear data from the gentleman who attended her as to the character of the miscarriage. He was, however, impressed by the fact that the uterus was far too much enlarged for the assigned date of pregnancy, and he describes the cavity of the uterus as being filled with quantities of blood-clot and pieces of after-birth.

One point of interest in this case seems to be the apparent latency of this growth : two secondary nodules in the vagina broke down and healed spontaneously ; the third, removed by me, was evidently undergoing a similar process. The absence of bleeding from the cavity, and the microscopic appearance of the growth, showed it to be undergoing necrotic changes.

In the absence of an efficient *post mortem* (which was refused) it is impossible to say whether the hæmorrhage from the lungs and the cerebral symptoms were the result of a secondary growth or a septic fœcus ; but, from the marked septic symptoms and such *post-mortem* findings as we were able to obtain, I am inclined to attribute her symptoms to sepsis.

After the removal of the uterus it would have been quite impossible to have diagnosed her condition from a severe and prolonged form of septic infection. One might, *prima facie*, expect this form of growth to assume varying degrees of malignancy, and it is possible that toxins resulting from the septic infection may have neutralised or arrested the growth of the secondary tumors, much in the same way as Coley's fluid sometimes influences a sarcomatous growth.

In consequence of recent theories as to the frequency of an excess of lutean tissue being found in the ovaries in cases of chorion epithelioma, I prepared a large number of sections from both ovaries. To one who does not claim any expert histological knowledge on this subject it seems an extremely difficult matter to decide what constitutes an excess of lutean tissue ; and, furthermore, when we are taught that this lutean tissue is undergoing a process whereby the connective tissues proper of the ovary are re-formed, it is evident that there must be stages in this process where it is impossible to distinguish the one from the other. I should be glad to have the opinion of any who have studied this matter on the specimens which I shall show. Of one point, however, I am convinced : it is this—that lutean tissue exists in all ovaries in very considerable quantities during the active periods of sexual life, and in specimens that I shall show, where the ovaries have been removed in cases of early ectopic gestation, the lutean tissue is very greatly in excess of anything that is met with in this special case which I have described.

PUERPERAL SEPSIS.

By H. Osburn Cowan, M.B., C.M., Glas., of Kew, Victoria.

I have chosen the title, Puerperal Sepsis, in preference to the older term, Puerperal Fever, as removing my subject at once out of the realm of epidemicity and placing it in that of dirt. It thus loses much of its mystery, but at the same time, as I shall hope to show, much of its menace.

CAUSATION.

Puerperal sepsis, like sepsis elsewhere, is due to breach of surface infection by pathogenic organisms. The organisms are various, comprising streptococci, staphylococci, the bacillus coli communis, and others; the sites and circumstances of their entrance vary, and so we find much variety in the clinical expression of microbial invasion of the parturient canal, either during labor or during the puerperium.

Under normal circumstances, the uterine interior in pregnancy and labor is sterile, and even under abnormal circumstances of dirt and disease, the experience of lying-in charities shows that the danger of puerperal infection can be reduced to the vanishing point. For instance, in the out-door practice of the British Lying-in Hospital, for the years 1880-1902, the proportion of deaths from septic causes was only one in 10,000 deliveries. As this covers the cases due to auto-infection, as well as those due to infection from without—and that, too, in the class of patient in which the former might be regarded as most likely to occur—it is evident that, as a cause of puerperal septic disease, auto-genesis may be regarded as a negligible quantity.

The most recent work on the bacteriology of the genital canal confirms this statement. Whitridge Williams says—"Bacteria, likely to be the cause of puerperal infection, are not to be found, except rarely, in the upper part of the vagina"; and Foullerton and Bonney, who have given the fullest consideration to this aspect of the question, say—"The puerperal fever which is likely to follow auto-infection will usually be of a comparatively mild type." Puerperal sepsis, therefore, for all practical purposes, must be regarded as not only due to infection, but to infection conveyed from without. It may be carried from case to case; but of hardly less importance is the fact that pathogenic organisms, which are found in myriads at and around the vaginal orifice, may be carried in on the examining finger, instrument, or douche nozzle. Nature has, in various ways, safeguarded the process of parturition against the unassisted entrance of infective organisms. She had, however, laid her plans long before the introduction of the methods of meddlesome midwifery, and no provision has been made against the actual introduction of infection from without, or of dealing with it when it has occurred. Given, indeed, the presence of pathogenic organisms in the upper reaches of the parturient canal, and no culture medium can compare with the interior of the puerperal uterus. Once the invading army has reached the uterus, it finds supplies and lines of communication ready at hand. The possibilities of extension and absorption are so vast that anything approaching to certainty in dealing with an established infection is manifestly out of the question. On every count we are compelled to the conclusion that the best endeavors of the obstetrician must be concentrated upon the prevention of infection in the first instance. Here prevention is not only better than cure, but prevention is possible and cure is dangerously problematical.

GENERAL CONSIDERATIONS AS TO PREVENTION.

It has been said that the medical man begins his struggle with infection on laying hold of his obstetric bag. But he must begin it long before this, recognising that successful warfare depends hardly less upon preparation than upon performance. Apart altogether from the added horror of puerperal infection, pregnancy and parturition in themselves involve no small risk and strain. Even under modern conditions there still remains a small proportion of women to whom pregnancy brings improved nutrition and general health; but for the large majority it involves more or less impairment of function—digestive, hæmapoietic, nervous, and mechanical. There results loss of individual resisting power and of tissue vitality. Deficient innervation, for example, and inefficient muscular contractility increase the liability to post-partum hæmorrhage, and the resulting diminution of systemic resistance to microbial invasion and the greatly enhanced activity of the absorbents seriously increase the liability to generalised infection. And over and above its special significance, albuminuria, with the cellular degradation to which it gives rise, has an important bearing in this direction. In short, from the date of his engagement to attend a confinement, everything that concerns the functional and organic health of his patient must concern the practitioner who desires to conduct her with the minimum of risk through her coming trial.

THE OBSTETRIC BAG.

It has always seemed to me that quite an unnecessary amount of abusive criticism has been directed against the obstetric bag. Various measures having been suggested for its reformation, and in vain, its abolition is now demanded. The so-called aseptic detachable lining is a delusion, and may be in a very real sense a snare. As it is impossible to keep such a lining aseptic, it cannot be looked upon as a defence, and after two or three boilings it becomes utterly lost to all sense of size and proportion. The *bag* should certainly be clean; but it is the *man* who must be aseptic. The principle which so much insistence upon the character of the bag is apt to make us lose sight of is that the only safety in obstetric work lies in personal and instrumental sterilisation for each individual case. The obstetric bag should be kept strictly for primary obstetric work, and it should be much larger than that in common use, in order to carry in washable coverings an apron and the necessary instruments and appliances.

PREPARATION OF PATIENT AND PHYSICIAN.

After reaching the sick room, and noting the general condition of his patient, the medical attendant's first duty is the careful sterilisation of his hands. I need not go into details as to this, but need only say that it should be as conscientious and thorough as for any surgical undertaking. If, owing to previous septic contacts, he is in doubt of its completeness, sterilised rubber gloves should be worn. Its use is so desirable, on the grounds of decency and defence, that no careful medical man, who looks at the question from the modern point of view, would nowadays proceed to the conduct of labor without the protection of a sterilised apron. The nurse will, in the meantime, have been engaged in the careful preparation of the skin at and around the vaginal orifice, and the combined abdominal and vaginal examination of the uterus may now be undertaken.

EXAMINATION OF PATIENT.

No part of a paper which I read at Hobart was so adversely criticised as that in which I urged the necessity for retaining the vaginal method of examination. The position which I then took up I still maintain. In a

large proportion of multiparous cases it may be entirely dispensed with, and in primiparous cases where the descent of the head is already well advanced. But if we keep before us as our ideal not to make more than one vaginal examination, and not to make that if we can avoid it, we can so safeguard the procedure as not only to increase but actually to diminish the risk to mother and child. Much as we may learn from abdominal palpation as to the position of the child, and even as to the progress of labor, there remain points upon which vaginal examination only can inform us, such as the condition of the cervix, the descent of the anterior lip, which not infrequently prolongs labor and renders it more painful, and the exact bearing of coccygeal abnormalities. For the early recognition, too, of prolapse of the cord, upon which the life of the child may turn, we are entirely dependent on vaginal examination.

I am interested to note that, in speaking of abdominal palpation in a recent article, Berry Hart says—" This method is not so suitable for private practice, but there the reduction of internal examination to a minimum, with the use of gloves, should give results as good, and probably *more* sure."

Needless to say, the casual " trying of the pains " by the nurse must be absolutely prohibited.

Vaginal examination must only be performed after thorough preparation of the physician's hands, and the careful separation of the labia minora, in order that the examining finger may not carry infective organisms from the external genitalia to the upper part of the vagina.

ASSISTED DELIVERY.

We have, so far, dealt only with the preliminaries to the conduct of a case of normal labor. The importance of their consideration lies in the fact that, in such a case, they represent the only risk of infection up to the end of the second stage. The same principles will guide us when we have to assist delivery. In any attempt to deal fully with the thesis of puerperal sepsis, something more than a passing reference to the use of the forceps is necessary. They may increase the risk of infection by the direct introduction of organisms, the production of trauma, and the consequent opening of fresh channels for infection, and the diminution of tissue resistance. The nicest discrimination is necessary in their use, so that it be not too frequent, too early, nor yet too long deferred. With a view to the safety of both maternal and fœtal structures, they must be accurately applied, and used to so assist the expulsive pains in their incidence and in their direction as to obtain the maximum of result with the minimum of force and of injury. It is my own practice to remove the blades when the head is well down upon the perinæum, with a view to minimising the risk of laceration.

Important as trauma is from the point of view of mortality, it is even more so from the point of view of morbidity. Broadly, I think it may be said that the morbidity of the puerperium is more largely determined by the traumatism produced during the second stage of labor, and its mortality by the complications of the third stage.

THE THIRD STAGE.

The management of the third stage of labor is the key to the control of post-partum hæmorrhage, which has been already referred to as seriously enhancing the risk of septic infection ; but it is in retained secundines that the greatest danger of infection lies.

Nature is deliberate with design in the separation of the placenta, and so far as my experience goes, seldom makes an attempt at actual expulsion within half an hour. It is not until after this period has elapsed that there

is any justification in ordinary cases for hastening the delivery of the placenta. Cases, however, occur from time to time in which, owing to over-fatigue of the uterine muscle, free hæmorrhage is observable while the placenta is still *in utero.* When confronted with such inertiá, great judgment is required to determine whether the danger of delay is greater than that of delivery. It is sometimes impossible to maintain uterine contraction sufficient to control bleeding, and I have not hesitated in such cases to empty the uterus after fifteen or twenty minutes, and give ergotinin and strychnine hypodermically and the liq. ergot. by the mouth, the latter in the hope of continuing the more immediate contraction induced by the former.

Both placenta and membranes should be carefully examined as a matter of routine, with a view to ascertaining, not only whether they are complete, but also whether separation has occurred in the proper plane. If not satisfied as to the integrity of the secundines, my own practice is as follows :—

1. If the placenta is complete, and only a small portion of the membranes is wanting, to leave it.

2. If a considerable strip is wanting, to go as far as the cervix in search of it. If that fails to discover it, again to leave it, and to instruct the nurse to watch carefully for its presentation.

3. If a portion of placenta is wanting, to pass my hand into the uterus. The necessity for this, however, very rarely occurs. It is much easier to remove manually the whole than a small portion of the placenta, and the accoucheur should not persist in attempts to forcibly express a placenta which is presumably adherent in whole or in part. I find, however, that greater patience in the third stage has markedly reduced the number of cases in which either procedure has been required.

Nothing gives one such a sense of security at the termination of labor as the integrity of the placenta and membranes.

THE REPAIR OF LACERATIONS, ETC.

I need not dwell upon the folly of pursuing the policy of the open door with regard to lacerations of the perinæum. If extensive, they should be repaired in the dorsal position, and deep and accurate apposition obtained. I am at a loss to explain nature's apparent indifference to the integrity of the perinæum, even in normal cases, seeing that it constitutes such an important safeguard against puerperal infection.

If for any reason there has been recourse to forceps extraction before the full dilation of the cervix, it is advisable to explore the upper reaches of the vagina and the cervix, as rupture into the posterior fornix is known to occur in instrumental delivery which has left the perinæum intact. In normal cases, however, and even in the large majority of difficult cases, *post-partum* interference is to be strongly deprecated. For this reason routine *post-partum* douching has been abandoned. In my opinion, vaginal douching *post partum* is worse than useless, excepting as a preliminary to intra-uterine irrigation, which will only be called for when there has been manual interference with the uterus itself. For such procedure only a fountain douche should be used, because of the one-way flow, the domestic Higginson, with its two-way suction, being, to my mind, a most dangerous appliance.

CLINICAL CONSIDERATIONS.

We have dealt, so far, only with preventive measures. If, however, infection should occur, how are we to recognise it, and how shall we treat it?

It is not necessary to dwell at any length on the clinical picture of puerperal sepsis. It is ever present to the mind of the obstetrician, and is as imperative as ever was the writing upon the wall. It should determine and

control every detail of the conduct even of a normal confinement. No man who has struggled with it for, perhaps, week after week, with despair for his "familiar" night and day, can deem any precaution too exacting which promises immunity from its recurrence.

I wish it were possible for me to define wherein lies the difference between the rise of temperature on the third to the fifth day of the puerperium, which ushers in puerperal septicæmia, and that which, though suggesting ugly possibilities, clears up entirely within twenty-four hours. When it is ushered in by a distinct rigor and a rise to 104° or 105°, the differentiation is not difficult; but it is not always so accompanied, nor does it always rise so high. The febrile reaction after septic infection is often insidious, and may suggest nothing more serious than mammary or intestinal disturbance. There are cases, too, in which a free *post-partum* hæmorrhage or some individual peculiarity robs the pulse rate of much of its significance. In my own experience an acceleration of the pulse, disproportionate to the rise of temperature—as, for instance, a pulse of 100° with a temperature of 99°—has frequently been traced to mammary disturbance, and has cleared up on the satisfactory establishment of lactation. On the other hand, nothing has been so effective in eliminating intestinal disturbance as a cause of high temperature in the puerperium as a dose of calomel. I have known the temperature fall after purgation from 103°.5 to 99°, with the clearing up of accompanying symptoms. If, however, after the elimination of such disturbing factors, the febrile temperature is maintained, and especially if it show a definite evening rise, the presence of infection may be inferred.

It has been usual to distinguish at this point between sapræmia, or septic intoxication, and septicæmia, or septic infection. It is now generally admitted that this is a distinction of degree and of duration only. The attack of pathogenic organisms upon dead tissue producing toxæmia may, at any moment, develop into a general invasion, producing septicæmia. It is more correct, therefore, and it is certainly safer clinically, to regard the so-called sapræmia as a stage on the road towards a general septic infection, and to treat the case from the outset as having all the inherent possibilities of the latter.

Fœtor of the lochia is not regarded as evidence of puerperal sepsis. In some of the most virulent cases of streptococcic infection it may be entirely absent. It is evidence of decomposition of dead tissue, but tells us nothing as to systemic invasion; in fact, given other evidence of general infection, I should regard marked fœtor of the lochia as a favorable symptom. It suggests local, rather than general, bacterial activity, and it promises a satisfactory response to energetic local treatment. Where it is possible, a bacteriological examination should be made of the uterine contents, and, if performed early, this may give invaluable indications for treatment. In the later stages of puerperal sepsis mixed infections so confuse the issue that the indications for treatment are much less definite.

TREATMENT.

Given the presence of infective organisms in the parturient canal during the puerperium, and everything favors their multiplication and distribution. This is no time for hiding one's head in the shifting sands of conjecture. Above all other explanations, let us banish the ever-handy influenza from our consideration of the earlier, and typhoid from that of the later, stages of puerperal sepsis. Every hour is of importance. Acute immediate general infection, presumably streptococcal, is very rare, and the development of puerperal sepsis may frequently be inferred before a general microbial distribution has occurred. Such inference should spur us to immediate action. If we have

been quite sure of the integrity of the membranes and placenta, it will be sufficient, in the first instance, to douche the vagina, and, subsequently, the interior of the uterus, and to deal with such breaches of surface as our examination reveals. This will remove the uterine *debris*, destroy organisms which have not penetrated the decidua, and, according to Pryor, if performed within twelve hours of the onset of the first evidences of infection, will effect a cure. If it does not, and especially if there has been any doubt about the placenta, my own practice has been to explore the interior of the uterus under an anæsthetic, and to remove adherent tissue and *debris* with a blunt flushing curette; thereafter to swab out the uterine interior with an antiseptic, strongly germicidal, but not escharotic; to irrigate with a normal saline or very dilute antiseptic solution; and then pack the cavity with 10 per cent. iodoform gauze. In the limited field of private practice I have not found it necessary to employ the more radical procedure of the *cul-de-sac* operation so strongly recommended by Pryor.

Such is my attitude towards the much-debated question of primary curettage in cases of puerperal sepsis. Is has given me most satisfactory results, and, carried out carefully, with a rigid regard for asepsis, has proved itself a sound and safe procedure. In cases of direct virulent streptococcic infection, in which there is no question of retained secundines, it is not indicated; but there are cases the true nature of which can only be determined by bacteriological investigation, and in which, more than in any others, good results may be expected from anti-streptococcic serum.

BACTERIOLOGY AND SERUM-THERAPY.

Splendid work is being done with a view to determining the precise bacteriology of puerperal sepsis. It has already established the preponderating importance of the streptococci in the causation of severe puerperal infections. Much, however, still remains to be done, for, as Dr. Foullerton points out, the group probably includes several species, differing in pathogenic action and in the character of their respective toxins, and requiring for their appropriate treatment different specific sera. Until these have been distinguished, the use of such a polyvalent serum as that offered by Burroughs, Wellcome, & Co., represents, perhaps, the best we can do under the circumstances. But just as the bacteriological examination of the lochia must be early to be unequivocal, so the serum-therapy of streptococcic infection must be early to be effectual. There is reason to think that the serum is rather germicidal than antitoxic in its action, and, if so, it is unreasonable to expect much result when the organisms have invaded a large cavity such as that of the peritoneum.

THE EXAMINATION OF THE BLOOD

has, so far, not given us any further indications for the treatment of puerperal septic conditions. It is hardly necessary to say that the demonstration of streptococci in the blood stream indicates definitely the presence of a streptococcic septicæmia; but the leucocyte count has not led to findings of definite diagnostic or therapeutic value. The normal variations during pregnancy and the puerperium are so great that the determination of morbid variation is well-nigh impossible. Besides which, it is not the mere presence of pus which determines leucocytosis, but the passage of bacterial toxins into the circulation; and this passage ceases to take place when free drainage has been provided: consequently, it is likely to be more marked in an acute localised mammary inflammation than in a generalised puerperal infection.

For the same reason, too, when the pus in a pyosalpinx has been definitely walled off, the leucocyte count does not indicate its presence.

To the clinical obstetrician the importance of the demonstration of the part played by streptococci in virulent puerperal septic conditions is as follows :—

(1) They constitute the most clearly and definitely preventable of all puerperal infections, being always introduced from without.

(2) Their presence may be definitely recognised at an early stage in the process.

(3) There is reason to believe that, at an early stage, they are most amenable to treatment by an appropriate serum.

The principle to bear in mind in dealing with all puerperal septic conditions is, be previous if you can, and if you cannot be previous, be early.

PREVENTABILITY.

I do not propose to go into the statistical aspect of this question, which has been so often dealt with before. The experience of the in-door and out-door departments of the great lying-in charities demonstrates the preventability of puerperal sepsis, and it remains to bring the statistics of private practice into line with these results, for up to the present they have shown no improvement. In the tables of the Registrar-General's returns there is no cause of death so open to attack as that which is covered by the term puerperal septic diseases. Much is said from time to time of the possibility of eliminating tuberculosis as a cause of mortality within three or four generations; but here is a cause which, by combined effort, could be removed within one generation. Much blame is laid at the door of the untrained midwife, and this difficulty is being largely done away with by the Midwives Act of 1903. Some such Act should be placed upon the statute books of all civilised communities. In the meantime, let each medical man in his own sphere demonstrate the fact that puerperal sepsis is preventable by trained intelligence, and an enlightened public opinion will see to it that such training is made available for those who cannot afford to pay for themselves.

If the infirm, the insane, and even the inebriate are made a charge upon the State, surely the mothers, upon whom its very continuance depends, should be not less the object of its care.

From a racial point of view, surgery must often regard its triumphs with doubtful satisfaction; but the healthy mother means so much to the future of the child, and the healthy child so much to the future of the race, that the safeguarding of parturition admits of no such mental reservation.

THE TOXÆMIA OF PREGNANCY.

By F. C. Batchelor, M.D., M.R.C.S., Eng., Dunedin.

My reasons for bringing this subject under your notice are these :— In the first place, the condition to which it is my intention to refer is of great practical import, and is at the present time attracting considerable attention from those interested in our special branch.

So far very little is definitely known on the subject; what we do know, however, promises to considerably modify our views and methods of treat-

ment of what I take to be a very common complication of pregnancy. Further advance in our knowledge can be best attained by drawing the attention of the profession generally to the condition and to the trend of modern thought concerning it. Further, I am hopeful that a discussion will ensue that may be helpful in eliciting the experiences of others, who must have frequently had to deal with similar conditions.

For clinical purposes the toxæmia of pregnancy may be best ranged under two main types :—

1st. The toxæmia accompanied by that appalling complication, eclampsia. Most authorities seem now to regard this condition as the result of toxic material circulating in the blood : that the condition of the kidneys, often in the past regarded as the primary source of the trouble, was now relegated to secondary importance, and considered due to the irritation, congestion, or inflammation of these organs in consequence of the toxins in their blood supply. The nature of the toxic matter and its source was not clearly understood, the weight of evidence tending to show that it was a form of auto-intoxication resulting from imperfectly oxidised proteid material, which, under normal circumstances, is excreted by the kidneys in the form of urea ; but, as the result of failure of oxygenation, poisonous materials or by-products of urea were produced which were highly poisonous, and that the organ chiefly responsible for this failure was the liver. With this form of toxæmia it is not my intention to deal. Many of you residing in the large centres must meet a greater number of cases than fall to the lot of one who lives in a comparatively small town, where such cases are, fortunately, not of frequent occurrence.

The second type of toxæmia to which this paper specially refers is unaccompanied, as a rule, by convulsive seizures, and is but little recognised by the profession generally. The severe cases are not of great frequency ; yet, from an experience of over thirty years' professional work, I am confident they are by no means rare, nor to be viewed as medical curiosities. The symptoms may be marked and virulent, and may lead to a fatal issue, and are specially liable to be overlooked or confused with other conditions ; nor is this at all surprising when we find the current works on midwifery, almost without exception, ignore the subject, or at most devote a few short paragraphs to the topic. When myself brought face to face with the disease I have altogether failed to find anywhere anything approaching a clear and definite description of it in any of the text-books. Further, although, as I have already stated, the severe cases of this type of toxæmia may not be very common, milder forms, it seems to me, are extremely frequent ; many of the minor disturbances, so common in early pregnancy as to be almost looked upon as physiological, are often probably a result of this toxic state. Fortunately, in a large proportion of instances they pass off spontaneously or under treatment, but they are capable of more efficient and satisfactory management if the factor on which they depend is recognised.

Finally, although some of you may have heard or read little of this disease, I am confident during the next few years you will find it taking a prominent place in obstetric literature ; once its significance is recognised, as is now being done, from its importance it must excite deep interest in all who undertake midwifery work, and is of great moment to the general practitioner under whose charge these cases invariably come.

The readiest method of bringing this subject before you will be to read you clinical reports of three cases which have come under my own observation during the past few months.

Case I. was seen by me in consultation with Dr. Gabites, of Timaru; he has kindly supplied me with a full report, from which the following *precis* was prepared :—

M.C., aged 21, married seven months, had always been healthy, strong, and fairly stout; no previous illness, except heat-exhaustion in Durban nine months before present illness; no history of malaria. Fell pregnant about five months before commencement of the present illness; early pregnancy, uneventful till the second month, when she complained of nervousness; got well under treatment.

August 3rd, 1904.—Went out in the snow in thin shoes; on returning home felt chilly and faint, and had bad pain in the stomach. There was a slight red discharge. Spent a restless night on account of the pain in the stomach.

Dr. Gabites first saw the patient on the morning of August 4th. Temperature, 102; pulse, 100; no cough or coryza; complained of pain in the back and over abdomen. Treated for influenza: gradually improved.

August 7th.—Felt quite well and took her food well; no feverishness and no pain or tenderness anywhere; ravenous, ate heavy meal. At 11 p.m. the same day Dr. Gabites was called urgently, and found the patient evidently suffering from intense pain; pain, referred to the right hypochondriac and the right lumbar regions, did not appear to shoot in any particular direction; there was extreme tenderness on pressure over these regions. Temperature, normal; pulse, 116.

August 8th.—The pain returned, but was bearable; pain referred to the right iliac region and down the right side. Morphia gave relief.

August 9th.—Pain and tenderness less; passed urine frequently, urine clear and high-colored; acid, specific gravity (1015), mucous deposit, no albumen. Temperature, 100; pulse, 90.

August 10th.—No pain. Temperature, 99; pulse, 85; seemed to be improving.

August 12th.—Had a rigor at 2 p.m. Temperature at 5 p.m., 101; pulse, 102. Flatulence troublesome; complained of nausea, no inclination for food. Patient's condition evidently worse. No pain, but extreme restlessness; felt as if her heart would stop.

August 13th.—Consultation with Dr. Drew. Symptoms of pain attributed to pressure of the pregnant uterus; restlessness, feverishness, coated tongue—probably due to a relapse of influenza. Treated for influenza.

August 15th.—Nausea more troublesome during the past two days. Vomited bile-stained fluid this morning; anorexia, faint icteroid tinging of sclerotic. Some return of tenderness and pain in right hypochondriac region. Patient in a highly nervous state; complains of pains first in one part of abdomen, then in another; tongue thickly coated, bowels constipated. Temperature, 101·2; pulse, 110.

August 22nd.—Trained nurse put in charge; from hence temperature is recorded on chart daily. Patient still restless, emaciation marked, complains greatly of thirst, vomiting has been a prominent symptom last three days. Nutrient enemata every six hours and bowel irrigated with warm water every twenty-four hours, half-pint of saline injected per rectum twice daily to relieve thirst.

August 26th.—Patient's condition remains unsatisfactory.

August 30th.—Consultation with Drs. Drew, Hogg, and Unwin; no definite conclusion arrived at. General opinion seemed to be in favor of

pyelitis, due to pressure on ureter. Temperature, 103.3; pulse, 120; slight rigor for a few seconds. As the patient's condition was becoming critical further advice was decided on.

August 31st, 5 p.m.—I first saw the case in consultation with Drs. Gabites and Drew. The patient looked extremely ill and emaciated, being markedly changed from the plump healthy young woman she appeared in a photograph taken shortly before her illness. Her facial aspect was pinched and anxious; there was a darkish flush over both malar regions, and a slight icteroid tinge of the skin and sclerotic. Her tongue was coated and red, her pulse quick and weak, running up to 120 at the slightest excitement or movement; she complained of indefinite abdominal pains, chiefly located about the right lumbar region; there was general hyperesthesia of the skin. An exhaustive examination failed to localise any distinct lesion; the uterus was enlarged to its normal size for the period of pregnancy, the fœtal heart and fœtal movements were to be detected, the liver dulness in the right mammary line was less than normal: it was noted, too, that there was entire absence of any enlargement of the thyroid. A blood-count showed 9,000 white cells per cmm. The symptoms seemed to me to point to some form of toxæmia; tuberculosis or enteric were possible conditions, and the diagnosis lay between these and some form of auto-intoxication of pregnancy. On the whole I inclined to the latter view. If tuberculosis had been going on for some weeks we should have expected some evidence by physical examination; there was none anywhere, and the family history was good. Any doubt as to the question of typhoid was subsequently excluded by an examination of the blood, which showed negative results tested by Vidal's re-action.

The examination of the urine supplied the strongest argument in favor of an auto-intoxication; the urine had a peculiar brownish-green color, was clouded when passed, was small in quantity, of low specific gravity (1010), with a marked decrease in urea; there was a slight trace of albumen, some leucocytes and small epithelium from the upper urinary tract, and a few granular casts. It gave a strong acetone re-action with the perchloride of iron. It seemed to me that if the condition was due to typhoid, even allowing that the patient was practically starving from an inability to retain food, yet, with the rapid tissue waste going on plus the excretion for a five months' fœtus, there should have been a much greater excretion of urea.

It was decided that, in addition to saline injections by the bowel, the patient should be moderately purged, fed by the mouth on small quantities of peptonised gruel only, and, with the object of increasing the evidently deficient metabolism, 5 grains of thyroid extract should be given thrice daily.

For the following four days there was a decided improvement. The urine was carefully collected and measured daily; the quantity was found to average 30 ounces daily, an increase in the amount passed prior to the administration of the thyroid. The specific gravity ranged from 1109 to 1013. The total average daily excretion of urea from September 1st to September 7th was 144 grains per diem—needless to remark, a very marked diminution from the normal excretion of a woman in the sixth month of pregnancy. During this period the appearance of the urine was much as at the first examination—cloudy, greenish-brown, considerable uratic deposit after standing, slight occasional tinge of albumen, bile acids in small quantities, with strong acetone reaction to the perchloride of iron. No efficient examination was made for leucin or tyrosin, but ordinary evaporation tests failed to show their presence.

September 5th.—Dr. Gabites reported :—"Patient improved until to-day ; was able to take small quantities of peptonised gruel. For three days temperature mostly sub-normal, and pulse rarely up to 100. Very restless all night, slept for a few minutes at a time ; thirst troublesome, gruel discontinued on account of urgent vomiting, delirious at times, patient weaker."

September 10th.—Dr. Gabites wrote me patient had had some weak turns, the pulse was irregular and weak, necessitating the injection of strychnine, and, in consequence, it was decided to discontinue the thyroid extract.

September 11th.—After consultation by letter and telegram in consequence of unfavorable report, and in view of the persistent diminution of the quantity of urea—less than 150 grains per diem—we advised induction of labor without further delay.

September 12th.—I again saw the case, in consultation with Drs. Gabites and Drew ; her general condition was worse, emaciation more marked, nausea and vomiting persistent, constant thirst and distress with feeling of extreme exhaustion. Temperature rises daily from 101 to 103 about 4 p.m. ; occasional rigors and very marked nervous irritability and restlessness : the nurse states it is impossible to please or manage her. Induction of labor decided on ; a bougie passed about 10 p.m.

September 13th.—It was decided that it would be unwise to employ slower methods of emptying the uterus, as the delay would probably have been fatal. At 10 a.m. Dr. Gabites administered ethyl-chloride, followed by ether. Patient was moved to operating table and labor induced ; the bougie had effected a moderate amount of dilatation of the cervix ; os about the size of a half-crown : digital dilatation failing to efficiently dilate for purposes of delivery, Bozzi's dilater was used, and delivery completed in about two hours. The placenta and membranes were perfectly normal. The child died during delivery.

Dr. Drew, during the latter stage of the operation, administered two pints of saline direct into the veins of the elbow. The patient stood these measures well, and was moved back to bed in fair condition ; two hours later, however, she was suddenly seized with pre-cordial distress, intense dyspnœa and restlessness. The pulse at the wrist becoming imperceptible, and the beat of the heart fluttering and irregular, we feared she was about to die from pulmonary embolism. After free administration of strychnine, however, the condition gradually passed off, and towards evening there was a decided improvement.

September 14th.—I saw the patient with Dr. Gabites in the morning. Nurse reported she had passed a better night than she had for some weeks past ; the nurse was specially struck with the marked decrease in the nervous irritability : she was now quite manageable, whereas previously she had been a perfectly impossible patient.

September 18th.—Dr. Gabites reports patient has been making steady progress ; vomited three times after operation, and complained of severe pain across upper portion of abdomen on two occasions ; sleeping well, no restlessness, pulse and temperature normal.

From this date the patient gradually improved, and in about a month had fairly recovered.

Specimens of urine were sent to me, from time to time, subsequently to her delivery ; there was a gradual improvement in quantity and quality. The return to normal was, however, gradual, it being some three weeks before it could be considered healthy.

CASE II.—The second case, to which time will only permit a very brief reference, was seen by me in consultation with Dr. Marshall Macdonald, who has kindly supplied me with data prior to my attendance.

B.F., aged 18, married eight weeks. First consulted Dr. Macdonald on February 8th, 1905; was then five months pregnant; had been attacked in the night with severe abdominal pains; there was also some vomiting and diarrhœa. Uterine contractions could be felt, and were accompanied by pain; os undilated. For ten days following she gradually got worse, temperature reaching 101, or higher, daily; vomiting more or less persistent; pulse ranging from 115 to 120. Urine showed a trace of albumen, was scanty, and of low specific gravity. Patient complained of constant thirst and epigastric discomfort.

February 18th.—The case was first seen by me in consultation with Dr. Macdonald. Patient's general condition was very low; pulse between 130 and 140; temperature, 102.5; tongue, brown and dry. Careful examination failed to elicit evidence of any local lesion; patient was suffering from painful uterine contractions, persistent vomiting, and great thirst; she was much emaciated. It was deemed advisable to remove her to the hospital to secure more favorable surroundings and efficient nursing.

February 18th to 23rd.—Patient was treated by mild laxatives, half a drachm of bromide with saline by the bowel thrice daily, small quantities of peptonised gruel only being administered by the mouth; a few doses of virbirnum with liq. opi. sed. soon subdued the painful uterine contractions.

Beyond the relief from pain, there was little general improvement. Very little food could be retained by the stomach; she slept badly, complained constantly of intense thirst and epigastric discomfort, was highly nervous and irritable, and suffered more or less from constant retching and vomiting, the latter being occasionally bilious in type. The patient was still wasting There was a slight icteroid tint of the skin and sclerotics, a marked decrease in liver dullness; the thyroid gland was not enlarged.

The urine was carefully measured and examined daily: during this period it averaged about 36 ounces daily. It was clouded on passing, was a peculiar brownish-green tinge, which formed a heavy deposit after standing; it was faintly acid reaction; specific gravity ranged from 1008 to 1011; there was generally a trace only of albumen; microscopically, a few round cells, small epithelium from the upper urinary tract, and occasional waxy casts. There was considerable deposit of urates, and crystals of uric acid. Bile pigments and bile acid were detected in small quantities.

No efficient examination for leucin or tyrosin was made: there was always a strong acetone reaction with the perchloride of iron; the urea was markedly diminished, averaging 148 grains per diem.

No local lesion whatever could be found to account for her condition, and we concluded the case to be one of auto-intoxication.

February 24th.—Five grains of thyroid extract were administered thrice daily.

February 25th.—The temperature this afternoon rose to 103.5, her pulse rate being 126; there was a marked increase in the quantity of urine, 52 ounces being passed, of low specific gravity (1009); urea, 220 grains.

February 26th.—She felt better; urine to-day measured 50 ounces. From hence to March 6th she seemed better, nausea and vomiting almost ceased, and the urine averaged from 40 to 50 ounces per diem; it still

retained the characteristics as when first examined, specific gravity being persistently low and the quantity of urea much less than normal, averaging 140 grains.

On March 6th she said she was quite well, and, despite all warnings, insisted upon leaving the hospital.

March 11th.—Re-admitted. Temperature, 102.4; pulse, 124; condition much as when she was first admitted; urine scanty; low specific gravity (1009).

March 12th.—Her condition was evidently becoming critical, and it was decided that further temporising measures were inadvisable.

March 13th.—Chloroform administered by Vernon Harcourt's inhaler; less than 2 drachms used. Cervix dilated by Hægar's, and a medium-sized Barnes' bag inserted. Small doses of chloral given through the day.

March 14th.—No result from Barnes' bag; chloroform again administered. Digital examination failed, in consequence of the extreme rigidity of the cervix; Bozzi's method soon proved efficient, dilatation being effected without much trouble, and delivery completed.

Nothing abnormal with child or placenta. Child died shortly after birth, sixth month. Towards the termination of the operation two pints of saline were injected into the vein at the elbow.

The patient's subsequent progress was satisfactory. There was a marked increase in the quantity of urine immediately following delivery, but for some days the specific gravity remained low and the urine clouded; it was about a fortnight before it became normal.

Case III. was of a somewhat different type; admitted into the hospital February 13th, 1905.

M.S., aged 28, pregnant three months; had been troubled with retching and vomiting for the past ten weeks. Examination failed to disclose anything abnormal; uterus enlarged to about the usual size for the period of pregnancy; no cervical catarrh or erosion. Urine scanty, but the exact quantity difficult to estimate in consequence of the relaxed state of the bowels from medicine.

Urinary examination showed brownish thick cloud, containing epithelium, urate, and a few round cells; no albumen; specific gravity, 1020. The nurse is sure the total quantity is less than one pint daily: urea estimated at 180 per diem.

The gravity of this ease was not at first sufficiently realised. I was inclined to view it as one of the severe types of reflex vomiting of early pregnancy, which, as a rule, rapidly improve in hospital with rest, purgation, and careful dieting.

February 15th.—Still vomiting; chlorotone, 5 grains every three to six hours, as required.

February 17th.—Vomiting less, but patient heavy and drowsy; looks worse. Pulse, weak, 120. Chlorotone stopped; bismuth and hydrocyanie acid by the mouth, and saline injections by the bowel, each containing, in addition to the saline, ½ drachm bromide of soda.

February 20th.—Less vomiting. and now able to retain small quantities of peptonised gruel.

February 24th.—Not much trouble with sickness; can take but little nourishment, feels sick, occasionally brings up a little bilious fluid. She is not improving. Pulse averaging between 120 and 130. The urine is per-

sistently scanty—less than 18 ounces per diem—dark and cloudy; slight trace of albumen; strong acetone reaction with the perchloride of iron; urea, 120 grains.

February 28th.—For the last two or three days the patient has been losing ground. She is not vomiting, but seems excessively exhausted; pulse persistently above 120; she is dull and drowsy. Although the patient has lost much flesh she is not excessively emaciated; her temperature for to-day, for the first time, was raised—100.6. There is a marked ictoroid tinge of the skin, the liver dullness is decreased; the urine is further reduced in quantity, only 10 ounces yesterday and 16 to-day; shows bile acid and pigment in small quantities; total urea in twenty-four hours less than 100 grains.

Consultation with Staff.—In view of the fact that so many cases of vomiting of early pregnancy are relieved by local treatment to the cervix, it was thought desirable to employ local measures before resorting to emptying the uterus. The cervical canal was therefore dilated with ivory glove stretchers, as recommended by Edgar, and carbolic acid applied to the whole cervical canal; this was easily done, without rupturing the membranes. Chloroform was administered by Vernon Harcourt's inhaler, which answered excellently; less than 2 drachms being used.

When the patient was returned to bed she was apparently in very fair condition, and I did not expect she would be much upset by what had been done. The following day, on the usual morning visit, I was shocked to find that the patient had become comatose and died about 11 p.m. the previous evening.

No intra-venous injection had been administered in this case, as her condition at the time I left her did not seem to call for its use. *Post mortem* was refused.

It must, I think, be pretty evident to you that in cases I. and II. we had to deal with very similar conditions. The most prominent objective symptoms were the nausea and vomiting, but in reality they were minor incidents; the rapid wasting, the intense nervous irritability, the constant thirst, and excessive lassitude and debility were, in my opinion, factors of greater importance than the vomiting itself.

In all three cases on some days the vomiting ceased, or caused but little inconvenience; but it was noticed that the cessation of the vomiting was accompanied by no alleviation of the general symptoms. The temperature chart in cases I. and II. is remarkable, and I wish to draw special attention to it, for, although reports of cases of toxæmia are by no means uncommon, nowhere in medical literature have I been able to lay my hands upon any record of temperature. In view of the theory of the causation of this condition, to which it is my intention to refer later, some such elevation in temperature might, I think, be anticipated.

The marked deficiency in the daily excretion of urea is, to my mind, the cardinal diagnostic point in all these cases, and upon which more reliance can be placed than upon any other individual symptom. It might be suggested that the deficiency in the urea resulted from the starvation alone; but when we remember that previous to their illness both these patients were healthy, well-nourished young women, that during their illness they wasted rapidly, had a more or less persistently high temperature, and, further, had to excrete material for a five-months' living fœtus, one is forced to conclude that the consumption of their own tissues, apart from a certain amount of nourishment that had been retained, should, under normal conditions, have produced a much larger excretion.

Case III., although of a different type, seems to me clearly toxic. The operation of dilatation of the cervix was simple and easy, taking only a few minutes, and should not have caused any serious disturbance. Although the girl was seriously ill, her condition did not seem immediately alarming; after her return to the ward there was no evidence of shock, she quickly recovered from the anæsthetic, and for some hours seemed fairly well; later, towards evening, she became drowsy, and finally comatose, and died. This seems more in keeping with death from toxæmia than from simple exhaustion. It was most unfortunate that no communication was sent me as to her condition; had an intra-venous saline injection been administered I think her life might have been saved.

As remarked earlier in this paper, this condition of toxæmia has received scant attention in our midwifery textbooks, and, apart from the eclampsic condition, is almost altogether ignored. Cases, however, may be found scattered through medical literature; and if you look up Frerich's treatise on "Diseases of the Liver," published in 1858, you will find described under a series of cases of acute yellow atrophy of the liver in the pregnant woman, under observation case No. 14, a report of a very similar condition to my first and second cases. His *precis* reads, "Repeated attacks of lumbago in the seventh month of pregnancy, gastric catarrh, icterus, delirium, convulsions, death under symptoms of blood-poisoning." In the more detailed account of this case, which follows, the convulsions were apparently of a very trivial character, and only occurred towards the termination of the case, when, he remarks, "convulsive movements occurred in the muscles of the face neck, and arms."

In the American Textbook of Obstetrics I have found the only useful account that, so far, I have come across in any obstetric work where this condition is considered apart from eclampsia. Under the heading of "The Toxæmia of Pregnancy" the writer suggests that toxic material which may jeopardise the life of mother and child may result from the deficiency in metabolic processes. The toxins he considers alkaloid in nature, and the symptoms they produce upon the pregnant patient are especially addressed to the nervous system. He quotes Van Santvoord, who, from clinical observation, ascribed toxæmia during pregnancy very largely to deficient action of the liver, by which an insufficient formation of urea causes the patient to retain in her blood toxic material. Further, in diagnosing the toxæmia of pregnancy he points out two clinical signs of special value: first in importance, the amount and character of the urine, the diminution in the output of urea being of great significance; second, the condition of the nervous system; the presence of pain, headache, thirst, lassitude; disturbance of vision, of hearing, of taste; sleeplessness or lethargy, irritability or apathy, melancholia, nausea and vomiting, are all symptoms to be noted.

More recent work by Nicholson and Eden has also drawn the attention of the profession in Britain to this subject, but the most valuable information that I have so far come across is a communication to the "American Journal of Obstetrics," in the February number of this year, by Dr. Ewing, Professor of Pathology, Cornell University, Medical College, New York, and to this I shall specially refer as epitomising recent views and being specially valuable from its wealth of pathological data.

Dr. Ewing records the results of thirteen years' study of this subject in the Sloan Maternity Hospital, under the heading of "The Pathological Anatomy and Pathogenesis of the Toxæmia of Pregnancy." From my reading of this article it seems to me that, clinically, his cases can be ranged under

two main types :—First, the toxæmia accompanied by eclampsia : second, the toxæmia where convulsions do not occur, but where symptoms may assume varying grades of severity ; one extreme being represented by what we look upon as the physiological vomiting of early pregnancy, the other by the disease known as acute yellow atrophy of the liver. Ewing holds that the focal lesion in both these conditions is the same organ, namely, the liver, and in a large number of *post mortems* in fatal cases of eclampsia he found, without exception in every case, marked pathological changes were discoverable in the liver cells, and consequent loss of function. It is not, however, with this class that I want to-day to deal.

The Class II., or toxæmia apart from eclampsia, has been usually described under the term of "pernicious vomiting of pregnancy" in its less virulent type, "acute yellow atrophy of the liver" in the more fulminating variety. Ewing reports autopsies on three cases selected from a number clinically diagnosed as vomiting of pregnancy, and shows that this disease, when fatal, may be associated with (1) acute yellow atrophy of the liver ; (2) with the necrotic process in the liver, which is not reduced in size ; (3) with less marked degenerative change in the liver which may be overlooked or ignored, but by microscopic examination reveals diffused granular and fatty changes : foci of intense degeneration with disorganisation of the liver cells ; foci of partial necrosis, in which the cells showed watery contents and pyknotic nuclei, and areas where the cells are distended with bile pigments, which indicated extensive auto-lysis and profound disturbance of the function of the liver—lesions, moreover, very similar to those to be found in eclampsia.

After dealing at length with the pathological changes in the liver, to which I do not propose further reference, he proceeds to deal with the pathogenesis of the toxæmia of pregnancy :—

"The exact nature of the disturbance of nitrogenous metabolism, which is responsible for the clinical manifestations of the toxæmia of pregnancy, is a failure of oxidising capacity on the part of the liver ; for this reason the proteid derivatives, which are normally combined by the liver into urea, are no longer combined, but circulate free in the blood in some poisonous form, and are, to some extent, excreted by the kidneys. The complex nature of the sources of the poisons renders less obscure the fact that the clinical manifestations of the toxæmia of pregnancy vary from mild vomiting to acute yellow atrophy.

"The relation of the mild to the severe cases of vomiting of pregnancy also required very careful consideration. No one doubts that mild and fatal cases of eclampsia are identical in nature, but there persists a definite impression in some quarters that acute yellow atrophy never occurs in a mild form, but is always fatal, while it is still generally taught that the mild vomiting of pregnancy is physiological and the severe vomiting is an exaggerated form of the other—but without definite pathological basis.

"Impressed by the supposed rarity and unfavorable prognosis of acute yellow atrophy, clinicians seldom attempt the diagnosis of this disease ; hence only the fatal cases are commonly recognised.

"Recent study of the toxæmia of pregnancy, and many other clinical conditions, furnishes abundant evidence that the morbid process in acute yellow atrophy is of very frequent occurrence, and is often followed by recovery.

"Vomiting is seldom the only symptom present in early cases of vomiting of pregnancy ; observation usually shows also striking mental symptoms—

headache, hysterical tendencies, pruritis, constipation, lassitude, &c., all of which doubtless result from the mild auto-intoxication which is the cause of the vomiting.

"The urinary changes in the toxæmia of pregnancy are of great importance for the diagnosis and prognosis, as well as for their bearing on the essential nature of the disease. These changes indicate chiefly deficient oxidation of proteid derivatives instead of urea, uric acid, ammonia, leucin, and tyrosin, and other unoxidised proteid radicals appear in the urine. Albumen casts are sometimes present, but may be absent in dangerous or even fatal cases.

"The present view of the toxæmia of pregnancy classes the disease as a functional disturbance of the liver, usually, but not necessarily, attended by severe anatomical lesions in this organ; and, secondarily, with functional disturbance and anatomical lesions of the kidney and other organs. The ground for regarding the disease as primarily in the liver is the fact that the synthesis of urea is exclusively a function of the liver."

There are endless theories as to the factors underlying this failure of metabolism in the liver; probably a combination of causes are at work :—

(1) The increased demands of the growing fœtus.

(2) Nervous disturbance. In two of the cases reported the patient had suffered great mental distress from the fact that pregnancy had occurred some time previous to marriage.

(3) Constipation is excessively common in early pregnancy, mechanical factors, &c. Some think in consequence of this loaded state of the bowel intestinal toxins are formed, which, when absorbed, may be a cause of the liver failure.

(4) Then again it has been noted in the fifth month of pregnancy there is physiologically a marked increase in the thyroid gland, with increased functional activity; when this fails to occur the normal metabolic processes may be interfered with.

In cases I. and II. it was specially noted that the thyroid gland could not be felt.

(5) Another recent theory of great interest is: It has been found there was in normal pregnancy synthycial elements, or the proliferating epithelium of the chorionic villi find their way into the systemic vein, and have been deported to the liver. Whether this is a physiological function which, in some instances, may exceed its normal limits, opens up a question of great interest at present but little understood.

One other theory I may mention, which cases I. and II. at any rate strongly negative, is that the toxæmic condition may depend on a diseased condition of the maternal uterine membranes. In both my cases the placenta, membranes, and fœtus were perfectly normal.

As Ewing shrewdly remarks, "It is not necessary to explain the ultimate origin of the toxæmia before recognising its existence and practical importance." One conclusion Ewing draws from the pathological study of his cases is the absence of any necessarily fatal character in the disease :—"We are not dealing with an uncontrollable bacterial infection, nor with a hopeless anatomical lesion, but with a disturbance of function which only secondarily leads to organic lesions. If the poison can be eliminated, or its further production prevented, there is nothing in the majority of the lesions which is incompatible with life, and there is demonstrative proof that extensive lesions of the liver of this type are sometimes followed by spontaneous recovery."

I fear it may seem that I have taken a great deal of time in describing these cases; but, personally, being so impressed with the importance of the condition and with the fact that it is so largely overlooked, and the symptoms are so often highly misleading, I feel justified in dealing with it at some length.

Do not for a moment think it is my intention to infer that every case of vomiting in pregnancy is the result of a dangerous toxæmia; clinical experience must have taught most of us how frequently this condition is relieved by comparatively simple measures: as, for example, unloading distended bowels, careful regulation of diet, quiet and rest; or, maybe, replacement of a displaced pregnant uterus. What, however, I do strenuously contend is this, that when all such sources of extraneous irritation have been removed, and when the vomiting and nausea persist, especially if accompanied by a train of severe nervous disturbances, a careful examination of the urine is demanded with special reference to the daily output of urea. If such examination serves to confirm the suspicions of toxæmia, the less the treatment is directed to the arrest of the vomiting the better for the patient; the sickness may be but an attempt to eliminate the poison. The bowels must be freely opened and kept open, any food by the mouth must be of the most digestible character and in very small quantities. Saline injections by the rectum, if absorbed, are of great value, by diluting and washing the blood and relieving the troublesome thirst. Where there is marked nervous irritability half-drachm doses of bromide of soda may be given two or three times daily with the saline by the bowel. If the sleep is broken and disturbed, half a drachm of chloral by the bowel at bedtime is the best remedy I know, and the least injurious.

The thyroid extract is well worth a trial, and is useful by increasing metabolism and by its diuretic action on the kidneys.

When a reasonable trial of these measures fails, and when the maternal system seems unable to accommodate itself to the demands of reproductive life, the only resource left is to empty the uterus; nor should this be delayed till the patient is *in extremis*, but, with that valuable aid to our resources—namely, intra-venous saline injections—I do not anticipate that fatalities attending the operation of emptying the uterus will be so frequent in the future as they have been in the past.

Further, it seems to me it is no longer justifiable to make a return of death as a result of the pernicious vomiting of pregnancy unless the liver has been subjected to thorough macroscopic and microscopic examination; and, if the alterations to which I have just referred are found, the death certificate would more correctly record the fatality as due to the toxæmia of pregnancy.

One further point before concluding, that is, to refer to the temperature chart in cases I. and II. Nowhere have I been able to find any data dealing with this point. In both cases I. and II. careful and repeated examinations failed to find any concurrent condition to account for the elevation in the temperature; but viewing these cases in the light of Ewing's article, and looking upon them, as I certainly am inclined to do, as a type of disease closely resembling acute yellow atrophy of the liver, might we not expect this condition to be attended by a rise of temperature? In many of the cases reported of acute yellow atrophy of the liver considerable elevation of temperature has been noticed in their early and late stages, and in a less virulent form there is no reason why a similar rise should not occur.

In cases I. and II. the symptoms in many respects resemble acute yellow atrophy of the liver, and its existence was seriously discussed at our first consultation; the decrease in liver dullness, nervous irritability, the marked lassitude and debility, the slight ictoroid tint, and the character of the urine were all highly suggestive of the disease.

ACUTE DILATATION OF THE STOMACH FOLLOWING ABDOMINAL SECTION.

By J. A. G. Hamilton, M.B., &c.

Acute dilatation of the stomach is a mysterious, and most fatal, condition. There is very little literature on the subject. None of the leading authors on gynæcology mention it as a possible complication in abdominal surgery. Campbell Thomson, of the Middlesex Hospital, has collected forty-four cases. No less than twelve were associated with surgical operations; only four, however, were associated with abdominal section. I have been doing abdominal work for the last twenty-five years, and have only quite lately been able to diagnose two cases of this condition; but probably I, in common with others, have overlooked many cases in the past. Patients, with constant vomiting more or less immediately following an abdominal section, which were put down as due to peritonitis, were in my opinion due, in many cases at any rate, to acute dilatation of the stomach. One case in particular I can recollect which followed an operation for pus tubes. I was at a loss to account for the symptoms, which somewhat differed from those usually seen in peritonitis; but, with increased experience, I have little doubt this was a case of acute dilatation of stomach. Pepper and Stangel suggest that the immediate cause is spasm of the pylorus; but it seems more probable that the symptoms are dependent on the absorption of toxins from the cavity of the stomach. Campbell Thomson, in an article in the *Lancet*, 1902, inclines to the belief that a primary paralysis of the organ must be regarded as the underlying cause in the great majority of cases, and not, as has been suggested, a constriction caused by tension on the inferior mesenteric artery under certain circumstances. In most cases the first part of the duodenum shares in the dilatation, the distension ceasing abruptly with collapsed bowel further on. Thomson relates a case in which he had made a necropsy. The acute dilatation of stomach was accompanied by distension of 8 feet of small intestine, and says in this case the superior mesenteric artery could not be considered as the constructing force, nor were there any other signs of constriction present. We have all met with cases of paralytic distension of the intestines which occurs during cases of acute peritonitis; in these cases we often find, during life or after death, the stomach has participated in the paralytic condition.

This is important, when we remember that peritonitis is clinically the condition which most resembles acute dilatation of the stomach, and for which many primary cases of the latter have been mistaken. As in every case where acute dilatation of the stomach appears after an abdominal operation, we have to decide whether the dilatation is a primary symptom or only the predominating feature of a general peritonitis. The symptoms are usually sudden in onset. A patient may seem to be doing well for some days or weeks after operation, when she complains of a fullness and discomfort in

her abdomen; this is quickly followed by the vomiting of huge quantities of greenish or brownish slimy fluid, with a sour odour; at the same time the pulse rate rapidly rises, the temperature falls or remains normal; the urine becomes scanty and loaded with urates; the bowels may act normally, or there may be diarrhœa, and the patient becomes progressively weaker, and may die in a short time. Physical examination shows distension of the abdomen, and on palpation a sense of fluctuation and a succussion splash may often be obtained, both of which are valuable aids to diagnosis when they are present. A peculiar fact about this condition is that the thinning of the walls of the stomach is not commensurate with the enormous dilatation. One would expect to find the walls of the stomach very thin, as in acute dilatation of a previously healthy bladder; but such is not the case, the walls appear to retain their normal thickness. If an attempt were made to blow out a stomach to the size it would certainly rupture at its lower curvature. From this it would seem as if the toxins have a softening effect on the stomach which permits the enormous stretching. As regards treatment, as soon as the condition is suspected lavage of the stomach should be started. This little operation is not a pleasant one, and usually patients strongly object to it; but, in acute dilatation of the stomach, they not only do not object to the passage of a tube but they usually beg for it, as it always relieves the constant vomiting—for a time at any rate; and in milder cases one or two washings out may cure the patient. All nutriment should, as far as possible, be given by the rectum; the thirst which is sometimes so distressing is best relieved by saline rectal injections and sips of hot water. Strychnine should be given hypodermically. I have tried salycilate of eserine, but without any good effect; if necessary, normal saline may be given intravenously. Drugs are generally useless, as there is no reasonable prospect of their being absorbed. If this milder treatment does not give relief, then some surgical treatment is called for. Cases in which the stomach has been opened, drained, and closed again have not done well. Robson and Moynihan, in their book on "Diseases of the Stomach," suggest doing a gastro-enterostomy, but it has struck me a jejunostomy would be more to the point: a gastro-enterostomy will only pass on the toxins, which nature is trying to eliminate by the mouth. If the patient cannot bear the stomach tube, which in my experience they generally bear well, then a very minute opening might be made in the stomach after Witzell's method—that is, an inch incision made in the stomach with a plication round a small tube, through which the stomach can be washed out, and, *a fortiori*, a similar opening made in the small intestine, as high up as possible, and the patient fed through this opening in the bowel, and then allowing the stomach a chance of eliminating the poisonous toxins. As these patients are generally in a bad condition, I think the operation would be better done under local anæsthesia in preference to a general one. There are no recorded cases, as far as I know, where gastro-enterostomy has been put to the test; but a friend of mine assures me he tried it in a non-post-operation case, which was diagnosed as "spasmus pylori." On opening the abdomen the stomach was found filling the abdomen like an immense ovarian cyst, with distension of the duodenum. A gastro-enterostomy was done, but the patient died. Although I have only seen two cases of acute dilatation of the stomach in my own practice, two other cases have come under my notice—one in an old man who had an operation for a double hernia; with frequent lavage of the stomach he made a good recovery. The other case occurred in a man who had an acute gonorrhœa, with pus in his epidydimis. The abdomen was opened, and a stomach reaching down to pubes found; it was opened and drained, but the patient died.

The following are the notes of my two cases :—CASE I.—The history of this case is interesting. E. W., *æt.* 22 years, admitted to Adelaide Hospital, May 5th, 1905. Menses, which have been regular, ceased in February. On April 20th she jumped off a washstand : after that a blood-stained discharge came on which has lasted ever since. Complains of sharp pains in lower part of the abdomen, which come on frequently. P. 126, small ; temp 99.8. On examination, cervix softened ; body of uterus enlarged slightly, in good position ; sound passed 3¾ inches ; no discoloration of vagina ; some milky fluid in breasts ; no areola ; nothing to be felt in fornices ; both lateral fornices very tender. May 7th, uterus curetted. Some thickened endometrium removed. I thought it might possibly be a case of ruptured tubal pregnancy, and told my house surgeon to let me know at once if any sudden change occurred.

May 19th.—Got up for a short time to-day ; feels well. No discharge. Pulse and temp. normal. On the evening of the same day she had a sudden rise of temp., 101, pulse 136. Feels as if something had given way. My house surgeon telephoned me her symptoms at 8 p.m., and I thought my original suspicion of ruptured tubal pregnancy was confirmed. I went down to the hospital at once and opened her abdomen. No blood escaped when the peritoneum was opened. The right tube was found thickened and inflamed and œdematous, but contained no pus. The left tube was in the same condition, but from the ostium abdominalis, which was patent, a few drops of pus escaped. This explained her condition. There had evidently been a sudden leakage of pus from the left tube. Both tubes and ovaries were removed, and abdomen washed out with large quantities of normal saline solutions.

May 20th.—Feels comfortable. P., 106 ; temp., 99. Flatus troublesome. Ordered eserine salycilate, one-forthieth gr. t.d.s.

May 21st.—P. 110 ; temp., 98.6. Slight distension. To have calomel, one gr. every hour, for five doses. In the evening bowels were well moved.

May 22nd.—Feels better. P., 120 ; temp., normal. Bowels moved. Abdomen a little distended. Eserine, one-fortieth gr. every six hours. Vomited greenish fluid. Ordered strychnine, one-thirtieth gr., hypodermically, every four hours.

May 22nd.—Been vomiting large quantities of greenish fluid all night. Temp., normal ; p., 120 ; only passed 8 ounces of urine during last twenty-four hours. Abdominal wound looking well. Some horsehair sutures removed. Stomach washed out night and morning with two quarts of saline solution : a large quantity of greenish fluid returned. She expressed herself as much relieved after each lavage. On palpation, stomach distended down below umbilicus. Succussion splash felt. She went on pretty much the same until 10 a.m. on May 26th, when she suddenly collapsed : pulse imperceptible, temp. sub-normal ; abdomen more distended. At 3.30 on same afternoon abdomen was re-opened. A dilated stomach was found reaching down to pubes, like an immense ovarian cyst. Small intestine collapsed. An opening was made in stomach, and several pints of dark-green sour-smelling fluid evacuated. Opening in stomach closed in two layers ; abdomen closed. Two pints of intravenous saline solution given ; but she never rallied after operation.

Post-mortem.—Stomach found extending down to umbilicus. Walls not much thinned out. The dilatation extended throughout whole course of duodenum, and ceased abruptly at duodeno-jejunal flexure, below which the intestines were collapsed. There was no constriction at duodeno-jejunal flexure.

CASE II.—Miss B., aged 42, August 8th, 1905. Supra-vaginal hysterectomy, for myoma of uterus. The operation was a difficult one, as she had a marked anterior curvature of spine, with a contracted right hip, which lay partly across the abdomen and very much hampered the operation. She made an excellent recovery. Pulse ranging from 90 to 100, temp. not over 100. She did well until August 28th, twenty days after operation, when she complained of pain in epigastrium, and vomited 6 ounces of dark-brown fluid: vomited continuously all that night; temp., 98.4; pulse, 126. Urine scanty, in fact almost suppressed—only passed 5 ounces last twenty-four hours. Rapid dilatation ensued, and the stomach not only formed a large swelling, filling up the superior abdominal region, but also extended well below the umbilicus towards the pubes. Stomach washed out with saline solution, three quarts; a quantity of brownish fluid came away. Ordered strychnine, one-thirtieth gr. four-hourly, rectal feeding; nothing by mouth but sips of hot water, to relieve thirst, which was excessive.

August 29th.—Feels comfortable. No vomiting since wash-out. Distension less. P. 100, temp. normal.

August 31st.—P. 90. Distension almost disappeared; able to take slight nourishment. Since then she has made good progress.

In both these cases the condition was recognised early, and stomach lavage commenced soon after vomiting appeared; but evidently the toxine infection was more severe in the first than in the second case. Whether the original infection had any hand in the cause in the first case I cannot say.

THE DIFFERENTIAL DIAGNOSIS OF APPENDICITIS IN YOUNG WOMEN.

By J. W. DUNBAR HOOPER, L.R.C.P., et. S. ED.

During the last five years, in private practice alone, sixty cases of inflammation of the appendix in women have come under my notice; of which forty were clearly associated with, or mistaken for, diseases of the reproductive organs. Of these latter cases thirty-one occurred in women under 30 years of age, and were operated upon; and in the following notes I have limited myself to these instances, entirely omitting children under 10 (four cases), and women over 30, of which the oldest was 58. For sixteen years I had under observation all the boys at Scotch College, East Melbourne, and during the whole of that time only two cases of inflammation of the appendix occurred, of which only one submitted to operation. Yet in five years, while medical officer to a large public girls' school, seven cases of appendicitis have come under my notice, of which five submitted to operation. Eleven cases were brought to me for opinion on a (supposed) purely gynæcological condition. Eight of these eventually proved to be an appendicitis, which, on operation, was associated with some lesion of the right ovary, or fallopian tube.

It seems to me that, until Howard Kelly's recent publication (1905), insufficient attention had been paid to the diseases of the appendix in young women. One is apt to regard all abdominal and pelvic complaints in women as arising from one or other of the many gynæcological lesions. It is, however, a very serious matter to allow a school girl, month after month, to dread her

menstrual period, to become morbid, invalided, unable to work for competitive examinations, to always have headache and malaise, when one thorough examination might elicit certain facts, viz., that either the appendix itself was to blame or adhering to one or other of the pelvic organs; and I am sure that, to-day, no parent or guardian would hesitate to allow the necessary operation on such a girl, once assured of its justice by a careful and competent medical man. Many girls, of course, suffer from anæmia, ovarian neuralgia, malaise, and headache, uric acidæmia, or even hysteria— "and some may have appendicitis on the brain" (Howard Kelly); but I do earnestly say that, of my own knowledge, some sad mistakes have occurred; as when a girl has been pronounced to be suffering from one or other of these complaints, whereas all the while she has had an inflamed and dangerously situated appendix. And when one thinks of it, such a state is not to be wondered at. School girls, certainly until quite recently, were never allowed sufficient muscular exercise; nearly always suffered from constipation, and it was even regarded as "unladylike" to own to such a common complaint. It is a matter of daily observation how little fluid, simple water, is drunk by school girls as compared with the ordinary boy who plays football, tennis, and cricket; and that water is necessary to wash away micro-organisms and flush the kidneys, and induce peristalsis. And when the resistance is lowered by constipation, anæmia, and copræmia, then bacteria, especially the B.C.C., travel into the appendix, and there or elsewhere, by their toxins, generate chronic blood-poisoning. This has been so frequently demonstrated that it seems a wonder to me how we can ever overlook the incidents.

Furthermore, we occasionally get those difficult cases where a young woman is said to suffer from gravel, urinary or renal calculi, and has to endure severe attacks of pain and vomiting, when probably the application of the X-rays would demonstrate the presence or absence of such calculi, and leave one free to investigate the condition of the appendix. I admit that it is exceptionally hard, in some cases, to undertake the responsibility of advising an operation for the removal of the appendix after only one mild attack of inflammation, when we are bound to say that such an attack *may* never be repeated. But, on the other hand, the laity is well educated in popular medicine, and many fully recognise the seriousness of the condition.

We now regard appendicitis to be due to micro-organisms which reduce the vitality of the patient, and quite possibly at any moment (and that a most inconvenient one—on a journey, or a visit to friend at a distance) these cocci cause toxæmia, inflammation, which may result in rupture of the appendix, suppuration, septic peritonitis, and death. I doubt very much if there is one medical man in this Section who, during the last ten years, does not know among his own acquaintance of some young life laid low before an operation had been carried out. It has occurred three times to me, within two years, to be called to little children under 5 years of age when they were *in extremis*, and subsequent examination proved suppurative septic peritonitis, due to perforated appendices, and in each case there was a history of frequent attacks of colic, for which a medical man had not been consulted. I feel very strongly that if, in my limited experience since I left the Women's Hospital, such a number of cases should come to me as a gynæcologist, many more must have come under the notice of my brethren in general family practice.

I will not trouble you with the various methods of operation, or of treating the wound; these will be dealt with in the Surgical Section. I merely wish here to draw attention to what, in my opinion, has been an unrecognised fact, namely, *that appendicitis, acute and chronic, occurs more frequently in young girls than we hitherto have believed.* Yet I do not want to become a

mono-maniac on appendicitis in girls! It would be only fair to say that, if I have sixty cases of appendicitis to report in five years' experience, I must, in these same years, probably have had nearly 600 gynæcological cases. In this vast continent so many of our people send their daughters to the cities, Melbourne, Sydney, Adelaide, Perth, and for their education, and these girls return home, many of them, to live in districts far away from medical advice. During these important years of their life they come under the attention of the medical officer of their schools, or have the advice of some practitioner in active family practice, and these are the years during which a diseased and disordered appendix can often be recognised, the parents or guardians warned, and on them rests the responsibility if the modern surgical treatment be not availed of while it is procurable.

I have reduced my experience to a table, for convenience and in order to make my paper as short as possible. If that paper is worthy of your notice, you will, I hope, find something of interest in every case; and I will ask you to specially observe that every appendix removed has been submitted for examination to an independent pathologist. And here I have to thank Dr. Mollison, Pathologist to the Melbourne Hospital, and Dr. MacKeddie, Clinical Pathologist to the Alfred Hospital, for very careful and interesting work. I do not feel that I have once needlessly exposed a young woman to an unnecessary operation, and I can look on these thirty cases as justifying the carefully considered advice given, namely, that, having once had an attack of inflammation in an organ "awkwardly placed" (as Worcester puts it) they should have that organ removed while it is fairly safe to undergo the operation under favorable circumstances, rather than await—it may be for a month or for many years—a second or third attack, when the inflammation may be so rapid as to preclude the chances of operation; indeed, the inflammation may occur at such a place, or such a date, that the sufferer is unable to even get the chance of an operation. I would venture to emphasize the fact that every one of these sixty cases—and I suppose that my professional brethren have had the same experience—has given me infinitely more worry, more anxiety in dealing with friends and relatives, than in managing the patient herself, or in considering the operation or its possible complications.

I do not allow time to be wasted in useless discussion, but seize the earliest and best moment for relieving a condition which at any moment may end in disaster. These cases require knowledge and decision, and, perhaps, above all, abundant confidence in aseptic surgery—though, as is well known, the peritoneum, if involved, leaves a large margin for error (Edmund Owen).

Inflammatory diseases of the appendix in women are frequently found associated with pelvic peritonitis, most often complicated with lesions of the right fallopian tube and ovary. The appendix itself may adhere to the bladder, the ureter, or broad ligament. In many cases the appendix is adherent to the posterior wall of the cæcum, or its tip may be fixed to the kidney or gall-bladder. The accompanying toxæmia of appendicitis is most dangerous when it exists without well-defined inflammation, being due to an excessive number of micro-organisms shut up in the appendix, and absorption of their toxins into the system by the lymphatics and veins. Pozzi and Tallamon and Landau have urged the importance of recognising the relationship between gynæcology and appendicitis. Dysmenorrhœa is, undoubtedly, in many cases wholly due to chronic inflammation of the appendix, with adhesions in the right pelvis. Howard Kelly records the practice of the John Hopkins' Hospital, that in five major gynæcological operations four may probably have these complications; and in all diagnoses of dermoids, ovarian cysts, parovarian cysts, uterine myomata, and ectopic gestations,

possible complications with the appendix are to be borne in mind. Therefore, we have to decide whether the case is one of appendicitis alone, or of pelvic disease, or of both : *and it is partly due to errors in diagnosis that appendicitis, being often overlooked in women, is, apparently, more common in men.* To-day Howard Kelly invariably removes the appendix in all abdominal operations, unless the condition of the patient absolutely forbids the extra strain. Duhressen and Rosenblum declare that chronic appendicitis is frequently the cause of uterine displacements, and Howard Kelly states that, out of eleven cases of appendicitis with adhesions, five certainly caused displacement of the uterus.

Some observers have claimed to find a definite arterial supply from the right ovary to the appendix, but Professor D. J. Cunningham failed to do so, and Howard Kelly denies its existence. It is mentioned in Quain's Anatomy. There is an undoubted relationship between pain in the appendix and the menstrual period, not only when the appendix is in the pelvis, but also when it is retro-cæcal. At these periods the splanchnic area is congested, and induces a favorable soil for the micro-organisms ; also, there is a definite relationship between rheumatic fever and appendicitis, and the latter is certainly influenced by the influenza bacillus. All the vascular supply of the appendix is from the right ilio-colic branch of the superior mesenteric artery, and undoubtedly the relatively strong muscular coats of the appendix render it capable of producing active peristalsis, inducing typical appendicular colic when inflamed or containing concretions.

It has been found possible to demonstrate the presence of a thin peritoneal fold passing from the mesoappendix to the adjacent iliac serosa in a median direction to the infundibulo-pelvic-ligament, also called the vascular pedicle of the ovary. On traction, these folds can be recognised distinctly with their blood-vessels and lymphatics, and thus there would be a connection between the vessels of the appendix and ovary, or between the portal and systemic circulations ; yet Kelly positively denies the existence of any considerable vascular or lymphatic connection between the ovary and the appendix, for he points out that the blood-vessels of each are completely formed before they become neighboring organs. He doubts whether the ovary can be injected from the appendix through the ligament, and, in certain experiments, not one single lymph channel was seen to pass in the peritoneum towards the ovary. But Lafforgue is satisfied that he found the appendicular ovarian ligament in seventeen cases out of ninety subjects. Treves denies that the cæcum has a mesentery. As a matter of fact there are nine anatomical directions to which the appendix may point, and there is no region, from the liver to the pelvic floor, where the appendix may not be found, the malposition being due either to pelvic adhesions or to arrested fœtal development.

The lymphatics of the appendix are subject to great variations, and at the base of the mesoappendix, in the angle which separates the appendix and cæcum from small intestines, there lies a gland—the appendicular gland—and it attains to considerable size. It may be considered the last of the misnamed mesenteric glands (Claudo). The nerves of the appendix are supplied from the superior mesenteric plexus, and divided into the plexus of Auerbach and that of Meissner. There is a small number of ganglia in the mucosa connected with one another, and forming a plexus with the glands of Lieberkuhn, and a careful microscopical examination of apparently healthy glands in the neighborhood of an inflamed appendix has revealed B.C.C. in those glands ; and in a case of suppuration, after removal of diseased appendix, the wound was reopened and the glands removed, and found infected with B.C.C., and the patient recovered.

The primary existing causes of an acute attack of appendicitis are constipation, and an undigested meal, setting up acute catarrh of the colon and cæcum.

Has the appendix any functions ? It exists in man, in rodents, and in apes, and is peculiarly rich in lymphoid tissue, like the tonsils, and has been called the abdominal tonsil. The carnivora have no appendix. Many observers are convinced that the appendix has some unknown influence on digestion in the cæcum, and certainly it has regular and powerful peristaltic action. Whether the appendix has any healthy function or not, unquestionably we know of certain factors which give rise to pathological conditions in the organ. For instance, deficient mastication, bolting the food, chronic dyspepsia, and the infection of a bolus of food by numerous micro-organisms which find their home in dental caries (William Hunter on " Oral Sepsis and Anæmia "). And in the aged, or gouty, or syphilitic, an endarteritis may favor gangrene of the appendix, and the danger of kinking is that it may effectively cut off the entire blood supply to the appendix. The inflammation, or catarrh, stimulating the nerve endings in the lumen thus causes hyperalgesia. Mayo Robson doubts whether appendicitis has actually increased, though he admits our methods of diagnosis have vastly improved. In 1892 many cases of suppuration in the iliac regions were operated upon in the London Hospital, and the proportion of cases was 2.5 males to 2.1 females.

Recent bacteriological research has proved that, in inflammation of the appendix, there are found within the lumen of the tube, and along the lymphatics, streptococci, more rarely staphylococci, the bacillus coli communis, various other micrococci, Weiselbaum's intra-cellular diplococcus, the bacilli of pneumonia, influenza, and diphtheria ; actinomycosis, and the bacillus of amœbic dysentery (Osler, 1890). There is a distinct analogy between catarrhs of the nose, the urinary and gall-bladders, in the formations of various calculi, rhinoliths, or gall-stones, and those concretions of the salts of lime and magnesia, with inspicated masses of bacteria, and some little fæcal matter, which are found within the appendix itself (Sherren, in *The Practitioner*, June, 1905). Sir Frederick Treves regards the presence of foreign bodies as occurring only in 3 per cent. of the total number of cases of appendicitis.

Signs and Symptoms.

(1) Onset of pain just over McBurney's point.

(2) Hyperæsthesia and muscular rigidity of the right rectus.

(3) Nausea and vomiting.

(4) Pyrexia ; rigor seldom.

(5) Altered pulse rate.

(6) Constipation, almost amounting to obstruction.

(7) Right knee slightly drawn up.

(8) A tumor or swelling in the right iliac region, cylindrical, size of a finger, steadily enlarging and painful on pressure, and defined by rectal examination under anæsthetic.

(9) Thoracic and quickened respirations.

(10) General appearance much altered, face flushed or livid, nose sharply pinched, and tongue thickly furred.

(11) A rising leucocytosis.

Complications.

Adhesions between the tip of the appendix and bladder cause dysuria, and, to the rectum, cause proctitis. Careful examination is necessary to differentiate from renal lesions, especially calculi in kidney or ureter, often

detected by the X-rays; from pneumonia, by thorough use of the stethoscope; and from typhoid fever, by the Widal agglutination test; from ovarian tumor, with twisted pedicle, or from movable kidney and torsion of the ureter and renal vessels, by vaginal or rectal examinations under an anæsthetic. Intestinal parasites are often present, especially the ascaris lumbricoides and thread worms.

In *children* acute thoracic disease may stimulate, or be masked by, abdominal symptoms. With renal calculi we generally have dysuria, hæmaturia, vomiting, and collapse. If the calculi are lower than the brim of the pelvis, there may be much difficulty in detecting them by the skiagraph. A peri-renal abscess may travel along and down the psoas muscle to the thigh or inguinal region, and it is well known that peri-appendicular suppuration can burrow up and behind the kidney, and even penetrate the pleura or the lung tissue.

Tenderness in the right iliac region is due to increased tension within the lumen of the appendix, or in the adjacent parts; and the disappearance of superficial tenderness, *without* other evidence of improvement—in aspect, pulse, temperature, and mobility of the rectus muscles—calls for immediate operation, as it so often follows perforation or rupture of the appendix (Bottomley, *The Practitioner*, June, 1905).

Appendicitis is very rare in infancy In 2,000 autopsies of children under 2 years old appendicitis was never once found (Holt). I have thrice seen it in children aged 3 years, and from this age, and up to 10 years, we have to distinguish it from simple colic, constipation, intussusception, psoas abscess, tubercular peritonitis, and pneumonia at the base of the lung, when a daily leucocyte count would assist.

Howard Kelly (1905) states that biliary calculi have never been found in the appendix. Many surgeons have mistaken certain facets on the fæcal concretions as evidences of gall-stones.

Chronic appendicitis has been mistaken for hip-disease by surgeons of excellent reputation and large experience, and Kelly records two graphic instances. It is obvious, therefore, that, in cases of possible appendicitis, one has to sit down at the bedside and cautiously eliminate many other lesions, some very obscure.

Lesions Likely to be Mistaken for Appendicitis.

(1) Tumor of the right ovary, or inflammation of the same.
(2) Extra-uterine pregnancy.
(3) Worms in the intestinal canal.
(4) Meckel's diverticulum, causing obstruction.
(5) Stone in right kidney or ureter.
(6) Movable right kidney and torsion of pedicle.
(7) Intestinal or omental adhesions (? tubercular).
(8) Calculi in biliary tract.
(9) Obstruction, due to impaction of fæces in cæcum.

Once the diagnosis is established, every clear case of appendicitis should be operated upon within the first twenty-four hours; in the meantime hot fomentations being constantly applied, as long as the skin does not become tender; an enema of hot oil and soapsuds administered—and I agree with those who do not give calomel, but rely on the castor oil draught. At the end of twenty-four hours, the indications for operation would be—

(1) Pains about the umbilicus or McBurney's point.
(2) Muscular rigidity in right iliac fossa.

(3) Tenderness in right iliac fossa; the sudden diminution or actual cessation of pain in that region *without improvements* in other directions, is an indication for immediate operation, because so frequently preceded by perforation or rupture of appendix.

(4) Increase of the localised swelling.

(5) Continued nausea and vomiting.

(6) Constipation, the rectum being emptied by enema.

(7) Elevation of temperature and pulse.

(8) Rising leucocytosis.

(9) Sudden seizure, with pain in the abdomen, with or without collapse.

(10) Ileus, the gravest symptom, and admitting of no delay.

It is most important not to dawdle or to wait for the presence of relatives, guardians, or friends, not only in the patient's best interests, but, as has been well put—a surgeon is never so unhappy as when watching a case of appendicitis, and noting progress or retrogression when it has not been operated upon; and even the most desperate cases of suppurative appendicitis, with peritonitis, sometimes pull through.

RETRO-DEVIATIONS OF THE UTERUS.

By T. G. Wilson, M.D., Ch.M., Syd., F.R.C.S., Edin.

One of the most striking features in the recent literature on the subject of retro-deviations of the uterus is the number of writers who maintain that those symptoms, which we have hitherto been accustomed to look upon as being due to some backward displacement, are caused not by this displacement but by some existing complications. One writer (1) at the recent Pan-American Congress at Panama, maintains that there is no normal position for the uterus, and that, whatever the position of this organ, it is only on the supervention of some complication that symptoms manifest themselves. On the other hand, there are authorities who maintain that a retro-deviation is essentially a pathological condition, and requires treatment whether there are any symptoms directly referable to it or not. Thus, W. D. Haggard (2) Professor of Gynæcology at the University of Tennessee, in a recent address on this subject, says:—"To disregard a case of retro-deviation simply because it is giving rise to no symptoms at the time is comparable to disregarding many cases of renal and cardiac disease, gallstones, and errors of refraction, &c., which are often discovered by accident, and to assume that because they are causing no symptoms at the time that they will never do so . . that, inherently, the causes which produce the retro-deviation will also engender complications . . . . that there is always the element of infection to be considered in the female pelvis, and it is this element of infection that determines the presence or absence of complications, such as adhesions, etc."

Certainly when a uterus becomes fixed in its retro-deviated position we may consider that the fixation is an important element in the symptomatology, and, whichever standpoint we take, the question of whether the uterus is fixed or free is a most important factor from the point of view of treatment.

On going through the notes of 900 consecutive gynæcological cases seen recently, I find that the uterus was in a position of retro-deviation in 153 cases, or a frequency of 17 per cent. Of these 153 cases I have noted that in 39 cases the retro-deviation was a result of, or coincident with, some degree of prolapsus

uteri secondary to pelvic floor weakening. Of the remaining 114 cases, in sixty-nine the retro-deviation was complicated, leaving forty-five cases, or 5 per cent. of the total number, in which the uterus was freely mobile and could be replaced in a forward position. I have noted the symptoms in these forty-five cases, with a view of determining whether they were to be considered as due to the retro-deviation or to complications present. In nineteen cases the symptoms the patients complained of were more directly referable to some accompanying condition—endometritis, retained decidua, &c.; while of the remaining twenty-six cases, in whom there was no other abnormal condition present except the retro-deviation, I found that in twenty-three cases the symptoms complained of were apparently directly referable to the retro-deviation itself, some at least being produced mechanically by the position of the uterus and adnexa.

In regard to such symptoms as menorrhagia, metrorrhagia, &c., while we must consider these as being directly referable to the condition of the endometrium, we must admit that, in the majority of cases, such complications cannot be effectively treated without at the same time rectifying the backward displacement of the uterus; and whether we are to consider a retro-deviation without symptoms as pathological or not, it would seem as if the patient's general health gets below par, and if any infective condition —endometritis, &c.—arises, the retro-deviation becomes an important factor in exaggerating and keeping up the condition, and therefore demands active treatment.

The treatment of those cases which are coincident with prolapsus uteri will naturally be from the point of view of the prolapse, and any method which aims at correcting the retro-deviation, without at the same time restoring the integrity of the pelvic floor, will be doomed to failure; and, for this reason, I purposely exclude such cases altogether in considering the treatment of retro-deviation proper. In regard to the treatment of mobile retro-deviations by mechanical means—*i.e.*, pessaries—I am quite aware that amongst many gynæcologists pessaries are considered a thing of the past, and, if the condition requires treatment at all, that operation is advised; but I cannot help thinking that there are certain cases where the pessary has a distinct sphere of usefulness, and should be the method of choice. I refer especially to cases of mobile retro-deviation occurring after confinement, in whom a well-fitting pessary will keep the uterus in a forward position, and help to prevent a long convalescence due to sub-involution. I have followed up enough of these cases to convince myself that, after a time, the pessary may be discarded without the retro-deviation recurring. This period after confinement has been aptly termed the "psychological period of the pessary."

With regard to the treatment of fixed retro-deviation by massage, after the combined vagino-abdominal method of Brandt, this method has never gained any foothold in English-speaking countries, and the objections to its use are fairly obvious, but it has been extensively practised in Continental hospitals; and while working in Professor Chrobak's clinic in Vienna I had some chance of seeing the method of procedure, and examining the patients before, during, and after the treatment. Naturally it is all-important that the cases should be carefully chosen, and when this is done it is quite certain that good results can be obtained. I have seen many cases with fixed retro-deviations who would certainly here be advised to have operative interference, in whom, after several sittings, the uterus has been got into forward position, and kept there temporarily by means of a pessary, and after a time it has been possible to discard the pessary without recurrence of the retro-deviation.

The operative treatment of retro-deviation presents a large scope, and the number of operations suggested is a fairly good index that no one of them

is generally applicable. Indeed it is rare to go through a gynæcological journal without seeing some new operation, or modification of some old operation, designed for the cure of this condition. Probably the most usual and most favored operation at the present time is some form of ventro-suspension or ventro-fixation; and, as regards the after-result, the cases I have had a chance of following up would indicate that the distinction between these two operations depends chiefly on whether the ligatures used for the suspension or fixation are absorbable or not, as a ventro-fixation done with an absorbable material, as catgut, in the course of a few weeks becomes practically a ventro-suspension. Apparently tendon, either the formalin prepared or the ordinary biniodide tendon, is very often not absorbed, or at any rate for such a time as to make it justifiable to consider it as an unabsorbable material. I have frequently seen tendon, which has been buried in the abdominal cavity for periods up to two and even four years, which was apparently unaffected at the time of a second operation, and on cutting sections through such tendons little or no leucocytal infiltration was to be seen, and as regards tensile strength it did not seem to have suffered at all. Catgut is always absorbed quickly, so that probably the adhesions formed round an absorbable ligature like catgut will tend to be less dense than round an unabsorbable ligature like silk or tendon, round which the adhesions, being denser, will naturally persist longer and not tend to disappear while the foreign body remains. As a result of these operations the uterus is changed from a pelvic to an abdominal organ, for a time at least, and from being a freely mobile organ it is made into a more or less fixed one; and we have to consider what the result of this fixation will be should the patient become pregnant, and also whether the operation will permanently keep the uterus in its forward position. In considering this operation there are such a number of modifications of doing it that it becomes difficult to compare them. While some operators insert their sustaining sutures on the anterior surface of the uterus, others use the fundus; and others, again, following Kelly's original suggestion, insert the sutures on the posterior surface of the uterus, in order to allow the intra-abdominal pressure to act on that surface. Then some operators use unabsorbable sutures, silk, silkworm-gut, &c., and others use catgut always. There are very different views, too, as to the ultimate result of the operation. The question of what really does happen to the suspensory ligament after a ventro-suspension is an interesting one, and I have kept notes of cases I have examined at varying periods after this operation, and also of cases where it has been necessary to reopen the abdomen at a subsequent operation. In thirteen of these the second operation was done at the Adelaide Hospital, where I have assisted at the operation, and I am indebted to Dr. Hamilton for these cases. First, in those cases where the ventro-suspension was done with tendon or silk: I have notes of eight of these, six done with tendon and two with silk, and all of whom I have examined at periods from six months to five years after the first operation. In six of these cases the uterus was still in a forward position, and a definite suspensory ligament was palpable on bimanual examination. In three of these six cases the abdomen was re-opened, and the actual condition of the ligament could be observed. Two of the ligaments were solid and firm, suggesting that they might cause trouble by preventing the expansion of the uterus during pregnancy. The third was much thinner, and stretched out to a length of 2 inches, and did not apparently take any active part in keeping the uterus forwards. In the other two cases no suspensory ligament could be felt, but the uterus was again in a position of retro-deviation. One of these patients had had a full-time child since the original operation, and the abdomen had to be again reopened on account of further adnexal inflammation. In this case also there was a thin

stretched-out, suspensory ligament, stretching from the old abdominal incision to the fundus uteri. So that in seven out of eight of these cases there was a suspensory ligament left as a result of the operation, though in two cases it had apparently stretched to such an extent as to cease to be of service in holding the uterus forwards.

These are the only cases I have been able to follow up which have been done with tendon or silk, as for the last four years most of the ventro-suspensions at the hospital have been done with catgut. I have notes of twenty-seven cases in whom a ventro-suspension was done with catgut, and whom I have examined at periods varying from six months to four years after this operation.

First, in regard to the position of the uterus. In eleven cases the uterus was in good position at the time of the second examination; in sixteen cases it was again retro-deviated. In none of these cases examined after six months was any suspensory ligament to be felt on bimanual examination. Of these twenty-seven cases the abdomen was reopened in seventeen of them, and the actual condition of the pelvis could be ascertained, and in none of these cases was any sign of a suspensory ligament discovered. The shortest period after the first operation that the abdomen was reopened was four months. Six of these cases had had the anterior aponeurosis included in the suspensory ligatures, so that the original operation was a fixation. In the ten cases which were not reopened it is impossible to say for certain whether any sign of a suspensory ligament would have been found, but it is certainly remarkable that in all the cases that were reopened there was no suspensory ligament found. In some of these cases the catgut used was formalin catgut, in others biniodide catgut, and in others iodine-prepared catgut. While I have recorded these results as I have found them, I naturally realise, from hearing of the experience of others, that sometimes a suspensory ligament does persist after a ventro-suspension done with catgut, and especially if there has been any suppuration in the abdominal wound. Taking these two series together, we get thirty-five cases who had had a ventro-suspension done, and at varying periods from six months to five years afterwards the uterus was found not to have retained its forward position in eighteen of them. While agreeing that it is not a fair criterion to take eighteen out of thirty-five cases, as during this period there were a great many other cases who did not report themselves, and who were, presumably, well, and also admitting that in some of these cases the retro-deviation had recurred as the result of a fresh inflammatory process, the fact of finding that the operation had not fulfilled its object in such a number of cases indicates that a ventro-suspension, as generally done, and especially when absorbable ligatures are used, cannot be regarded as really effective in permanently maintaining the forward position of the uterus.

The above results would indicate that a ventro-suspension done with catgut does not fix the uterus permanently to the abdominal wall, but merely holds it in a forward position for some months, and so gives the uterine supports time to regain their tone; while when an unabsorbable ligature is used the uterus, as a rule, is definitely suspended by a cicatricial ligament. It seems rather difficult to believe the statement we often see that such a ligament will stretch with pregnancy, and involute after delivery, and so still hold the uterus forwards. I have attended five cases in confinement who have had a ventro-suspension done, one with silk, one with tendon, and three with catgut, and in all these cases the confinement was easy. Four other cases who had had ventro-suspensions done, and who have had what they described as normal labors, are included in these thirty-five cases. Of these nine cases in only three was the uterus in a forward position after the confinement, which is instructive, as indicating the importance of parturition

as a causative factor in retro-deviations, and also how unlikely it is that the suspensory ligament, if one should be present, will stand the strain of the growing uterus during pregnancy and still hold the uterus forwards after confinement.

It is generally admitted that a ventro-fixation proper is not a justifiable operation in a woman who is likely to have children, though even now one constantly sees accounts in the journals of cases where Cæsarean section has been necessary when such patients have gone to term. In regard to this, it is of interest to note that Andrews (3) in a recent review of the effects of ventro-fixation and ventro-suspension on subsequent pregnancy, has collected 395 cases of labor following these operations, and of these there were twenty cases where Cæsarean section was performed as a direct result of difficulties caused by the previous operation. Of these twenty cases fourteen followed ventro-fixation, and six followed some form of ventro-suspension. He came to the conclusion that the method which caused least difficulty with subsequent labor is a ventro-suspension, though he adds that this operation has the theoretical objection that the suspension may not be permanent. In a great number of these cases the original operation was done on account of retro-deviation accompanying prolapsus uteri.

While the position of the uterus after the removal of the ovaries seems to be of little consequence, if it is wished in such a case to put the uterus in a forward position there would appear to be no objection to a ventro-suspension done with catgut; but in those cases in which the abdomen is opened, and the ovaries not removed, what is to be done to correct a retro-deviation? If a suspension with catgut be done, judging from these cases, we can never tell exactly what will happen afterwards. If the uterus remains forward, well and good; but if not, can we ask the patient to have another suspension done every few years, or after each confinement? Or are we to use an unabsorbable ligature, and aim at having a firm suspensory ligament left, which may cause trouble with a subsequent pregnancy, or, if not, may stretch so as to cease to be effective as a sustaining ligament, and which will remain as a potential source of danger from giving rise to internal strangulations, &c.? That this is a real possibility we have only to read the accounts of such cases recorded by Jacobs (4), Leopold (5), Rufus B. Hall (6), Thomas (7), Lindtors (8), and many others. Is there no other method by which the uterus can be kept forwards without leaving a permanent source of danger in the abdominal cavity? The different methods that have been suggested by the vaginal route do not seem to have been very effective, though they all have their warm advocates. Shortening of the so-called utero-sacral ligaments, vaginal shortening of the round ligaments, Pryor's operation and vaginal fixation may be mentioned, though this last method is now generally considered as unjustifiable in a child-bearing woman. The abdominal route is at present the favorite one, at any rate when the condition is complicated; for, as a rule, it is these complications, and not the retro-deviation, that demand a section. For an uncomplicated case, if it requires surgical treatment, the Alexander-Adams operation is certainly the method of choice; and some operators (9) apparently prefer to finish up with this operation after having dealt with the pelvic condition through a median incision, though the extra element of the time required must be a disadvantage. Noble's suggestion of using a transverse incision, after the method of Pflannelstein, in these cases, and, after dealing with the pelvic condition, shortening the round ligaments extra-peritoneally from the extremities of the original incision, would seem to be the ideal operation, at any rate where a large abdominal incision is not required. The different methods of shortening the round ligaments intra-peritoneally—Wyllie's, Mann's, and Baldy's operations—

ERRATUM.

Page 279, *for* "FRANK A. NYLUASY" *read* "FRANK A. NYULASY."

while they all have the, to my mind, theoretical objection of leaving the weakest parts of the ligaments—the part in the inguinal canals—as one of the points of fixation, certainly do maintain the forward position of the uterus, and apparently do not interfere with subsequent pregnancy. Gilliam's operation, which has been warmly advocated by many American gynæcologists, is open to the objection that an opening is left on either side where a coil of gut may become strangulated, though it has been suggested that the parietal peritoneum may be sewn to the round ligament in order to obliterate these two spaces. All these operations are more or less on trial, and until the ultimate results as regards their permanency, and especially the effect of subsequent pregnancy, are more fully recorded, it is impossible to lay down any fast rule ; but, granting that they do not cause interference with gestation and parturition, when surgical interference is adopted for the cure of a retro-deviation in a child-bearing woman the utilisation of the round ligaments to effect this certainly seems to be the most rational procedure.

References.:

1. Waite, *Medical Record*, February 4th, 1905.
2. Haggard, *American Gynæcology*, September, 1903.
3. H. R. Andrews, *Journal of Obstetrics and Gynæcology of British Empire*, August, 1905.
4. Jacobs, *Deutsche med. Wochenschrift*, 1894, page 489.
5. Leopold, *Monatschrift fur Geburts, u. Gyn.*, July, 1895.
6. Hall. *Journal of Medical Association*, 1899.
7. Thomas, *American Gynæcol. and Obstet. Journal*, February, 1901.
8. Lindtors, *Centrablatt f. Gynæ., Bd. XXII.*, page 414.
9. Cleveland, *American Gynæcology*, July, 1902.

A RARE FORM OF PELVIC TUMOR—OPERATION—RECOVERY.

By Frank A. Nyluasy, M.B., Ch.B., Melb.

My reason for reporting this case is that the condition—cyst of Gartner's duct—is such an exceedingly uncommon one. It is not even referred to by Howard Kelly, for instance. Bland Sutton is the only authority, so far as I am aware, who has described it. Herman and Jellett both mention the subject, but their accounts are evidently derived from Sutton's. I have myself never previously seen a case like it, nor have the medical men who assisted me at the operation.

The patient in the present instance is a young married woman, aged 27 ; slight in build, with a somewhat anxious expression. She has, for the past four years, complained of constant aching pains in the back, tenderness in the left ovarian region, severely painful scanty menstruation—worse since her marriage, four years ago—constipation, and occasional difficulty in urination.

On examination I found the cervix lying up under the pubic arch, and the narrow end of a fluctuating tumor dipping down behind the cervix, and in close contact with it. A distinct fulness in the right iliac fossa could be detected on palpation, but was not obvious on inspection, which probably accounted for the fact that the tumor was completely overlooked by previous medical attendants. I had the greatest difficulty in convincing the patient that she had anything so serious as a pelvic tumor : all sorts of reasons but the right one had been given to account for her symptoms. It was only

after I called in the aid of another medical man, to make a conjoint examination under chloroform, that I succeeded in convincing her (almost completely) that she had a pelvic tumor at all. (She was really only fully convinced when she was shown the tumor after operation.) The fact that she had no startling symptoms, and that the tumor grew downwards into the pelvis, without producing obvious enlargement of the abdomen, and that previous medical attendants had asserted that there was nothing seriously wrong, explains the reason of her incredulity. After some weeks' consideration, however, she submitted to the operation. This was performed on February 17th, 1904.

Operation.—An incision, 3 inches long, was made in the midline below the umbilicus. A part of the cyst was seen lying snugly in the right iliac fossa. There was a little ascitic fluid surrounding the growth, and its peritoneal covering had a dark deeply-congested appearance. The fallopian

tube and upper part of the meso-salpinx were quite free from the cyst; the abdominal end of the tube was closed. The ovary lay on the anterior and outer part of the cyst, but was not attached to it anywhere. The cyst extended right across, behind the uterus, to the opposite side, and lay in contact with the left broad ligament, which was wrapped firmly round it. The left fallopian tube was dilated into a hydrosalpinx as large as small intestine; the corresponding ovary was dilated into a cyst the size of a small mandarin orange; and both were bound down by universal adhesions. The uterus was displaced forwards by the main cyst, and had also been pushed over to the left as the growth enlarged. [Even after the main cyst had been removed, the uterus retained its left-sided position until its multiple adhesions with the left broad ligament had been severed.] On cautiously passing the fingers behind the tumor, as far as they could safely reach, they dipped right down

into the pelvis, where many bands of adhesions were felt crossing in an antero-posterior direction, between the pelvic and tumor walls. The tumor itself was situated between the walls of the mesometrium, and was everywhere densely adherent to it. The tumor, moreover, had lifted up the posterior wall of the peritoneum, and there effected some important attachments. The bladder, as far as could be felt at this stage of the operation, was free from the cyst. However, the absolute impossibility of releasing the growth, without previously emptying it of its fluid contents, was thus made clearly evident. A number of flat sponges were thereupon placed in the abdomen, so as to completely surround the cyst, from which two and a half pints of perfectly clear watery fluid were now evacuated by tapping. Then began a process of enucleation, the difficulty of which can probably be best appreciated by those who have had experience in the removal of an adherent cyst that has burrowed deeply into the pelvis between the layers of the broad ligament (and this growth had extended down to the cervix uteri, as felt per vaginam). The collapsed cyst was drawn up as far as possible, and the process of separation began at its outer side, and was continued towards the front. Many of the adhesions on the anterior part of the cyst were so dense that they could not be torn through with the fingers, but had to be cut with scissors.

The right ureter was flattened out into a broad band, firmly attached to the base of the cyst wall, the peritoneum having been lifted off the ureter by the growing cyst. The ureter was separated from the cyst by the aid of dissecting forceps and scissors. This being accomplished, the enucleation of the rest of the cyst was comparatively easy; but, at best, such an operation tries one's patience greatly. Fortunately, hæmorrhage was not severe, so that it was unnecessary to ligature the infundibulo-pelvic ligament. As soon as the cyst was removed, attention was directed to the other side. It was difficult to get hold of the ovary here, owing to the adhesions and displacement of the uterus. This ovary was at length reached, and was found to be about three times its normal size: it burst during removal, discharging clear fluid. The same thing happened with the hydrosalpinx, owing to the density of the adhesions and the difficulty of their separation, but the clear fluid was caught by the surrounding sponges. The fallopian tube, which formed this hydrosalpinx, was dilated to quite the size of small intestine, and was at least 6 inches long, so that its separation was also tedious and troublesome. Ligatures were placed on both ends of the corresponding broad ligament. The

patient was now left with one ovary and one fallopian tube—on the right side—but, unfortunately, this tube had been sealed up by inflammation, so that all chance of the patient realising her hopes of future pregnancy was out of the question, unless the closed condition of the tube could be rectified. The abdominal end of the fallopian tube was, therefore, slit up, and a few silk sutures put in to keep it open. The patient has now a possible chance of one day becoming a mother.

With all the manipulations required to separate these very adherent growths, it was a little difficult to decide whether drainage should be adopted or not. Having made sure that there was no bleeding into the pelvis, and feeling convinced that there was nothing septic to be feared, either from the growths or the sponges, instruments and ligatures, it was decided to dispense with drainage altogether, and the wound was, therefore, closed in three layers and dressed with iodoform gauze and a flannel roller bandage.

I may remark, in passing, that it is quite unnecessary to suture the separated walls of the broad ligament, as recommended by Howard Kelly, Dudley, and many others, inasmuch as these walls fall together nicely after the removal of the contained cyst, and there is no great amount of oozing between them. As Bland Sutton states, " Enucleation is usually accompanied by more loss of blood than simple ovariotomy, and the prolonged manipulation is often responsible for severe shock."

Although this tedious operation, lasting an hour and three-quarters, was done at the patient's own house, on one of the hottest and most oppressive days of the year, she did not suffer much from shock, and she had little or no vomiting.

In speaking of Gartnerian cysts, Bland Sutton further remarks, in his work on the " Diseases of Women," that " cysts of this character, which burrow deeply, often entail risk in removal, as they lie in intimate relation with uterus, ureter, and bladder. The cyst, when large, will come in contact with the iliac arteries and veins at the brim of the pelvis, and even rest upon the inferior vena cava. Gartnerian cysts arising in the terminal segment of the duct project into the vagina. In some instances these cysts may be treated surgically through the vagina with greater success than by cœliotomy." For my part, I should like to know what those instances are, and how to differentiate them. In the meantime, and after the experience of my own case, I should view the advice to operate on Gartnerian cysts through the vagina as likely to lead to the most disastrous results.

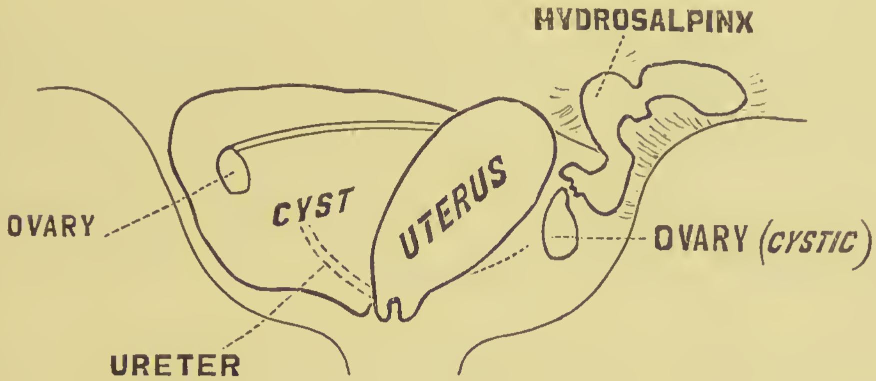
After the operation I saw the patient at six o'clock in the evening, when she was complaining of a good deal of pain, as was only natural after the separation of so many adhesions. I therefore varied my usual rule of withholding morphia, and gave her her first and only dose, of one-quarter of a grain, hypodermically. But she was otherwise remarkably well, and, in fact, made the easiest recovery I have ever seen, either in my own practice or in that of others. She had one rise of temperature to 100° on the day following the operation, and thereafter everything was normal. She never required a catheter, and menstruation came on at the expected time—two days after operation—without pain. She vomited twice slightly after the anæsthetic. The bowels acted freely after an enema on the third day. She was given sips of hot or cold water whenever she desired, as I altogether disapprove of the quite unnecessary practice, initiated by Martin, of absolutely withholding drinks shortly after abdominal operations. This practice is cruel in the extreme, and I agree with Professor Mummery that it has absolutely nothing to justify it. In fact, the more I see of these operations the more convinced I am of the harmfulness of a rigid adherence to what Treeves calls " the abdominal ritual."

My patient was allowed to get up at the end of the third week, the external wound having united perfectly; and she has not only remained quite well, but has greatly improved in health since the operation, eighteen months ago.

I must here express my thanks to Dr. Rennie for his able assistance at the time, and to Dr. Trood for his skilful administration of the anæsthetic—chloroform, followed by ether.

Appended is a rough sketch of the growth *in situ*, and a photograph of the same after its removal. The main cyst measured 8 inches by 5. The shaded photo. exhibits some of the numerous adhesions very clearly. The peculiar tube-like prolongation at one end is also seen, but it appears much shorter in the photograph than it really is. The remains of the removed ovary, and one-half of the previously dilated but now shrunken fallopian tube, are also depicted.

The rarity of the affection is my excuse for reporting it at length.



SECTION OF EYE, EAR, AND THROAT.

EYE DIVISION.

INTRODUCTORY ADDRESS.

By W. Odillo Maher, M.D., Ch.M., Dub., M.R.C.S.

President of the Ophthalmological Section.

Gentlemen—My first duty is to express my appreciation of the marked distinction conferred upon me by the members of the Executive Committee of the Australasian Medical Congress in electing me to the position of President of the Section of Ophthalmology, and to thank them for what I consider to be one of the highest honors they had in their power to bestow. The invitation came to me as a surprise, and my first inclination was to decline, fearing I should not be able to adequately discharge the duties of this office. On reflection, however, I deemed it my duty to accept, for I felt that the compliment was intended, not only for me personally, but also for the State to which I belong.

The first difficulty which presented itself was the selection of a subject for an address worthy of your attention. After much consideration, it occurred to me that, as it was nearly a quarter of a century since I first commenced the study of Ophthalmology, a brief sketch of some of the principal advances that have taken place within my own observation would not be wholly devoid of interest.

It was in the year 1881, when I was appointed Clinical Assistant to the Royal London Ophthalmic Hospital, Moorfields, that I commenced the study of diseases of the eye. At that time the consulting staff of the Moorfields Hospital consisted of Drs. Critchett, Bowman, Dixon, and Hutchinson; and the visiting staff of Drs. Wordsworth, Streatfield, Hulke, Lawson, Adams, Lyell, Cooper, Tweedy, and Warren Tay. Of the former only one, Mr. Jonathan Hutchinson, and of the latter only the last three mentioned, survive. Then, too, it was my good fortune to meet our courteous and energetic Secretary, Dr. Symons, with whom I was intimately associated for two years—first as Clinical Assistant, and afterwards as House Surgeon.

Looking back, one cannot but be deeply impressed by the vast amount of research and useful original work performed by many arduous and earnest workers in this department of medical science. Still we must all feel how much remains to be accomplished; how little we can do to arrest the progress of many of the cases of optic nerve atrophy, how ineffectual is our treatment of some of the diseases of the eye—as, for instance, detachment of the retina—and how limited is our knowledge of the direct causes of many of the changes met with in the fundus oculi.

Owing to the varied details, and to the limited time at disposal, it would be impossible to give more than a brief—and, I fear, a somewhat disjointed—retrospect. I therefore propose confining myself to some of the advances I consider most noteworthy.

I well remember the first demonstration of retinoscopy at Moorfields. It was given by a former House Surgeon, who had just returned from a trip to the Continent. Its simplicity, approximate accuracy, and its advantage as an objective test were at once appreciated, and within a very short time it was adopted by all. Retinoscopy was first suggested in 1874 by Cuignet, who called it keratoscopy. Bowman, however, about ten years before this, drew attention to the possibility of diagnosing astigmatism by means of the ophthalmoscopic mirror; but, unfortunately, he did not follow the subject up. Retinoscopy affords us an invaluable means of estimating the refraction of children, and of those who are mentally deficient, or whose powers of observation are of a low order. By this means the refraction of the whole of the media can be estimated. In this respect it differs from, and is far superior to, keratometry, another objective method of estimating refraction to which much attention has been given of recent years. The most improved keratometer, or ophthalmometer, is probably that of Javal Schiotz. It is an excellent instrument for determining the amount of corneal astigmatism, and the axes of the greatest and least corneal curvatures; but, beyond this, affords no information.

During the period under review our views as to the treatment of myopia have undergone a material change. Formerly it was unusual to prescribe the full correction for constant use, and it was thought that the strain of the ciliary muscle during the effort of accommodation produced an increase of the myopia with its deleterious effects; now it is believed to be the convergence of the optical axes which plays the most important part in causing this increase; and many cases have been recorded which show that, when the full correction has been worn constantly for all purposes for a number of years, there has been no increase in the myopia in the majority of instances, while in many the increase has been slight, and in but few has it been marked. Hence the practice nowadays to order, when the eyes are healthy, the full correction for constant use except in very high degrees, and if the myopia increases, to increase proportionately the strength of the glasses. In myopia of twelve dioptres and more the removal of the crystalline lens, when successfully performed, improves the vision immensely. This operation was first described and recommended by Desmonceaux in 1776, and later, in 1817, advocated by Beer; but it remained for Fukala to bring it prominently under the notice of the profession. Many cases of detachment of the retina following this operation have been recorded; still the advantages derived by the majority of patients are so great that there can be little doubt but that this operation will long continue to hold a place in ophthalmic surgery.

Errors of refraction as a cause of headache are now more generally recognised by physicians than formerly, and, consequently, such cases are more frequently referred to ophthalmic surgeons.

Comparatively recently Helmholtz's theory of accommodation has been questioned by Tscherning, who maintains that during accommodation the anterior surface of the lens, instead of becoming spheroidal, assumes a more or less conical shape. This he considers is caused by a tightening of the zonula. Karl Grossman more recently has had an opportunity, in a case of aniridia with small opacities on the anterior and posterior poles of the lens, of studying the changes which take place during accommodation, and his observations confirm Helmholtz's view as to the relaxation of the zonula; but also show that the anterior surface of the lens, instead of becoming spheroidal, becomes conical at the centre, and flattened towards the periphery, as stated by Tscherning. Grossman also states that a slight posterior lenticonus is developed during accommodation. It is difficult to understand

how a relaxation of the zonula could result in these conical changes in the surfaces of the lens, unless, as suggested by Priestly Smith, there is an arrangement of lens fibres causing resistance to be progressively greater towards the periphery of the lens.

In referring to some of the very valuable drugs now in general use in ophthalmic surgery, and which were for the most part unknown twenty-five years ago, cocaine is deserving of first mention. In the year 1884 Carl Koller, at the Heidelberg Congress, read his memorable paper on Cocaine, and demonstrated its marvellous power of producing local anæsthesia. This wonderful discovery marked an eventful epoch in the history of ophthalmic surgery. Cocaine has facilitated enormously the performance of most eye operations, and has saved patients an incalculable amount of inconvenience and pain. Several other excellent local anæsthetics have since been discovered, and of these I shall mention only eucaine and holocaine. The latter, a synthetic product, is probably the best local anæsthetic known. It is an antiseptic; produces a deeper anæsthesia of the iris than cocaine: does not affect either the pupil or accommodation; causes little, if any, dryness of the corneal epithelium; and does not produce any toxic effects unless introduced sub-conjunctivally. Euphthalmine is probably the best mydriatic to use to dilate the pupil for the purposes of ophthalmoscopic examinations, as it does not affect the accommodation, or only to a slight degree, and the mydriasis, which is very marked, passes off in a few hours. Homatropine, whose action is also transitory, is not only a mydriatic but also an excellent cycloplegic, and, combined with cocaine, is very generally used to paralyse the accommodation before estimating errors of refraction in adults. The cycloplegia, however, is not so complete as that produced by atropine, which is the best drug to use to paralyse the accommodation of children.

The stypic properties of suprarenal extract have proved a great advantage in many of the minor operations on the eye, and it is said to be useful also in the treatment of certain eye diseases. Of late attention has been directed to the use of the organic silver compounds in the treatment of conjunctival affections, especially in the treatment of purulent and gonorrhœal ophthalmia, and also in the treatment of chronic dacryocystitis. The advantages which are claimed for them over the inorganic salts are that they are less irritating, less coagulating, less caustic, and more penetrating; and at the same time, they are strongly bactericidal in their action. The organic preparations of copper and mercury are also highly spoken of. Another drug to which I shall briefly refer is fluorescein, which beautifully maps out with its green and yellow staining corneal abrasions and ulcers. The Rontgen rays are of great value in ophthalmic surgery: they not only enable us to diagnose the presence of foreign bodies in eyes, but also materially aid in locating their positions. These rays have also been successfully employed in the treatment of lupus and epithelioma, and cases of trachoma have been recorded as cured by them.

I now propose to briefly review some of the more recent operations in ophthalmic surgery.

H. Knapp, of New York, in 1892, drew attention to the marked benefit derived by expressing trachoma by means of a roller forceps. In many cases the improvement following expression far exceeds that obtained by any other mode of treatment. Previous to the introduction of the roller forceps, granulations were often expressed by crushing the lids between the thumb nails, but the expression of the trachomatous substances by this means was very imperfect, and the results were often unsatisfactory.

The simple extraction of senile cataract has been revived, and warmly advocated by many distinguished ophthalmic surgeons. The most serious

drawback to this operation is the tendency of the iris to prolapse ; but, when this does not take place, the results are most satisfactory, and the cosmetic effect all that could be desired. Formerly it was the practice not to operate on senile cataracts until they were mature ; now, when a patient becomes incapacitated by senile cataracts, one does not hesitate to extract, washing out, if necessary, the soft matter, as was first suggested by Professor Horner of Zurich, in 1881, and more prominently brought under notice some years later by the late Dr. McKeown of Belfast ; or, to artificially ripen the immature cataract by Forster's method, and extract a few weeks later. Recently the extraction of senile cataract in its capsule has been advocated by Major Smith, who has recorded excellent results.

Berlin, in 1868, reported a number of cases of extirpation of the lachrymal sac for dacryocystitis. This paper attracted little attention at the time. but the operation has since been revived, and, for the treatment of chronic cases—especially among the laboring classes—is a distinct advance. The epiphora which follows is surprisingly little, but should it be such as to cause any inconvenience, it can be readily cured by the removal of portion of the lachrymal gland.

About twenty years ago Mules described the operation which bears his name. After a few years it fell into disuse, owing to the large proportion of glass balls, which failed to be retained. Later, with the advance of our knowledge of asepsis and with improved technique, it again came into favor ; and my experience is that it seldom ever fails. The great improvement in the appearance and in the mobility of the artificial eye, after a Mules' operation, is such that in my opinion in all suitable cases it is to be preferred to enucleation. A few cases of sympathetic ophthalmia following it have been recorded, and the probability is that there is a slightly greater risk of sympathetic after this operation than after enucleation. I have never had the misfortune to see such a case, although I have seen several following enucleation. In order to minimise the risk of sympathetic, and still retain the advantages of Mules' operation, the glass ball has been enclosed in Tenon's capsule, but the results have not been satisfactory. As a substitute for the glass balls, balls of gold, silver, and paraffin have been used.

Regarding strabismus the operative treatment is less in vogue than formerly, and advancement continues to grow in favor as compared with tenotomy.

The galvano cautery is now much more generally used in the treatment of conical cornea and corneal ulceration, especially in that variety which is associated with hypopyon ; and its use in the treatment of corneal ulcers is much facilitated by first staining them with fluorescin. Sub-conjunctival injections are also highly spoken of by some authorities as treatment for hypopyon ulcers : they are also employed in the treatment of other diseases of the eye. Donders was the first to call attention to their action.

The removal of the superior cervical sympathetic ganglion for the relief of glaucoma was first performed by Jonnesco, in 1897. Since then it has been tried in a large number of cases, but the results cannot be said to be encouraging. In chronic and hæmorrhagic glaucoma it has been followed by temporary good results, but in cases of acute and sub-acute glaucoma the benefits derived from this operation, if any, are not to be compared with those obtained by iridectomy. Sympathectomy is suitable only in cases of hæmorrhagic glaucoma, or when a patient will not submit to an operation on the eye. The ciliary ganglion has also been removed for glaucoma, but the results were disappointing. This ganglion can easily be got at, by means of an operation devised in 1887 by Professor Kronlein, of Zurich, for the removal

of orbital tumors and foreign bodies within the orbit without sacrificing the eye. Briefly, it consists of temporarily turning back the outer wall of the orbit, and thus exposing the orbital contents. Several brilliant surgical triumphs have been achieved by means of this operation : notably one, in which a tumor was removed from the optic nerve by Dr. Antill Pockley a few years ago, the patient retaining excellent vision.

Of the many useful instruments invented within my own knowledge, it is my intention to mention only a few, namely, Morton's ophthalmoscope, Priestly Smith's perimeter, Haab's electro-magnet, Prince's forceps, Knapp's roller forceps and cutting needles, Lang's twins, Ayers' chalazion foreeps, Maddox glass rods, the Geneva lens measure, and Javal Schiotz's ophthalmometer.

Thanks to Listerism and the subsequent development of asepsis and antisepsis, the results in ophthalmic surgery have materially improved. Panophthalmitis and severe iridocyclitis, leading to phthisis bulbi following operations, are practically things of the past. In the early days of my practice I can well remember the great dread one had of these complications ; but now one approaehes operations on the eye with greatest confidence, feeling assured of a happy result in almost every case. Following the advent of Listerism came the study of the bacteria, several of whieh—the Kock-Weeks bacillus, the diphtheria bacillus, the tubercle bacillus, the xerosis bacillus, the diplo-bacillus of Morax-Axenfeld, the gonococcus, the pneumococeus, the streptococcus, the staphylococcus, and a few others, are found assoeiated with eye diseases. Much yet remains to be discovered before we can elaim to have a definite knowledge of the exact relation of many of these miero-organisms to the different diseases of the eye, and the fact that the infections are so often mixed adds to the difficulty. According to the present state of our knowledge it would appear that the Kock-Weeks bacillus is the most common cause of acute contagious conjunctivitis, the diplo-bacillus of Morax-Axenfeld of one of the commonest varieties of chronic catarrhal conjunctivitis, the diphtheria and tubercle bacilli are the causes of diphtheritic and tubercular affections, the gonococcus of gonorrhœal ophthalmia, and the pneumococcus of hypopyon keratitis and pneumococcic conjunctivitis. The xerosis bacillus—which, morphologically, so closely resembles the diphtheria bacillus—is frequently found in the normal conjunctiva and in far greater numbers in xerosis, of which, however, it is not considered to be the cause. Streptococci, on the other hand, are said not to be found in the normal conjunctiva : they occur in two varieties of conjunctivitis—a simple catarrhal and a pseudomembranous form, while staphylococci are found in the normal conjunctival sac, but probably do not produee any inflammatory eondition.

Before eoncluding I desire to pay homage to the memory of those heroes of our science who, during the past quarter of a century, have passed away, leaving behind them works whieh will for ever be revered. The names of Graefe, Donders, Arlt, Helmholtz, Bowman, Critchett, Desmarres, Sichel, Jager, and of the other great men who have laid the foundation of the proud edifice of modern ophthalmology, will always remain intimately associated with its history ; and we, as oeulists, must deeply feel how great is the debt of gratitude we owe them.

Let me thank you for the patient hearing you have accorded me. I am well aware of the many imperfections of my retrospect, and of its manifold omissions, but I feel I would not be justified in oecupying more of your valuable time. And in conclusion, gentlemen, permit me to express the hope that reviewing the past may stimulate us to greater efforts in the future.

A DISCUSSION ON THE PRESENT DAY TREATMENT OF TRACHOMA.

Introduced by F. Antill Pockley, M.B., C.M., M.R.C.S.

When invited by the President of the Section to open a discussion on some ophthalmic subject, it was only after some hesitation that I accepted, because of the importance of the task, the difficulty in selecting a subject, and my inability to do it justice. Relieved, however, by the reflection that discussion implies difference—or, at least, modification or amplification—and that the introducer may be regarded merely as one who tosses the ball into the arena, leaving it to others to keep it moving, or as the advance guard that opens an engagement and then retires under cover of the heavy artillery, I accepted the honor—though still with misgivings.

The initial difficulty of the choice of a subject was met by the consideration that it should be one of wide-spread interest, of which all the members should have experience and could speak with benefit to us all. From this point of view it appeared to me that our present position in regard to the treatment of trachoma would be a useful matter for discussion.

I do not propose to go over the wide field of the treatment of trachoma generally, but merely to elicit the opinions of those present as to the comparative merits of the older and the more recent methods. Neither is it my intention to touch on the treatment of the numerous complications and sequelæ of the disease. All this would furnish enough controversial material for a week's discussion.

It seems fitting to take, as an arbitrary dividing line, the period of the second Intercolonial Congress held in Melbourne, 1889, when, in excellent papers by two ophthalmic surgeons of ripe experience—Dr. Aubrey Bowen and Dr. R. B. Duncan—we had given us what we may take as the recognised best methods of treatment at that time. I would inquire whether our present methods give speedier, better, or more lasting results than those then in vogue, or whether we have to confess that trachoma still remains as formidable and intractable a disease as ever? At that time treatment was mostly by local application (medicinal), and practically all the external remedial agents in the Pharmacopœia had been tried (and some not in it—such as inoculation with gonorrhœal pus), with the result that, as topical applications, silver nitrate and copper sulphate—the remedies of the ancients—remained our best friends. Perchloride of mercury, which was just coming into fashion, was going to do great things. Has it justified those expectations, or have we found that its usefulness, and even its safety, can be challenged? Our friend, Dr. Symons, was, I think, then using ethylate of soda: does he still pin his faith to it, or has he found anything better? In the sixteen years that have elapsed we have had new medicinal agents galore put into our hands, such as the organic salts of silver (protargol, argentamine, and argyrol), adrenalin and hemicine, cuprocitrol, ichthyol, various naphthol compounds, and many others (not forgetting "mud eels' blood"). The mechanical and surgical methods of treatment have come more into favor: and we have also to learn the results of our experiences with X-ray, high frequency, and radium treatment.

Let us consider first some of these medicinal agents, and see whether in them we have anything better or more reliable than the time-honored (if somewhat barbarous) silver nitrate and bluestone. The most satisfactory of these newer reagents appear to be the organic silver salts, and especially argyrol. To what extent have they realised expectations? They certainly are useful in some conjunctival and lachrymal sac troubles; and in trachoma also they

often do good. In all but the more severe cases of trachoma, and in cases that improve slowly or hang fire under other treatment and call for a change, they may be used with advantage; and in such their painlessness renders them specially suitable for children and nervous or sensitive patients. Though I have used them for years, however, personally I have not the same confidence in them in severe cases of trachoma that I have in nitrate of silver, bluestone, or lapis divinis. I find also that patients themselves feel that the latter do them more good, and prefer them in spite of their painfulness. Neither (as personal experience has shown me) are the organic salts free from the risk of staining the conjunctiva (though I have seen it stated that they are safe in this respect): for this reason it is unwise to give patients these preparations to take away for their own use. Another drug that for a time had a certain (though I think over-rated) reputation is ichthyol, or ichthargin. It never appeared to me to do much good, though it has a place for a change, especially when silver salts are beginning to stain. Adrenalin and its congeners greatly impress the patients by their effect of bleaching (though only temporarily) the conjunctiva, and this seems to be their only virtue.

Though the continued use of weak lotions of perchloride or biniodide of mercury appears to be of no particular advantage, and may be injurious to the cornea, an occasional swabbing with 1 to 500 perchloride in glycerine, with or without previous mild grattage, is, I think, undoubtedly useful.

Some years ago I tried the effect of nascent iodine. I had a clamp made, like a Snellen's lid clamp, but with both blades of solid metal. To the surface of the inner blade was stitched a piece of lint soaked in a saturated solution of iodide of potassium. A current of two or three milliamperes was passed through it for a few minutes. It has a remarkable immediate effect on the sago grain granulations, making them white and friable, so that they either loosen and fall out or disintegrate and disappear; it also causes a fair amount of general inflammatory reaction. I consider it a valuable method of dealing with cases in which the sago-like bodies are numerous. One objection to it is that it is painful. Recently Roselli has used nascent iodine, which he obtains by the simultaneous application of potassium iodide and peroxide of hydrogen.

Cuprocitrol, in the form of a 5 per cent. ointment, appears to be another useful introduction, and may be used without apparent injury by the patient himself; but, as I have been using it for a few months only, I cannot speak with any certainty.

Mechanical and Surgical Treatment.—Though the methods employed to-day are, with some modifications, as old as the time of Hippocrates, it is only quite recently that they have been revived. The modern plan of expression by means of the forceps of Knapp, Grady, or Kuhnt, is, in suitable cases and in experienced hands, one of the most valuable methods we have of shortening the treatment; though I do not see the immediate cures recorded by Knapp. Possibly this may be because of a difference in the Australian type of the disease. This method is chiefly indicated where there are large, soft, or "ripe" sago bodies, without much previous inflammation or thickening, or when the inflammation and discharge have subsided. In most other conditions it does harm. Simple as the operation is, however, it must be done with judgment and caution, for one frequently sees disastrous results, in the form of extensive laceration and deep scarring of the conjunctiva and partial symblepharon caused by a too vigorous use of the forceps by inexperienced operators. In some cases a light subsequent brushing with a tooth brush and boracic acid adds to the efficacy of this method. Where the tougher papillary condition prevails, light scraping with a sharp spoon expedites matters. Scarification by numerous longitudinal incisions, often

recommended, appears to me to be fraught with danger and apt to be followed by too much scarring; though a few incisions when the tissues are greatly congested and engorged gives relief by local depletion.

Excision of the Retrotarsal Fold.—This is an operation believed in and practised by Dr. T. K. Hamilton when most of us regarded it with suspicion, but latterly it has come into more general favor, and is, I consider, a most useful operation in suitable cases where the retrotarsal fold is exuberant. It should be carefully done, avoiding excising the sub-conjunctival tissue or the ocular conjunctiva. A good plan is to raise the conjunctiva on a thread passed longitudinally along the fold, or on two threads passed through either end of the fold. There will be contraction afterwards, of course, but not more than (if as much as) results eventually from a long and tedious treatment by local applications. Suitable cases for this operation, however, do not often come my way. It appears to be a feature of the Australian type of the disease (as was first suggested to my notice by Dr. Gordon McLeod) that the retrotarsal fold is not so prominently affected as it appears to be in the European form. Of the combined excisions of the fornix and tarsus, at present popular in Germany, I have no experience, and hope to hear something about it from some of those present who may have practised it or seen the results.

X-Ray Treatment.—As was to be expected, trachoma has been submitted to this, and many cures have been recorded by Mayou and Sydney Stephenson, in England, and by others on the Continent and in America. I have used it in both hospital and private work, choosing for purposes of comparison cases where both eyes were equally affected, and using the X-rays on one, and bluestone, silver nitrate, &c., on the other eye. I cannot say that I ever saw it do more good in active trachoma than can be more readily effected by more ordinary and simpler methods: in some cases it made matters worse. It has, however, appeared to be useful in reducing the hyperplastic and cicatricial condition found in old chronic cases.

High Frequency Currents.—My experience of these is too slight and too recent to be of any value. Of treatment by radium I have had no experience whatever, nor have I employed the method of " massage."

To sum up briefly, of local applications I should say silver nitrate and copper sulphate still hold pride of place, such as it is; the former to be used in moist discharging conditions, and the latter in the hard, dry, less vascular cases. Save in exceptional cases the nitrate should not be employed stronger than 10 grains to the ounce, and the copper sulphate should be applied lightly and evenly, and well into the fornices; the object being to excite a moderate reactionary inflammation, and not to cauterise the conjunctiva. Argyrol is a very efficient substitute, in the milder cases especially. As variants, swabbing with 1 to 500 perchloride tannin ointment or glycerine of tannin, or ichthyol 10 per cent. solution, or cuprocitrol ointment, may be used with benefit; nascent iodine is also distinctly useful. For the patient's own use a simple lotion of boracic, or borax, or hazelene, or weak sulphate of zinc, is as good as anything else.

Of mechanical and surgical methods, expression, in suitable cases, and when properly performed, is most useful; and so is excision of the retrotarsal fold in suitable cases. It is in the surgical treatment of trachoma that the most beneficial advance has been made in recent years.

X-Ray treatment, high frequency current, and radium emanations are only on trial, and appear of very minor value.

Whatever routine treatment we adopt, its usefulness in long-continued cases is increased by temporary intermission, at suitable times and for suitable periods, of all local treatment except simple lotions and ointments; and of more importance than we are apt to keep in mind is the improvement, as far

as possible, of the patient's general health, and the establishment, as far as practicable, of better hygienic conditions—especially good food, fresh air, cleanliness of surroundings, and change of air or residence.

Finally, we have all seen cases of what has evidently been severe trachoma that have got well after lasting for years (as far as such cases ever do get well—that is to say, with a pale, smooth, glistening conjunctiva, delicate cicatricial lines, and normal shape of lids), when the patient has had absolutely no treatment whatever. The lesson of this is not to be unnecessarily energetic in our methods, but to hasten slowly, using the mildest effective methods when the patient has the requisite time and patience and confidence in his attendant. Unfortunately few patients can afford the time, or expense, or loss of employment entailed by long treatment; and there is, moreover, the risk that the longer the disease exists the greater the liability of its being communicated to others. There is no disease in the whole round of ophthalmic work that demands so much patience, experience, and resourcefulness on the part of the surgeon in its treatment as does trachoma, with its numerous complications and sequelæ, and we have, doubtless, all prayed for the discovery of the trachoma coccus and its antitoxin. But that day is not yet.

Dr. Barrett (Victoria) said that it seemed to him that the discussion had assumed too limited a form. The weakness of the position was that we did not clearly understand the cause, or, in general, the pathology of trachoma, and we were simply discussing the effect of a number of remedies applied to the surface of the conjunctiva in a purely empirical way. When one recollected that the lid was enormously thickened and infiltrated, and that the immediate effect of a remedy applied to the conjunctiva could only be for the moment to the depth of a fraction of a millimetre, the crudity of the proceeding appeared tolerably obvious. So far as the merits of the various local applications were concerned, he must confess that nothing gave him more uniform results than the free expression with Grady's forceps, followed by the alternate use of sulphate of copper and 1 per cent. cyanide of mercury solution, with occasional alternations to preparations of silver. With Dr. Orr, he had been for some time engaged in collecting evidence as to the conditions under which trachoma seemed to take rise in Victoria, and, though the work was far from complete, they had found that on the whole the trachoma came from the dry, hot, and dusty districts of the State. Very few cases came from the fertile western district. He understood that it had recently been discovered that the anthropoid apes could be inoculated with syphilis. Possibly they could be inoculated with trachoma also, and, if that were once done, the pathology of the disease would probably become well known.

Dr. Lockhart Gibson (Brisbane) said he expressed in suitable cases, but not in all. He used a 5 per cent. solution of nitrate of silver, carefully protecting the cornea, and neutralising it almost at once. Corrosive sublimate, of the strength of 1 to 5,000, when poured very freely into the conjunctival sac, was found by him to be a very useful mode of treatment. It was impossible to be sure of a cure until a case had been well for, say, six months.

Dr. Hogg (Tasmania) remarked that cases of trachoma in patients who had never lived out of Tasmania were extremely rare, most of the cases which he saw in Tasmanians having been acquired in the other colonies; and he had no doubt that hygienic and climatic conditions had a distinct bearing upon the disease. Expression was of great value. He thought too much had been made of the dangers of incision. In some bad cases he practised incision with grattage and brossage with mercuric chloride or cyanide,

1 to 500 : this shortened the period of treatment, even if it were not sufficient in itself. He had no doubt that relapses occurred, and that patients should be advised to report themselves from time to time.

Dr. Symons (Adelaide), in reply to a question, said he still had faith in ethylate of soda for cases where the granulations were fleshy and vascular, and especially if pain were a marked symptom. Of late ethylate of soda had been set aside in place of more recent drugs—as protargol, cuprol, &c. Dr. Pockley has not mentioned acetate of lead in the treatment of trachoma. He had recently treated a case throughout the whole course of the disease, the duration of which was less than six months ; a result which he considered good, though he could not compare it with the results of cure in three months obtained by Dr. Barrett.

Dr. Gault, Dr. Kent Hughes, and Dr. Webster also spoke.

Dr. Antill Pockley, in reply, said that one noteworthy point brought out by the discussion was the existence of very different opinions as to the seriousness of trachoma ; and it came as a surprise to him to hear ophthalmic surgeons say they regarded trachoma with complacency, and could nearly always guarantee a cure in about three months. His experience of eighteen years, as ophthalmic surgeon to the Prince Alfred Hospital, led him to believe that in most cases of trachoma one could never " guarantee " a cure, and in many cases it was impossible for any treatment to cure. Could it be maintained that cases such as this : chronic trachoma of long standing, with infiltration, thickening and drooping of the lids, perhaps contraction, entropium, and pannus could be guaranteed a cure in three months ? He could say that cases had been coming to the Prince Alfred Hospital on and off for many years, and, in the intervals, having a turn of treatment at other hospitals, and still were not cured. The explanation of the difference in opinion probably lay in the fact that some surgeons' experience was gained chiefly from private patients, who generally came early, and who could generally be cured in a short time ; whereas other surgeons, with a majority of hospital cases at a more advanced stage, and with various complications, were not so optimistic, but found that the treatment often ran on for many months, or even for years, and even in the end were not " cured."

KERATITIS DENGUE AND POST DENGUE.

By J. Lockhart Gibson, M.D., Edin., M.R.C.S., Eng., of Brisbane.

Although my experience of these cases has not been large, they appear to form a very definite double group, and to have been unrecorded before. Their diagnosis appears to me important for prognostic reasons. Treatment in the cases of keratitis dengue appears to be unproductive of direct benefit, and in the post-dengue cases it resolves itself into an effort to palliate symptoms. Keratitis dengue is one of the most serious affections which can attack an eye, and I am unaware of any other condition which makes one feel more impotent. Considering the very large percentage (probably 75 per cent.) of the people who in Brisbane were attacked by dengue, the cases of keratitis dengue were extremely rare. Three cases came under my notice, all of them in patients suffering from prolonged or relapsing attacks of the fever. I have heard of no others. Doubtless the post-dengue cases would have been as serious had the febrile attacks continued in them.

I am indebted to the post-dengue cases for being in a position to offer an explanation of the etiology of both varieties. The post-dengue cases, of which

I saw five, are affected with patches of keratitis neuroparalytica. The dengue cases also appear to start as keratitis neuroparalytica ; but to this is added rapid infective ulceration of the affected cornea

A case of keratitis dengue.—Male patient, aged 42, referred to me by Dr. Jackson. Attack of dengue began on a Monday night. On Wednesday pain commenced in the right eye. On Thursday night pain in right eye was unbearable. When Dr. Jackson saw him on Friday morning, pain and photophobia were so intense that morphia was injected subcutaneously.

I saw him two hours later. Photophobia was still great. Very little injection of the conjunctiva. In the lower outer quadrant of the cornea there was a 3mm. to 4mm. patch denuded of its epithelium, suggesting that this had first been raised as a bleb, and then thrown off, leaving a sharply-defined shallow ulcer, with a transparent floor except in its very centre, where a small pin's head of deep opacity existed.

Being my first case, I did not regard the outlook as very grave. The pupil was easily dilated, and a disinfecting lotion of quinine ordered. Pain subsided quickly, and, probably because the peripheral neuritis had resulted, after an initial stage of hyperæsthesia, in insensibility of the cornea. In thirty hours the change for the worse was very remarkable : the affected patch had enlarged to include one-half of the cornea—it was covered with white macerated necrotic débris, and its margins were dense white and raised. The swollen margin apparently included the eipthelium and immediately subjacent interstitial substance. Temperature was 102°. It was quite evident that the eye would be lost unless radical treatment could stop the apparently infective ulceration. It was explained that the cautery point might do this, but that the outlook was not hopeful. The whole area was cauterised with a dull red electro-cautery point. For twenty-four hours ulceration appeared to be at a standstill, making one regret that the cautery had not been applied when he was first seen. During the succeeding twenty-four hours it spread again rapidly to involve the whole cornea. The whole surface was then again touched with a dull red cautery point, but without beneficial result. The conjunctiva became inflamed, but apparently as a secondary affection. There was very little mucopurulent discharge after the first day, and at the end of a week some chemosis of the conjunctiva.

The cornea, when the eye settled down, was represented by little, if any, more than its posterior limiting membrane with the iris adherent to it behind. On the second day the eye was so hopelessly going to the bad that I do not regret having applied the cautery point as a forlorn hope ; but I am more than glad that I did not do so on the first day, as its use then would have left a doubt in my own mind as to whether the cautery was not a contributing factor in the advance. If my belief that the cases are primarily neuroparalytic be correct, cauterising the ulceration, even though itself of an infective nature, could not stay the progress.

Cases of keratitis post dengue are also very important for prognostic reasons. Treatment is merely palliative. It was these cases which cleared up to me the pathology of the cases of keratitis dengue. I found the area of cornea affected to be, after the first few hours of hyperæsthesia, anæsthetic, and that the anæsthesia persisted in one case for two months; that after three months sensibility had begun to return ; after four months it was quite restored.

The eye becomes affected after the actual attack of dengue has subsided, generally during the first week of convalescence. In its most typical form a triangular area of the cornea is affected, with its base at the periphery and its apex at or beyond the centre of the cornea. At its onset there is acute pain with photophobia and lachrymation. The corneal epithelium is possibly

raised as a bleb at the extreme outset, but this I have not seen ; it, or a superficial layer of it, appears then to slough, and to leave a very shallow ulcer, in whose floor appear a number of small pin's head opacities, apparently situated in the more superficial layers of the interstitial substance of the cornea. In the worst case the triangular area involved a fourth of the cornea ; the least severe had a very narrow triangle. In the worst case there was swelling and inflammation at the corneo-scleral margin, not unlike that seen in cases of acute inherited interstitial keratitis. In this case photophobia and lachrymation continued until the corneo-scleral margin cleared up, and until the peri-corneal injection had disappeared. In the three severe cases repeated and continued use of a mydriatic was necessary, to control the irritability of the iris. I found in all cases that the use of bisulphate of quinine was followed fairly quickly by some healing of the superficial ulcer, but that the interstitial opacities persisted for months, and that the epithelial surface of the area—especially that part in the centre of the cornea—became thickened, and glistening white in appearance. This glistening white central portion, composed chiefly, I think, of thickened epithelium, gives place to a much slighter opacity, and results in comparatively little disfigurement. How completely it will clear up I cannot say. It is still (September, 1905) decreasing in my first case, which began five months ago, and sees now 6-24. After two months sensibility begins to return in the severe cases, but very slowly, and after three months the sensibility of the affected area is still markedly less than that of the surrounding cornea.

The post-dengue cases are evidently due to peripheral neuritis of the portion of the corneal nerve plexus corresponding to the affected area, with resulting keratitis neuroparalytica. The initial stage of extreme hyperæsthesia, the subsequent absence of sensibility, the very prolonged character of the affection, and the fact that peripheral neuritis in other parts has been not uncommonly observed during convalescence from dengue all point to this diagnosis.

It is important to recognise the true nature of these cases, if only because the patient can be informed that a severe attack will persist for months, and may permanently impair sight.

I have given strychnine internally, in the hope that it may have some influence in hastening the recovery of the paralysed nerve filaments. It certainly raises the general health, and possibly helps recovery of the nerves.

In the cases of keratitis during the actual attack or attacks of dengue, we have, I believe, added to keratitis neuroparalytic infective ulceration of the cornea. This may, of course, be due to infection from without ; but as, in the patients observed by me, it occurred in all those who at the time were still suffering from the fever, and in none of those whose temperature had fallen to and remained at normal, there seems reason for concluding that infective ulceration was due to the organism of dengue itself acting upon the portion of cornea unprotected by a nervous supply.

A CAUSE OF PSEUDO-MYOPIA.

By J. Lockhart Gibson, M.D., Edin., M.R.C.S., Eng.

I have recognised for many years that nasal obstruction, especially post-nasal obstruction, emphasizes the presence of errors of refraction. My attention was first called to this by finding that children requiring the correction of, say, one and a half dioptres of hypermetropia prior to the removal of adenoids were able after their removal to relinquish their glasses and make their extra

accommodative effort without discomfort. A second observation in this connection is one which most of us must have made, even if the explanation given it has not coincided with mine, viz., that children with comparatively small amounts (say 1½D.) of hypermetropia often hold their books very close to their eyes when reading, much closer than emmetropic children—although they, as a rule, see well in the distance. I have found that such children have nasal obstruction, and that it is more important to remove this than to correct their error of refraction; *i.e.*, after removal of adenoids the extra accommodative effort for near work is often made without discomfort, and is not made, as formerly, in excess of requirement. My explanation of the mechanism by which this excessive accommodation is brought about will not add another to the already too swollen list of reflex nasal neuroses. I have considered it a result of the distinct and constant increased effort required in respiring through an obstructed nose—of the nature, in fact, of an overflow stimulus from the respiratory centre.

The cases which have induced this paper are cases which exhibit to test-type examination all the characteristics of myopia, and sometimes those of myopic astigmatism. They are generally children, and are not common. I have observed the condition in one adult. I will quote, shortly, two cases; one because he was my first case, and because his pseudo-myopia was astigmatic, and the other because he turned out to be actually hypermetropic. Each was cured of discomfort and of short sight by the removal of adenoids. In each case the cure has been permanent.

E. B., aged 11 years. First seen, August 20th, 1900. History—Eyes troubling for a year. They feel weak, and he cannot look at anything for any length of time. Has a nervous spasmodic habit of moving the skin of his head, and his ears, and of frowning. Reads 6-12 and with an effort some of 6-9. To direct ophthalmoscopic examination, no myopia. With correction—Right eye — 1 D. cyl., axis vert. Left eye — 0·50 D. sph. — 0·25 D. cyl., axis vert.—reads 6-5 and eyes are equal, and fan in each case is equal.

A considerable mass of adenoid hypertrophy on naso-pharyngeal roof, hiding upper one-third of septum. Drum membranes are indrawn and opaque. Breathes heavily at night. Hearing natural. Myopic astigmatism diagnosed to be spasmodic. Advised removal of adenoids and no correction of refraction. Removed adenoids on September 19th. Facial spasm steadily decreased and myopic astigmatism gradually decreased, until in a month from operation his facial spasm had disappeared, and he saw 6-5 without any correction. There was a doubtful 0·25 of hypermetropic astigmatism in his right eye, axis horizontal, and no astigmatism in his left eye. He has remained well and quite free from eye trouble. No mydriatic was employed.

S. N., aged 14 years. Sent to me on January 15th, 1904, by Dr. Lilian Cooper. For months has held book very close to his eyes and has seen badly in the distance. No headaches. V. Right eye, 6-36; left eye, 6-24. To direct ophthalmoscopic examination, neither fundus requires a — glass.

With correction—Right eye — 1 D., left eye — 0·75. Sees 6-5. Has adenoids on naso-pharyngeal roof, hiding base of septum. Had not been noticed to breathe with difficulty.

January 19th.—Removed a considerable quantity of adenoid hypertrophy from his naso-pharynx.

January 22nd.—Reads 6-9 with both and each.

January 26th.—Reads 6-9 and some of 6-6.

February 6th.—Reads 6-5.

February 27th.—Reads 6 5, and can be made to do so also with + 1·25 D. sph.

He has kept perfectly well and has no trouble with his eyes or head. No mydriatic was employed.

Dilatation of the pupil has been observed (De Schweinitz) to occur in vomiting, and in forced respiration; and if forced respiration can induce

dilatation of the pupil, there can be no reason why it should not be associated with spasm of accommodation. The overflow stimulus explanation appears to receive some support from the cases we get of facial spasm due to accommodative effort. Twitching of the lids associated with accommodative effort is of course common, but I have seen some cases of very marked facial spasm on the side corresponding to a more hypermetropic eye, the spasm being at times clonic, and at other times continuing for quite half a minute or more, and then relaxing to be soon repeated. When this spasm has not been of very long standing, correcting glasses cure it. This spasm has evidently been an overflow stimulus from the accommodation centre, first to the orbicularis centre and after to the muscles of expression. It has been a result of prolonged overactivity of the accommodation centre, and is apt in adults not to show itself until some presbyopia has supervened. It is not very infrequent in children.

Galassi and Harold Gifford describe what they call an orbicularis pupil reaction occurring when a forcible effort is made to close the lids. The contraction of the pupil which then occurs they ascribe to an overflow stimulus from the nucleus of the orbicularis fibres of the facial to the pupil contracting centre We would all admit that the muscles of expression are implicated in forced respiration, and there is no reason why, in some people, a further overflow should not implicate the accommodation centre. I have myself described marked facial spasm in children simulating chorea, and cured by the removal of adenoids—which induced it, either by interference with free nasal breathing, or by acting as a foreign body in the nose: the facial spasmodic contractions being then part of an unconscious effort to move or dislodge them.

DISCUSSION.

Dr. Pockley said *(à propos* of remarks by Dr. Kirkland)—“ We cannot let that pass.” Surely it was the general experience of ophthalmic surgeons that it was often the slighter degrees of ametropia, especially of astigmatism, that caused headache, which was promptly banished by appropriate correction. He practically never found it necessary to vary the prescription of another occulist within a few weeks or months, although opticians' prescriptions almost invariably needed alteration; and as for changing his own prescription three times in as many months, he could say positively that he had never done such a thing—except, perhaps, in the case of a neurotic patient, for whom one often found that no correction gave permanent satisfaction, though a very trifling alteration of the glasses gave comfort for a time.

PARAPHYMOSIS PALPEBRÆ.

Dr. Lockhart Gibson showed a photograph of a case of paraphymosis palpebræ, occurring in both eyes at the onset of acute trachoma. The case came under his care at the Children's Hospital after it had existed for some weeks. Eversion of the upper lid was complete, its lid margin forming a tight constricting band above. The swelling of the constricted lid was extreme. Incisions and a canthoplasty gave some relief to tension. The complete eversion lasted for months. Had it not occurred at the onset of attack there can be little doubt but that both eyes would have been lost, as no cornea could have withstood such pressure from so rough a conjunctival surface. After treatment of the trachoma for some months, it became possible to replace the lids. The cornea remained clear throughout. The boy ultimately became cured as to his lids, and regained normal vision. He was in hospital for more than twelve months.

INCREASED INTRA-CRANIAL PRESSURE AS A CAUSE OF OPTIC NEURITIS.

By F. Wallace Mackenzie, M.B., C.M., Edin.

Increased intra-cranial pressure has long been recognised as a cause of optic neuritis.

Schmidt-Rimpler expressed the opinion that the neuritis is due to the cerebro-spinal fluid being forced into the space between the optic nerve and its sheath.

Parinaud extended this theory by suggesting that the accumulation of fluid under pressure in the ventricles interferes with the lymphatic circulation, and that the neuritis is caused by the œdema thus created.

Sourdille suggested that the actual mode of production is somewhat different. He pointed out that the chiasma is actually, anatomically, almost portion of the wall of the third ventricle, and is closely incorporated with it. An œdema, therefore, of the walls of the ventricle would directly act on the chiasma, and so down the optic nerves to the discs. The swollen nerve would thus be compressed in the optic canal.

Probably none of these theories is correct. Reasoning by analogy, it is difficult to understand how an inflammatory process can be set up, either by interference with the lymphatic circulation or by compression of the nerve alone. I am inclined to think that the increased tension leads to a change in the fluid secreted, and that a toxin is formed which excites the neuritis.

The following cases illustrate the belief that optic neuritis is one of the consequcnces of increased intra-cranial pressure.

J. N., aged 23 years, was in good health till October, 1903. At that time he had a severe blow on the back of his head, from a fall off a horse. In December following his sight began to fail. He consulted his doctor in the country, and was told that he had optic neuritis. He went to Wellington, where he was treated by two doctors. Then he went to Dunedin: there he was treated by electricity for some weeks. He got a little better at first, but soon relapsed, and gradually got worse. He then went home, and had no treatment for 10 months. All this time the sight gradually failed, and he suffered daily from headaches, which made him stupid.

I saw J. N. first on December 31st, 1904. At this time he walked with his head erect and his eyes wide open. The pupils were partly dilated, equal, and reacted to light. Vision was only perception of light; hearing was considerably impaired; and he complained of almost continuous headaches. There was atrophy of both optic nerves, with some blurriness of the outer edge of each disc.

I took him into hospital, and had his head shaved. There was a depression in the skull, extending from half an inch below and behind the left parietal eminence nearly to the lambda.

On January 6th, 1905, assisted by Dr. Faulke and Dr. Gilmer, I trephined the skull over the outer part of the depression, taking out an inch disc of bone; the disc had a well-marked ridge on the inside, and the ridge could be felt to be continued downwards. The dura mater showed no pulsation. I cut the ridge off the disc, and filed the surface smooth; then cut a groove out of the skull, in the position of the rest of the ridge, with nibbling forceps. The dura mater was opened by a curved incision, when a large amount of cerebro-spinal fluid escaped. There was normal pulsation of the brain, and it was of the usual color and consistence. The dura mater was sutured with catgut, the disc of bone was returned to its place, and the scalp wound sutured. A small hank of celluloid string was put into the lower end of the wound for

drainage : this was removed next day. At the end of six days the wound was dressed again, and the scalp sutures removed. The patient was kept in bed for a month. He had no more headaches after the operation, and there has been a gradual improvement in his sight. From having perception of light only, he can now—six months after the operation—distinguish large objects, such as a watch or a pocket-knife, and he can walk about the streets alone.

Brudenel Carter pointed out, some years ago, that optic neuritis could be cured by opening the optic nerve sheath ; and Gowers has observed that, even in cases of incurable cerebral tumor, optic neuritis cleared up after trephining the skull and opening the dura mater.

At the Intercolonial Medical Congress, held in Sydney in 1892, I reported a case of "Atrophy of the Optic Nerves with Dropping of Watery Fluid from the Left Nostril." This patient's trouble began with headaches and progressive blindness, due to optic neuritis, together with attacks of unconsciousness, which were probably due to gradually increasing intra-cranial tension. At the end of a year a dropping of cerebro-spinal fluid began from the left nostril, and, coincidently with this, the headaches and attacks of unconsciousness ceased. The dropping from this man's nose continued for four years, and he remained in good health. Then he consulted an American gentleman, who had established a reputation in nasal affections. A few brisk applications of the galvano-cautery cured the dropping from the nose, and the patient came under me again, as the headaches and fits had recurred. After this the patient went from bad to worse, and ultimately died in a general convulsive attack.

I have seen more than one case of optic neuritis clear up after a radical operation for mastoid suppuration. In these cases I believe that the inflammation in the neighborhood of the cerebral membranes sets up increased secretion of fluid within the dura mater. In such a case, in which I explored the middle fossa of the skull for an abscess which was not there, I noticed that there was no pulsation of the brain until the dura mater was opened. The patient had marked optic neuritis, which subsided and left quite useful sight.

It is my opinion that, even where no local lesion can be found to account for the optic neuritis, it is good treatment to open the dura mater and relieve the increased tension inside that membrane.

ACCOMMODATION IN APHAKIC EYES.

By R. H. Jones, M.B., B.S.

If the members of this Section of the Congress were asked to give an account of the mechanism of "Accommodation," I suppose most would quote Helmholtz's views, and others perhaps would give Tscherning's. Both theories require that the lens should be present, and in correct anatomical relation to the surrounding parts. It is more than likely no one would speak of the possibility of accommodation taking place in an eye from which the lens had been removed. That such a possibility exists is shown by the cases I am going to report.

In January, 1902, I was consulted by a young man 17 years of age, who wished to know if it were possible to prescribe glasses for him that would give better vision than those he was wearing. Seven years previously he had been operated on for cataracts. He was under observation for four months, and had a second operation on the left eye.

Examination:—Right Eye—There is a small scar in the cornea, up and out, such as might be caused by a discission needle. Anterior chamber is deep; pupil active, oval, with longer axis down and out. Left Eye—Anterior chamber deep; pupil active, oval, with longer axis down and out.

In both pupils are the remains of lens capsule, but in each there is a clear opening.

R.V. c̄ +7.D Sph.
+2.D Cyl. axis 15° = 5-9.
L.V. c̄ +11.D Sph.
+ 0.75D Cyl. axis 75° = 5-5 2 letters.

My patient expressed great pleasure at the result, and said he could see everything, even at close range, quite distinctly.

Testing him, I found with this correction he could read J4 from 15 to 60 centimetres with the right eye, and J4 from 12 to 65 centimetres with the left. (His vision was still further improved with +3.D. Sph. added to this correction; he could then read J1 comfortably.)

This power to read small type after the loss of the lens was quite new to me; and I regret that I did not find out how the accommodation was affected by mydriatics and cycloplegics. I have not seen any account of this condition in the text of Ophthalmic Surgery, but I have a recollection of seeing its occurrence mentioned in one of the journals. I spoke to Dr. Symons when he was last in Sydney about this case, and he told me then that he had seen similar cases. Since then he has kindly sent me brief notes of another patient who has preserved the power of accommodation after the loss of the lenses. Dr. Symons' patient was a woman 32 years 8 months of age. In each eye there was congenital cataract, and vision was less than 6-60. The operation for the removal of the lenses was needling.

For the right eye—+5D Sph. for distance and +9D for reading, and for the left eye +5D Sph.
+1.25D Cyl., axis 160° for distance, and +9D Sph.
+1.25D Cyl., axis 160°
for reading were ordered. With these vision was 6-6 and J1. Afterwards, however, the patient stated that she could read better with her distance glasses than with those prescribed for reading, or any intermediate strength. She wrote, under date August 14th, 1905, that she "cannot see near work better with reading pair than distant pair." This patient would seem to have preserved greater power of accommodation than mine.

Case 3. Senile Cataract—Extraction with Iridectomy.—In September, 1904, Mrs. B., aged 58 years, consulted me. Five years before she had been told by an ophthalmic surgeon that she had in the right eye a dark cataract.

Examination.—Right eye, mature cataract. Left eye no fundus reflex. Lens becoming opaque.

On November 9th I did an iridectomy and extracted the right lens. Recovery normal.

On December 20th.—
R.V. +10.D / +1.50D Cyl. 30° = 5.5/18. c̄ +14.D / +1.50D Cyl. 30° = J2 slowly
The distance glasses ordered in No. 2 neutral tint.

January 19th, 1905—
R.V. +10.D / +1.50D, Cyl. 30° = 5.5/12 (3 or 4 letters) +14.D / +1.50D, Cyl. 30° = J1.
Ordered +13.D
+1.50D. Cyl. 30° for reading

August 24th—
R.V. +10.D / +1.50D 30° = 5.5/6 nearly all, and with these glasses reads J4 fluently from 45 up to 17c.m. and J2 slowly at 23c.m.

The common features of the first two cases are that they lost their lenses early in life, and that the irides were not mutilated. Any attempt by me to explain how accommodation was brought about would only be of the nature of guesswork ; but I report the cases because of their comparative rarity, and in the hope that some members may be able to throw light on them.

DISCUSSION.

Dr. Lockhart Gibson mentioned the case of a very hale and accomplished lady, now 72 years of age, for whom he had removed two cataracts—one in 1898 the other in 1901. Her corrected vision in each eye was and is 6-12. With the glasses, which enable her to see 6-12, and which she wears constantly, she can read J1 at 12in.. and very comfortably at 15in. A secondary membrane had required needling in each eye, but the opening in it was not very small in either. She has correction for near work, but often uses her distant ones for it. For intermediate distances she is very comfortable with her distant glasses.

EXOPHTHALMOS IN RELATION TO DISEASE OF THE ADJACENT BONY CAVITIES.

By F. Wallace Mackenzie, M.B., C.M., Edin.

Exophthalmos, apart from exophthalmic goitre and pulsating exophthalmos, is a symptom which is not at all commonly met with ; even in eye hospitals, where a large number of patients are seen daily, cases are quite uncommon. On account of this comparative rarity the actual cause of the exophthalmos in a given case is often difficult to determine, and as the cases which occur are generally those in which one is called on to give a prognosis, and to treat, the matter of finding out the cause is one of great importance.

My present purpose is to confine my remarks to the experience I have gained in studying the few cases which I have myself had to deal with, and to indicate the lines upon which I have classified them, for the purpose of diagnosis.

The bony cavities adjacent to the eye are the orbit, the frontal and sphenoidal sinuses, the ethmoid cells and the antrum of Highmore, and the cranial cavity.

I have seen one case in which the exophthalmos was due to a sarcoma which invaded the orbit from the base of the brain. The patient, who was 33 years of age, came as an out-patient at the Wellington Hospital. He complained of blindness in the left eye, with severe headaches. The left eye was slightly more prominent than the right. On exploring the orbit with a hollow needle, a bony mass was discovered deeply seated to the outer part. The external palpetral angle was extended outwards by an incision, and a square piece of bone removed with saw and chisel from the outer angle of the orbit: this enabled the bony mass to be palpated with the finger. It was smooth, rounded, and quite hard, and it was evidently a bulging outwards of the wall of the orbit. The bone was replaced and the soft parts sutured. The man remained in hospital for a month, getting gradually worse. He was then sent home to the Wairarapa, where he died. I secured a piece of the growth, which proved to be a small spindle-celled sarcoma. While in Wellington Hospital this man was treated with iodide of potassium in large doses and mercury.

The next case I saw was a married woman aged 35, who had borne several children. The right eye had gradually become more prominent than the left—there was no other symptom, except a degree of discomfort on turning the eye upwards—the protrusion was straight forward and the lids could cover the globe. There was good movement in every direction. The patient was treated first with iron and arsenic, as she was anæmic ; but there was no improvement locally. Then she was ordered iodide of potassium in 20 grain doses, with 1-50th grain of perchloride of mercury. The discomfort on moving the eye upwards disappeared in a few days, and at the end of a month the bulging of the eye was decidedly less. At this time 3-grain doses of quinine hydrochlorate were given three times daily, in addition to the iodide and mercury. Improvement went on slowly, and I lost sight of the patient after three months. She reported a year after coming under treatment, and the position of the eye was normal. There was probably a gumma in the orbit.

The following year a boy of 14 years, and a married woman of 52, came into hospital in the same week, both with well-marked exophthalmos. I exhibit their photographs (figures 1 and 2). The boy, a quarter-caste Maori,

FIGURE 1.

had his left eye displaced downwards fully half an inch below its fellow. All movements were good, sight was normal, and he felt no discomfort. The upper and inner bony wall of the orbit was prominent, and could be dimpled in on pressure. This was evidently the cause of the proptosis. The boy's father said the condition began shortly after an attack of measles, and had gradually got more marked during the past six years. At first he had had a considerable discharge of pus from his nose, but this had disappeared for some years.

I first removed the anterior end of the left middle turbinated body with cutting forceps and snare, because it was swollen and rough; but it only contained a large bony air cell. Then, under chloroform, I opened the frontal sinus. It contained a thick, sticky, yellowish, transparent fluid; when this was washed out it appeared fairly healthy. I passed a curved probe into the nostril and drew a silk thread through the infundibulum, and then drew a rubber drain into the canal, leaving one end out at the forehead, the other at the nostril. This remained for a fortnight, and then it slipped through itself, and the wound was allowed to close. The eye did not go back into its proper place. The boy is now a man of 24 years, and the eye is still down his cheek, but gives no trouble, and he has binocular vision.

The woman (figure number 2) had the left eye bulging prominently between the lids, but not displaced downwards; vision was good and the movements quite free, the lids closed over the eye with an effort. She stayed in hospital for two months, and got gradually worse. She was treated with

FIGURE 2.

iodide of potassium and mercury, together with iron and arsenic. She suffered very much from headaches and vomiting latterly, and ultimately went home and died with symptoms of brain tumor. This woman had no nasal trouble, nor any evidence of involvement of the accessory sinuses. She probably had a sarcoma of the orbit, which spread to the interior of the skull.

Orbital cellulitis is, in my experience, the most frequent cause of the variety of exophthalmos under consideration. All of my cases—of which I have records of seven—were associated with a suppurative process in one of the adjacent cavities. One of these cases followed directly on the extraction of an upper molar on the same side. The patient was a delicate lad of 21 years of age. He had suffered from, and was still affected with,

diseased bone in the left shoulder joint. Two months before I saw him—October, 1903—he had a right upper molar tooth extracted for toothache. The right eye became bulged and painful, and remained so till I saw him. At that time he complained of a dull pain in the bone above the eye, and the discharge of a thick, clear, jelly-like material from the right nostril. On examination the bridge of the nose was somewhat broadened, and the right ala collapsed on inspiration. The breath had a bad smell, and the patient looked ill. The inferior turbinated body was swollen; pus came from under the anterior part of the middle turbinate and trickled down over the septum from the roof of the nose, which was smeared with pus. In the mouth the right second upper molar was missing; the wisdom tooth on the same side was half erupted and loose. A probe passed from the socket of the second molar into the antrum of Highmore disclosed the fact that a considerable part of the alveolar margin was loose. An incision through the gum allowed the partly erupted wisdom tooth to be removed, along with much loose dead bone, leaving a large opening into the antrum. The eye still retained its position of proptosis downwards and outwards, so I excised the anterior end of the middle turbinate. The cells in it contained a glairy mucopus. Far back in the nose, and attached under the middle turbinate, was a large polypus, blocking the nostril. I removed it with a snare. It had probably been much more than two months growing, and may have been a result of the same disease which set up the acute necrosis in the jaw. Next day the bleeding from the nose had ceased; a probe passed through the alveolus showed that there was bare and brittle bone under the orbit. Pus was discharging freely from the nose, both from the middle turbinate and the roof, and the eye had returned almost to its normal position.

The lad was in very poor health, so, having given an outlet for the pus in two of the diseased cavities, I allowed him to go to his home in the country. Six months afterwards his family doctor reported that the patient had improved considerably, but that he had lately had an attack of orbital cellulitis which caused marked exophthalmos and required an incision under the lower lid, where he discovered that the orbital margin was necrosed. I have not seen the case since, but I suspect there had been an attack of acute necrosis of the superior maxilla when the tooth was extracted, and that the patient has a dangerous course to pass through yet. I think that, in addition to the trouble in the superior maxilla and ethmoid bone, there is pus in the frontal sinus, and that very radical treatment will be required if an attempt is to be made to cure the disease. In the maxilla I expect sequestra will gradually separate and require assistance to get away, while the ethmoid cells and frontal sinus—having suitable outlets for the pus—may not demand active interference until the patient's health is better.

Another instructive case of orbital cellulitis was a boy of 14 years of age. He came first in April, 1898, complaining of pain and swelling of the right side of his face from a blow with another boy's head at football ten days before. He had lost a night's sleep from the pain. The skin over the lower margin of the orbit was greenish colored from the blow, but it was also swollen. The upper lid was swollen hard and partly closed—the eyeball bulged forward. There was a boggy feeling above the inner part of the upper eyelid, and both orbital ridges were intensely painful on pressure. Temperature 101° F.

I advised incising the tissues over the bones in these places to relieve tension, and the same afternoon operated under an anæsthetic. The periosteum over the lower orbital ridge was healthy, but an incision half an inch long was made into it to be sure. The skin wound here was sutured and covered with collodion and cotton wool. An incision down to the bone was then made from the supra-orbital notch towards the inner canthus. Here

the periosteum was separated by pus over a considerable area of the roof of the orbit, and a probe readily passed backwards for fully half an inch. A strip of gauze was put into this wound and a dressing applied. The boy spent a bad night: he was very sick—probably from the chloroform—and pain in the eyebrow was bad enough to keep him awake all night. Next morning his temperature was 101° F. The wound was dressed: a good deal of pus was on the dressing, and putting a strip of gauze into the wound caused great pain. I decided to explore the frontal sinus, and did so the same afternoon under chloroform anæsthesia. I gouged a hole through the roof of the orbit internal to the supra-orbital foramen and just under the arch. The bone is thin here, and the small mastoid gouge went through very easily. The sinus was evidently full of pus under some pressure, for it squirted out when the gouge went through. The cavity was washed out with boiled salt water, and a dressing applied with a gauze tape in the wound, but nothing in the sinus. The boy had a good night and no more pain. The wound discharged for six weeks and gradually closed up. No bone came away, and the frontal sinus seems to have suffered no inconvenience, but is apparently quite healthy again. The proptosis only cleared up gradually, taking about a month.

Of the next case I have attached a photograph. At the time the photograph was taken he was a married man of 33 years with three children. (Figure 3.) The condition began at 4 years of age, after an attack of measles.

FIGURE 3.

His father told me it got gradually more marked for about a year, and then remained stationary. It has caused no trouble since. He has binocular vision, and all movements are normal. He has + 2 D astigmatism, ax. v., in each eye, and—with correcting glasses—his sight is normal. There is a

soft elastic swelling occupying the inner angle of the orbit. Its centre is a little above the centre of the pupil, and it is evidently what pushed the eye out of its position. The man consulted me about pain in the eyes, which glasses relieved, and was quite satisfied with the position of his eye. I am of the opinion that it is just as well left alone as long as it remains quiet. I think there is no doubt it is a case of ethmoidal mucocele.

In classifying the extrinsic conditions which I have observed to lead to protrusion of the eyeball, I have excluded pulsating exophthalmos, as I consider it to be a subject of sufficient importance to demand separate consideration, and it is not a condition likely to be mistaken for any of the other causes. I have divided the cases into seven classes :—

1. In tumors pushing the eye forward the expression is very striking: the eye has a peering, intent, or frightened expression, which gives one a generally correct impression of the cause of trouble at first sight.

2. In exophthalmos due to a chronic thickening of the periosteum, in addition to the forward position of the eye—which is not very marked—I have observed a thickening of the eyebrow, with a smoothness of the skin of the forehead and temple on that side.

3. In acute periostitis of the orbit, which I believe always involves the subjacent bone also, the patient looks ill and is in great pain. The eye will have a fixed appearance, and the upper eyelid will be thickened and brawny if the pus has invaded the cellular tissue of the orbit. I have not met with acute periostitis apart from the suppurative form involving the subjacent bone.

4. In acute orbital cellulitis the lids are swollen and thickened. The skin is red over the lids. The upper lid partly covers the eye, and cannot be raised. The eye is bulging and fixed.

5. In ethmoidal mucocele the eye is pushed forwards and to the outer side ; there is a smooth rounded swelling just above the inner canthus, and the expression of the patient is often rather sheepish. There is a marked difference in the distance of the two pupils from the middle line.

6. In distension of the orbital wall of the frontal sinus there is a fulness in the upper inner corner of the orbit, and the eye is displaced downwards and outwards.

7. In an acute case of suppuration in the left frontal sinus, which proved fatal from an abscess in the third left frontal convolution, there were swelling and redness of the upper eyelid, closure of the eye, and swelling above the eyebrow.

PLUMBIC NEURITIS.

By J. Lockhart Gibson, M.D., Edin., M.R.C.S.

When asked by our President to fill a gap by opening a discussion on a subject of my own choice, I could agree to do so only if permitted to adapt an already promised paper on Lead Neuritis. I hope by summarising the present position of our knowledge of the action of lead on the optic and ocular nerves, and by going into some detail upon my own observations on the numerous cases in children which have come under my notice, members may be induced to give the Section the benefit of their own views on this interesting and important variety of toxic neuritis.

Since discovering the source of the lead which poisons Queensland children, and its method of ingestion, I have had no opportunity of bringing

the subject before a meeting of oculists. I am, therefore, anxious to do so now, and have hopes that my matter may be food for discussion. If my own observations bulk largely, I trust to be forgiven on account of the very unusual opportunities I have been fortunate enough to have.

I am anxious to establish certain peculiarities regarding ocular neuritis due to lead which may be helpful in leading to an at least provisional diagnosis of lead-poisoning, in cases whose symptoms would justify not only the thought of almost any form of toxic neuritis, but which would even more forcibly suggest a diagnosis of basal meningitis, or of cerebral tumor. The whole question of toxic neuritis would have been too large for this paper. Still it is possible that some members, whose opportunities of observing other varieties of toxic neuritis have been greater than their opportunities of observing the variety due to lead, may give us their experiences.

Regarding the frequency of lead neuritis in general ophthalmic practice—of ninety cases gathered from literature by De Schweinitz, sixty-five were males and twenty-two females: in three the sex was unstated. The age was unstated in eighteen, but ranged from 12 years to 79 years in the remaining seventy-two. He concludes that it is a comparatively rare condition, even when other symptoms of lead-poisoning are present. In 1897 I recorded the fact that, between 1891 and that date, twenty-four cases had been admitted to the Brisbane Hospital for Sick Children; of these sixteen were girls, and eight were boys: the youngest was aged 4 years, the eldest 8 years. Seven of these cases became quite blind, one or two others recovered to count fingers at 2 yards, the others appeared to regain natural sight. From 1897 to June, 1905, thirty cases have been admitted; making in all fifty-four cases of ocular neuritis due to lead. This number is exclusive of cases seen in private practice.

The total number of cases of plumbism admitted to the Brisbane Hospital for Sick Children since 1891 reaches the respectable total of 200, *i.e.*, including cases of wrist and ankle drop with the fifty-four ocular cases.

Amongst forty-eight lead workers examined by Packard, twenty-four had at one time or other symptoms of lead-poisoning; but no case of amblyopia directly traceable to the metal could be discovered. Among 15,000 case records in the General Ophthalmic Hospital practice of De Schweinitz for five years, only three undoubted cases of lead neuritis presented themselves. Uhthoff found, amongst 130 cases of toxic amblyopia, only one case of lead amblyopia. The conclusion, therefore, has been that only a small proportion of the toxic amblyopias are attributable to lead. This conclusion appears to require reconsideration, at least in climates where conditions making children liable to lead-poisoning obtain.

The Queensland cases have demonstrated the peculiar fact that only very rarely is a child suffering from the wrist and foot drop forms of lead palsy affected at the same time with ocular neuritis, and *vice versa*. I have, indeed, observed only two or three cases. But several of my ocular cases have had attacks of wrist and foot drop antecedent to or subsequent to their ocular attacks.

Lead neuritis may be either acute or chronic. Acute cases, Swanzy says, appear to be often associated with acute cerebral disturbances—due probably to the effect of the lead poison on the brain. These effects include convulsions, delirium, and coma. In those cases in which acute cerebral disturbance occurs, Gowers says that optic neuritis is often observed, with considerable swelling of the discs, and accompanied by hæmorrhages. Even some such severe cases of optic neuritis have been observed, Swanzy says, to recover complete vision, while others, of course, have resulted in atrophy of the nerve. To quote Swanzy:—“Inasmuch as the symptoms of bad cases of lead encephalopathy usually consist of severe headache, vomiting, and convulsions, the spasm being

in some cases epileptiform, it is evident that, when intense double optic neuritis is added, the diagnosis between this disease and cerebral tumor has to be considered."

Byron Bramwell, whose experience of both lead encephalopathy and of cerebral tumors has been extensive, says—"So closely do the two conditions (tumor and lead encephalopathy) in some cases resemble each other that I never commit myself to a positive diagnosis of intracranial tumor without having previously excluded lead-poisoning."

According to Swanzy—"The aids to the diagnosis will, of course, be the presence or absence of the characteristic blue line on the gums, as also of anæmia, colic, constipation, wrist drop, and lead in the urine, together with the previous history of the patient's occupation."

The greater number of our Queensland ocular neuritis cases has been of the acute variety, and I think that you will agree with me that they are much more likely to be mistaken for basal meningitis than for cerebral tumor; and that the diagnosis, in isolated cases at least, is not, necessarily, made easy by as many of the aids mentioned by Swanzy as we could wish, although, if we take a group of cases, all these aids are available—blue lines in most, anæmia in some, colic in most, constipation in some, wrist drop (in children generally combined with ankle drop) in some (either at the time or previously), and lead in the urine of some. And we shall find that evidence corresponding to the patient's occupation is also now available.

The disease, shortly, as observed amongst Queensland children. is characterised by the sudden onset, with or without preliminary colic, vomiting, and constipation, and with or without severe headache, rigidity of the neck, or retracted head and acute pains at the back of the neck, of paralysis or paresis of one or both external recti, accompanied by optic neuritis or choked disc, and unaccompanied by rise of temperature or by albuminuria: accompanied also by the well-known blue line on the gums, produced by the deposit of the black sulphide of lead; by the finding of small but distinct quantities of lead in the urine; by the fact that the patient has access to painted surfaces. which have been either recently painted, or, more often, whose paint has become powdery and easily detachable—this powder being the very soluble carbonate of lead: accompanied further by the fact that the patient bites his or her nails. The ages of the children affected range chiefly between 2 and 8 years. Only too frequently older children are seen with signs of slight or marked optic atrophy, and with signs and histories supporting a diagnosis of former lead-poisoning.

Acute lead neuritis has received two chief explanations. First it has been ascribed to the mechanical action of lead inducing effusion into the ventricles and subarachnoid spaces, resulting in distension of the sheath of the optic nerve. In favor of this explanation, according to Clifford Allbutt, is the fact that it is invariably met with in cases of lead encephalopathy where there are signs of increased intracranial pressure, the convolutions of the brain being found flattened *post mortem*. The second explanation is that of Leuber and Deutschmann, who have suggested that the lead particles cause direct irritation to the nerve head by being carried there in the vaginal sheath of the optic nerve itself.

In most of the cases observed by me I have favored direct irritation, not only to the vaginal sheath of the optic nerve, but also to the connective tissue framework of the nerve itself. This explanation would accord with the usually accepted one of chronic lead neuritis.

The very severe cases, however, certainly suggest distension of the ventricles, and it is not unlikely that some of the obscure brain cases which in Queensland children have not been attributed to lead have been of this nature.

The ocular cases do not die; but, to temper our gratification at this, is the absence of *post-mortem* evidence of the exact nature of the involvement of the retrobulbar portion of the optic nerve.

The explanation that the optic neuritis is due to a uræmic state of the blood receives little support. It receives none from my cases, as none of the purely ocular cases had albumen in the urine.

The fundus changes in acute lead neuritis have been variously described. Gowers and Byron Bramwell describe swollen papillæ. The peculiar feature of the Queensland cases has been extreme swelling of the discs; five and six dioptres of swelling being quite common, less than three dioptres uncommon. The outline of the papilla is entirely obliterated, the disc is represented by an elevated area, marked by radiate striæ fading off gradually into the surrounding retina. The veins are large and tortuous, the arteries relatively smaller, but not actually reduced in size in the acute s'age. When the swelling is due to non-solid exudation, enlarged capillaries may be seen below its surface; when the exudation is solid and opaque white, as it often is, merely the coarse radiate striæ of the—at parts—hidden vessels are seen.

My measurements of the elevation of the discs have been taken by comparing the refraction of the surface of the disc with that of the retina during the attack, and more especially with the refraction of the retina and disc after all swelling has subsided. Six dioptres means an advancement of the disc to the extent of nearly two millimetres. There are sometimes hæmorrhages, but not very often, and never large ones. Occasionally the neuro-retinitis extends to and includes the macular region.

I have seen only one case when the optic neuritis was practically entirely post-bulbar, with almost no pinchings in the discs until atrophy supervened. In this case paraplegia externa was complete.

A symptom observed in one case is worthy of record. E. M., a peculiarly intelligent girl of 8 years, was admitted in January, 1904, with a typical attack of acute lead neuritis, with lead in the urine and blue lines on the gums. She recovered quickly, and ultimately regained perfect vision. The child had been ill for three weeks, and had squinted for one week. Our house surgeon, Dr. Lucy Gullett, found that her statement that she saw double with one eye was correct. I confirmed this, finding that she saw double with both eyes open and also with the right eye closed, but single when the left eye was closed. For this reason she covered the left eye when she wished to look at anything. The swelling of the discs only amounted to two dioptres. I noted that there appeared to be relatively more exudation in the horizontal meridian of the left fundus than in the vertical. The case, however, was examined a little hurriedly, and a more careful examination left for two days later. The monocular diplopia had, however, then disappeared: it had lasted for several days. I believe the above to have been its explanation, by causing temporary astigmatism.

A peculiarity of the Queensland cases has been the fact that optic neuritis has never been observed unassociated with some paraplegia externa. As a rule one external muscle is paralysed or paresed; often both. In only one case, where only one muscle was paralysed, was it other than an external rectus: in that case it was an internal rectus. In the most severe cases all the external muscles are paralysed, and the eyeballs are quite stationary. The levator I have not seen implicated. As a rule, where ophthalmoplegia externa is complete, there is opthalmoplegia interna also. Such cases have resulted in complete blindness, with post-neuritic atrophy of the discs. The ophthalmoplegia externa has always been recovered from.

The excellent sight possessed by some of the cases, with several dioptres of swelling in the discs, is astonishing. If the cases can be removed from their

surroundings and confined to bed before the swelling has existed for long, and sight has remained good, the outlook is hopeful—although not without reservation. Fortunately the presence of squint draws attention to the child even more perhaps than the complaint of defective sight—unless this last is very defective. If the cases are to be explained by interstitial neuritis of the optic nerve itself, it is not difficult to understand that the optic nerve fibres, which, from their possession of no neuri-lemma or grey sheath, must be more vulnerable to the pressure of an inflamed interstitial tissue than the fibres of any other cranial nerve, should be damaged beyond recovery, although the sixth and other ocular nerves invariably recover from a neuritis which is surely interstitial.

Lead, of course, is a recognised cause of paralysis of the external ocular muscles ; and there are definite records of its choice of an external rectus, of a levator, and of the whole motor oculi. I have found no record, however, except in my own papers, of a group of cases where the optic neuritis due to lead was constantly associated with paralysis of at least one external ocular muscle. I have seen only one case in which the optic neuritis was unilateral, and in that case the only external muscle paralysed was the external rectus of the other eye. The sudden appearance, indeed, of paralysis of an external rectus accompanied by optic neuritis, and unaccompanied by temperature, and sometimes accompanied by no other prominent symptoms, is sufficient in Queensland to justify a provisional diagnosis of lead.

Of the chronic variety of optic neuritis due to lead, there appears to be more record than of acute ; and it is generally accepted as retro-bulbar neuritis, resembling somewhat that produced by other toxic agents, although a central scotoma appears to be less commonly observed when the neuritis is due to lead, and a concentric narrowing of the field is more often spoken of. Appearances in the disc have been described as follows :—" There may be no findings followed by atrophy, doubtless due to retro-bulbar neuritis, or the disc may be pale in its outer half, and the inner half greyish-red and opaque, with ill-defined borders such as is described in alcoholic neuritis ; or a diffusely reddened and swollen papilla, without notable swelling, and sometimes with hæmorrhages, may be found."

The chronic cases which I have observed in children have a history of defective sight at least for months, and where the optic discs are not atrophic they are pale, with somewhat irregular and blurred edges. They present, indeed, a half-way appearance between a normal disc and a disc in a state of post-neuritic atrophy. The acute cases, also, which do not run an entirely favorable course, present, ultimately, appearances which vary according to the amount of damage done : the worst being examples of complete post-neuritic atrophy—the partial recoveries presenting pale discs with somewhat irregular edges. The defective vision in both acute and chronic cases has been more a concentric narrowing of the fields than a loss of central vision ; but the patients have been young for accurate observations in this respect. As a rule their color sense is not relatively more reduced than their light sense. A child with vision reduced to fingers at five or six yards will show no central scotoma for color—in fact will have a color sense most acute at the centre. Some of the apparently chronic cases were probably cases which at the outset were acute : occasionally the history suggests this conclusion, and sometimes tells of more than one successive summer attack. The following is a case in point :—

A girl, E. B., aged 8½ years, came as an out-patient, with the following symptoms and history. Defective sight : gets nervous twitchings at times. Two years ago was in bed for three months during the summer, with abdominal pains and weakness in the arms and legs. Suffered from a repetition of this attack last summer. Marked blue lines on the gums. Partial atrophy of each

optic disc. Evidently the defective sight dated from the second attack. Probably keeping the child in bed prevented greater damage to the sight by preventing further ingestion of the poison.

To recapitulate and extend the proofs I published in 1904 of the source of the lead which poisons Queensland children, and its method of ingestion, would make my paper too long. Further observation has confirmed those proofs. It will be sufficient if I emphasize one or two points. Verandah railings, in southern Queensland, at least, after having been painted for two or three years, are covered not by an oily but by a powdery substance, which easily rubs off and adheres to clothes and hands. This powder adheres specially well to sweat-moistened hands. It is nearly entirely composed of the very soluble carbonate of lead. Of eighty-five cases of lead-poisoning, all varieties, from 1898 to 1903, sixty-nine were admitted during the seven summer months—*i.e.*, during the months in which their hands may be expected to be sweat-moistened—and sixteen only during the five winter months. The poisoning is practically confined to children of an age likely to contaminate their fingers, for these play and hang and run round verandahs and fences, and clutch the railings and painted corners of walls in running past. The fact that more girls than boys are affected may be attributed either to a possibly greater prevalence of the nail-biting habit amongst girls, or to the fact that they are more confined to verandahs and houses than boys of the same age.

It may be accepted, I think, that in warm climates easy access to painted surfaces—especially to such as have been exposed to sun and weather—in conjunction with a habit of biting the nails, may be expected to tend to the ingestion of lead and to consequent lead-poisoning, and that the danger to young children is very considerable.

One difficulty in diagnosing these cases when isolated is the fact already indicated, that every case does not furnish every aid to diagnosis. Sometimes a blue line is absent from the gums, though possibly present in an unaffected member of the same family. Sometimes the sample of urine sent fails to yield lead on testing. In one case this occurred until after the administration of iodide and of pilorcarpin, when lead was found by the analyst. It must be remembered that hospital cases have been removed from opportunities of further ingestion of the poison before any collection of urine for analysis begins. The urine has not always been collected immediately. If the child were left amongst its surroundings the lead would doubtless always be found.

Of four cases under treatment at the same time in 1904—all typical, all presenting the same symptoms, hardly differing in degree, all recovering perfect sight—two failed to give lead in the only sample (a week's urine) sent to be analysed, while the urine of the other two cases yielded lead. One of those whose urine yielded lead had no blue line, and one of those whose urine failed to give lead had no blue line. Three bit their nails; the fourth, aged 2, sucked his fingers. In each case abundant lead was available in the form of powdery paint—except in one case, where the paint was still sticky from its freshness.

To illustrate shortly three degrees of neuritis :—

1. A mild attack. E. B., aged 2 years, sent to me by Dr. Clowes, with history of squinting for five days and of defective sight. Symptoms came on suddenly; otherwise in good health, though a little fretful. Slight tendency to retraction of the head. Paralysis of right external rectis: paresis of left external rectis. Double optic neuritis, with several dioptres of swelling in each disc. Pupils not enlarged, and re-act well to light and accommodation. No blue line in gums. Sucks his fingers. Special opportunities for

powdery paint to adhere to his hands from verandah railings and verandah floor, upon which he spent his days Lead, .32 millegrammes found per litre of urine examined. Result—complete recovery.

2nd. K. R., aged 6½ years, A more severe case. In hospital at present time. History on admission :—Ill two weeks ; began with vomiting, violent abdominal pains, and constipation ; eyesight began to get weak a few days ago ; to-day cannot see at all. Examination :—Very restless, continually crying out as if in pain ; head slightly retracted, and neck held stiffly ; pupils sometimes large, at others small, re-act slightly to a lighted match when large. The only indication that ophthalmoplegia externa is not absolute is a right eye very slightly directed outwards as compared with the left. Six dioptres of swelling in each disc. For the first week her pulse was irregular at times. Four days after admission she was still unable to see a lighted match, and ophthalmoplegia externa was absolute. Twenty-five days after admission only one external rectis remained quite paralysed, the other muscles all acted slightly ; some fairly well. Counted fingers at 1 yard with her left eye ; three weeks later, fingers at 7 yards with left eye ; very little sight with right eye. Now (three months after admission) left eye sees certainly 6-24, probably 6-18 ; right eye cannot count fingers. The swelling in the discs had practically disappeared a month after admission. Discs both present an appearance of partial post-neuritic atrophy. It is possible that this case had a little lead encephalopathy ; she is now very well.

3rd. A. R., aged 5½ years. A still more severe case. Was seen in consultation in 1900. History :—He had then been ill for two or three weeks, and his case had been diagnosed by Dr. Meek as lead neuritis ; with this I concurred. Illness began with pain down the arms, spasmodic, and not always in the same arm—no paralysis ; no temperature ; retraction of head and pains about head and back of neck. Cries out with these pains on movement ; no colic, and eats well. Lead has been found in the urine. Both external recti paralysed. Five to six dioptres of swelling in each disc. Counts fingers at 18 inches ; even this amount of sight was put in my notes with a mark of interrogation. When seen two months later he had in the meantime suffered from wrist-drop, but not from ankle-drop, and he was still unable to dorsi-flex his wrist. Pupils natural. Paralysis of external recti has nearly disappeared. In good health. Each disc atrophic. Four years later he counted fingers at 2 feet with one eye, and at 6 inches with the other.

4th. To illustrate the gradual improvement in sight which may occur. M. S., girl, aged 5 years, an attack of severe lead ocular neuritis in 1899. Five dioptres of swelling in the discs and much neuro-retinitis encroaching on the macular regions. Paralysis of right external rectis, paresis of left external rectis. Recovered, with damage to the retinæ and some post-neuritic atrophy of discs. A year later counted fingers at 6 yards and saw 6-60; two years later right eye saw 6-18 ; left eye 6-24 partly. Now (1905) sees—right eye 6-9, left eye 6-12. Her discs still show marked evidence of previous neuritis, and are pale and irregular. There are marked evidences of old retinitis between the disc and the macula in her left eye. In each fundus the veins about the macular region are somewhat dilated. She has no scotoma for color, and her fields show only slight concentric contraction, central vision being most acute. A sister of this patient suffered from colic and very marked blue lines in her gums, also from twitchings of shoulders and arms. Not uncommonly in families of the lead cases observed by my colleagues in Brisbane have there been histories of infants, just old enough to crawl and hang on to railings, dying from unexplained convulsive seizures. These have long been attributed by Dr. A. J. Turner and myself

to lead ; and, with the explanation of paint as its source, such a diagnosis receives much support. Treatment has consisted in removal from their homes, confinement to bed, and hypodermic injections of pilocarpine daily for six weeks, iodide of potassium after the first few days, in the proportion of five grains thrice daily to a child of 8 years. Sulphates by the mouth. Latterly I have had anterior splints applied to both arms for some weeks, in an endeavor to break the children from the habit of biting their nails.

As prophylactics, zinc white instead of white lead should be used as a basis for paint where the painted surfaces are intended to be of a whitish color, but where a dark color is desired the red oxide of iron should be used.

A prophylactic more easily enforced is correction of the nail-biting habit.

Dr. Pockley considered the Section was greatly indebted to Dr. Gibson for his paper. It was a most valuable contribution, and, as far as he knew, Dr. Gibson's views as to the frequency of lead-poisoning as a cause of blindness in children, and his ingenious and apparently correct explanation of the way in which it was brought about, were quite new and original.

Hearing the paper recalled a case he had had fifteen or sixteen years ago. The patient, a child, had been staying with relatives in Queensland, when she was seized with convulsions from which she quickly recovered, but was found to be totally blind, and unable to stand or hold her head up without support, and her hand-grip was very feeble. The eyes showed total paralysis of all the oculo-motor muscles of both eyes, and also of the sphincter iridis, and probably of the ciliary muscle. There was most intense optic neuritis, and, eventually, atrophy and permanent blindness. The patient is now alive and well, an accomplished musician, but quite blind. Two other men saw the case in consultation. but could not decide on a cause for the trouble. It was thought there might be a growth below the floor of the aqueduct of sylvius, but there were many objections to this, the only proffered explanation. Looking at the case in the light of Dr. Gibson's paper, he had little doubt that it was really a case of acute lead-poisoning.

Dr. Barrett said that Dr. Lockhart Gibson's paper on the causation of optic neuritis by lead-poisoning was extremely interesting, and it would be a necessity in the future to carefully examine the urine for lead in every case of profound optic neuritis. He suggested, however, that the chemical method adopted for the detection of the lead should be scheduled in the publication, so that it might be beyond cavil, since the diagnosis of the causation clearly rested in some cases on this presence of lead in appreciable quantity in the urine.. He congratulated Dr. Gibson on the work done.

Dr. Lockhart Gibson, in replying, furnished the following explanation of the comparative infrequency—or, in places, absence—of lead neuritis in the States of Australia south of Queensland. A large proportion of the dwellings in Queensland are built on piles or stumps, and stand consequently a few feet from the ground. They are all supplied with verandahs, and these with verandah railings—often of wood, sometimes of iron. These railings and the wooden walls of the houses are painted with lead paint. Children, especially those between 2 years and 8 years of age, play mostly on the verandahs. Each time a child touches the railings, or, indeed, the verandah walls, some of the powdery paint adheres to the fingers—especially in summer when these are sweat-moistened. If the child neither bites his nails nor sucks his fingers he escapes, but if he does either he cannot fail to ingest the lead in considerable quantities. I have taken particular notice that in the southern States the houses are not built on piles or stumps, but on the ground ; that they are less often of wood, and therefore less often

painted; and that the verandahs—which are, indeed, less common—are comparatively seldom supplied with verandah railings. In other words, the children have comparatively few opportunities of contaminating their fingers. The summer, also, is shorter in the southern States than in Queensland.

After the discussion it was proposed by Dr. Lockhart Gibson, and seconded by Dr. Kent Hughes, that the following resolution be forwarded to the Congress:—"That the danger of blindness from lead-poisoning amongst young children in Queensland has been recognised; that it is highly probable that painted verandah railings and other painted surfaces, especially those exposed to weather influences, are the source of the lead: that since the paint is carried to the mouths of the children when they bite their nails or suck their fingers, it is important that paint applied to surfaces within the reach of children should contain no lead: that zinc white is suggested as a suitable substitute."

Lead-poisoning in Young Children.

Home Secretary's Office, Queensland, December 13th, 1905.

1. Lead-poisoning in young children, resulting in paralysis, blindness, and in some cases death, is a not uncommon disease in Queensland; 200 cases having been observed in the Brisbane Children's Hospital alone since 1891.

2. It has been ascertained that the source of the lead is the paint of painted railings and walls of houses. It is well known that paint, after being exposed to weather influences, is in a powdery condition, and easily removed by rubbing the surface with the hand, especially a sweat-moistened hand. It has been shown, through specimens supplied to the Government Analyst, that this powder consists of an almost pure carbonate of lead in a soluble form, so that should a child rub off some of it, and afterwards eat with unwashed hands, or otherwise put his fingers into his mouth, he absorbs lead into his system. It has, indeed, been demonstrated not only that the disease is chiefly prevalent in summer, when the hands are likely to be moist with sweat, but that without exception children so poisoned either bite their nails or suck their fingers.

3. It is urgently desired that no lead be used as a basis for paint applied to surfaces within the reach of young children. Zinc, or other paints without lead as a basis, are suitable substitutes.

4. Meantime parents should see to it that children do not bite their nails or eat with unwashed hands.

B. Burnett Ham, Commissioner of Public Health.

Process used in Government Laboratory, Brisbane, for Detection of Lead in Urine.

Government Chemical Laboratory, Brisbane, January, 1906.

At least half a litre of urine is evaporated nearly to dryness in a porcelain evaporating basin with potassium nitrate (20 grams to the litre of urine) over a Bunsen burner, and then to dryness on the water bath. The residue is transferred in small portions at a time to a red-hot porcelain crucible (6cm. diameter), and the whole heated to quiet fusion. The mass must be white when allowed to cool.

On cooling, it is dissolved out with the smallest possible quantity of water, twice its volume, of 1.19 specific gravity, hydrochloric acid added, and evaporated to dryness on the water bath. The mass is soaked in hydrochloric acid, and the heating and soaking in hydrochloric acid kept up till the nitric acid is all driven off.

The mass is then dissolved in boiling water, which is just acid with hydrochloric acid, the solution saturated with sulphuretted hydrogen while hot, and sulphuretted hydrogen kept passing till the solution cools.

The color obtained is compared with that produced by known amounts of lead in the usual way. It is always advisable, where slight indications are obtained, to allow the sulphuretted hydrogen saturated solution to stand over night.

If much color is obtained the lead sulphide can be filtered off, washed, dissolved in nitric acid, and precipitated, and weighed as lead chromate.

It is very important that all re-agents used, including the distilled water, should be tested for lead. Traces of lead are much more common than is generally supposed. I have even found it in new Berlin porcelainware, which had been previously washed with soap and water. Washing once with lead, free acetic acid gave traces of lead. Further washing, however, showed no more lead. All porcelain and glassware is therefore treated with acid before being used in this work, and " blanks " run regularly, to make certain of the absence of adventitious lead.

J. Brownlie Henderson, Queensland Government Analyst.

THE CLOSURE BY SUTURING OF SCLERAL AND CORNEAL WOUNDS, AND OF THOSE MADE BY THE SURGEON IN THE EXTRACTION OF CATARACT, WHEN THERE IS MUCH LOSS OF VITREOUS, GAPING, OR DELAYED UNION.

By James P. Ryan.

In the minds of ophthalmic surgeons a certain amount of prejudice exists against the employment of stitches in the treatment of perforating wounds of the sclera and cornea. I believe that such a procedure was proposed many years ago by Williams, of Boston, as part of the technique in operations for the extraction of cataract, but I do not know that it was ever practised. The prejudice has no doubt arisen from the fear of providing a channel through which pathogenic micro-organisms might gain an entrance to the interior of the eyeball. There is undoubtedly some ground for this apprehension, but I am inclined to think that the danger has been somewhat exaggerated. At all events I have stitched many scleral, and some corneal, wounds during the last few years, and I have not been able to convince myself that in these cases infection has ever resulted from the practice. Indeed, I go somewhat further, and hold that the immediate closure of such wounds does away with a comparatively large surface which would otherwise be open to the chance of infection from the conjunctival sac. In general surgery perfect coaptation of the wound surfaces is a guiding principle, and this principle, so far as it can be applied in ophthalmic surgery, should be a settled rule of action. I need hardly remind you that, in by far the larger number of instances in which infection occurs, the *materies morbi* is lodged within the eye at the moment of the accident, and that cases of after contamination from the conjunctiva are comparatively rare. It must not be understood that I am an advocate for suturing ordinary small rents in the eyeball, or for closing by this means the incisions made in the usual run of cataract operations. In a considerable percentage of sclero-corneal wounds such a procedure is unnecessary; in a few stitching the conjunctiva may be called for; but, in a still smaller number, suturing the sclera or cornea is advisable, and will be followed by satisfactory results.

I shall briefly illustrate the treatment I recommend by quoting a few cases :—

In August, 1888, a boy of 9 years was brought to me from the country with a jagged sclero-corneal wound of the right eye, extending from near the centre of the cornea to beyond the sclero-corneal junction. The accident was caused by a piece of tin two days previously. The eye was soft, the iris torn and protruding, and the pupil opaque. The protruding iris was excised, as much lens substance as possible was coaxed out with a curette, and, while this was being done, some vitreous escaped. As the tendency was to further loss, and as the patient was of a highly nervous and excitable temperament, I deemed it prudent to stitch the wound; and, accordingly, under chloroform, a single fine silk suture, passed through the whole thickness of the cornea, effectually closed it. The suture was removed on the fourth day afterwards; the case progressed favorably, and the boy recovered with 6-24. or useful vision.

A man, aged 40, had his left eye cut open by a piece of broken glass. He was seen by me some hours afterwards. The eye was soft, and his vision was reduced to perceiving hand movements. There was an irregular wound ½ inch long in the lower part of the sclera, from which vitreous protruded: this was cut off, and the lips of the wound brought together by a single silk suture. His recovery was uninterrupted, and eventually he obtained a vision of 6-9.

On the 22nd of January of last year a man, aged 48, was admitted under my care to the Victorian Eye and Ear Hospital, Melbourne. His right eye was blind from an injury received some years previously; and, as it was tender and irritable looking, I excised it. The cornea of the left eye was much damaged, and cloudy in some portions, from former keratitis and lead deposits; the pupil was irregular, the lens opaque, and the vision amounted to counting fingers at 8 inches. It did not appear probable that much could be done under such unfavorable conditions, but he was most anxious that something should be attempted, and he was willing to take the risk of failure. Accordingly a preliminary iridectomy, upwards and outwards, behind the clearest section of the cornea, was done early in April; and, on May 18th, the lens was extracted, the incision being in the corneal tissue, but close to the sclero-corneal junction. At the end of five days there was no appearance of union of the flap, and the vitreous began to protrude. This was slight at first, but, in spite of careful compression with pad and bandage and the exercise of the greatest care in keeping the patient quiet, the protrusion increased in size, and the danger of the expulsion of the eye contents became so imminent that on June 6th, nineteen days after the extraction of the lens, the hernia of the vitreous was reduced, and the wound closed by two fine silk sutures, which were allowed to remain *in situ* for a week, when complete union of the flap had taken place. There was no subsequent trouble, and six weeks later the vision with + 12.D. was 6-36, sufficiently good to enable him to do rough work. The operation was done under cocaine, and with the usual antiseptic precautions, and the difficulties were not so formidable as I had anticipated. I was fully prepared for a large escape of vitreous, but none was lost. The ordinary eye speculum, Clark's, was used, but it was held well away from globe by an assistant. A small full-curved needle carrying the silk thread was passed from without inwards through the whole thickness of the edge of the lower flap, the latter being steadied by a fine fixation forceps. The hernia was gently smoothed and forced back with a spatula, disengaging it wherever it may have formed adhesions, and held thus back by an assistant. The needle was passed from within outwards through the upper margin of the incision, and the ligature tied. The second suture was similarly passed, and the wound was effectually closed. In this case no other course of procedure was left open to one; and I have no hesitation in saying that no other line of treatment could have saved the eye. This case was published at greater length in the *Lancet* of November 5th, 1904.

S. B., a woman of 67 years, in poor general health and exceedingly shaky, was admitted to the hospital with double cataract. That of the left eye was

hyper-mature, and was extracted under cocaine anæsthesia. Some loss of vitreous occurred, and, as she was exceedingly restless and unsteady, I closed the incision with a suture, that was removed four days afterwards. The healing process followed the usual course, and, with correction, she got 6-18 and Jg. 4.

W. H., a man aged 64, double cataract. The right eye was first operated on, with good result. In the left both P.L. and projection were poor. After extraction of the left cataract the lids closed spasmodically, and some vitreous was squeezed out: four days afterwards, and while the dressing was being changed, the same thing occurred. The upper lid was forced between the lips of the wound, and there was another loss of vitreous. A single suture was introduced, and the wound closed. I had small hope of saving the eye. The condition was unsatisfactory for a fortnight, then things began to mend, and when he left the hospital, some weeks later, he was able to count fingers with this eye.

P. L., a man aged 45, with prominent eyes, and hyper-mature cataract of the right one. While operating on this eye there was some escape of vitreous and gaping of the wound, which was closed with a single suture. The subsequent healing was slow; but eventually, with correction, his vision amounted to 6-24.

L. G., a woman of 70, was admitted to the Victorian Eye and Ear Hospital for painful sub-acute glaucoma of the left eye. The vision was nil, but an iridectomy was done with the object of relieving tension and pain. There was some loss of vitreous and gaping of the sclero-corneal incision, which was closed with a suture. The case did well.

A man, aged 78, was admitted to the hospital with recent traumatic cataract of the left eye. Vision was P L. Sedative measures were carried out for ten days, and then, as the tension was increasing, I proceeded to remove the lens. On withdrawing a Graafe knife, after completing the incision, some vitreous escaped; the eye had a shrunken appearance, and the wound gaped. In fear of further loss, this was closed with a single suture, and the healing process was complete when the suture was removed five days later. The eye quieted down in three weeks. He has only P.L., but as the case is recent, and as there is still some blod clot and lens matter in the pupil, some improvement of vision may take place.

Now a few words about the technique and difficulties of the operation. It depends on circumstances, and the fancy of the surgeon, whether a speculum or retractors are used to keep the lids apart. In either case no pressure should be made on the globe. The fixation forceps for holding the edge of the wound should be long-bladed and narrow at the points, the needle or needles small and full-curved, the silk not the finest, and the needle-holder—like the fixation forceps—should be long and fine. It might be better to avoid carrying the needle through the whole thickness of the sclera or cornea, and this may be accomplished by entering the needle from without inwards in the first instance; but, when passing it from within outwards, through the second wound margin, the difficulty of only partially transfixing the flap is much greater, and should, perhaps, not be attempted. Or the surgeon may use two needles on the same thread, and pass both from within outwards. While the suturing is being done an assistant should be on guard, with a broad spatula, to oppose or control any loss of vitreous. To avoid contamination of the suture by its touching the head or clothes of the patient, these ought to be covered with antiseptic gauze and a sterilised towel, or the end of the silk may be suspended and prevented from touching anything by an assistant supporting it with forceps. The judgment of the operator will have to determine how long the stitches should remain *in situ:* the time will vary from twenty-four hours to several days, preference being given to the shorter period. The main difficulty arises in preventing an escape of vitreous while passing the needle through the wound margins. Needless to say, antisepsis, so far as it can be carried out in eye operations, should be strictly enforced; and, during the subsequent treatment, the

eye should be flushed two or three times a day with a solution of cyanide of mercury of the strength of 1 in 1000. I also recommend that, as part of the preparation of the patient, the eyebrows should be shaved and the lashes cut as close as possible to the lid margin, and the latter be thoroughly squeezed, cleansed, and disinfected, as is my custom in iridectomies and cataract operations.

The few cases cited do not, I am aware, afford sufficient evidence to prove that the sclera and cornea can always be stitched with impunity, but at all events they afford some ground for believing that the practice is hardly so dangerous as it has been represented to be.

With regard to the employment of this procedure in cataract cases, I believe, as I stated above, that it has been proposed, but I do not know that it was ever carried out in practice.

CONGENITAL (?) DISPLACEMENT OF LENSES—ERYTHROPSIA.

By Andrew B. Orr, M.D.

J. H., aged 17, son of a German farmer, was sent by Dr. May, of Bundaberg, to the Brisbane General Hospital, where I saw him first and subsequently in my rooms. J. H. stated when first seen that his sight was very bad, and that all objects looked at with the left eye—the right being covered—appeared pinkish red ; such coloration was only noticed since the doctor had put a drop in his left eye. The left pupil was semi-dilated. On examination, both lenses, clear where visible, were found to be displaced outwards and slightly upwards ; their inner free edge did not quite extend to the centre of the pupil. There was no antero-posterior tilt, and they seemed fixed, movements of the head causing no apparent change in their position. Differences in depth of the anterior chamber and partial iridodonesis corresponded with the description given in similar cases. Corneæ clear, fundi n. Tn. Sight of mother, brother aged 14, sister aged 12, stated to be good ; that of father defective, but no glasses worn. Patient went to school when 6 years old, his sight bad then, but got worse since. No history of injury.

Erythropsia is by no means an uncommon complaint of patients whose eyes are aphakic, whether after extraction of cataract with or without iridectomy or otherwise ; less frequently is it noticed by those whose eyes are normal. Dilation of the pupil, though probably helpful in inducing it, is not necessary. It is stated to occur more frequently when the eyes are suddenly exposed to bright light, but occurs also under various other conditions. (*Vide* Graeffe's Archiv. f., Ophthalmologie Bd. 42, s. 207 ; Ueber Erythropsie, von Prof. E. Fuchs & Bd. 43, s. 19 ; and Erythropsie von Prof. Snellen.)

J. H. complained of objects looking pinkish red when seen by either eye, the pupil of one being dilated and the other eye covered. On dilating both pupils with homatropine he stated that, when out of doors, everything looked pink ; in a badly lighted room this was hardly noticeable. I ordered him spectacles O.U., 15 sph., to be worn constantly. This increased the vision of each eye or both together from Fgs. at four metres to 6–12, and enabled him to read Jaeg. 8 easily. Corneal astigmatism, 45. 1.75 : 0 1. Correcting such did not improve his vision. O.U., 18 sph., enabled him to read Jaeg. 2.

Wearing O.U., 15 sph., with pupils dilated, the position of his lenses caused him no annoyance ; when of normal size, however, he stated that brightly illuminated objects, *e.g.*, trees, appeared green, yellow, pink. The flame of a lamp, looked at from a distance in a darkened room through a 40 D lens, looked

white, yellow, and pink, and the same occurred when looking through Bennett's pin points. This was, I suppose, due to the edges of his lenses acting as prisms. Monocular diplopia in either eye was easily produced when a red glass was placed vertically over half the pupil. Occlusion of such portion of the glass of his spectacles as would prevent his seeing through his own lenses restricted his field of vision to a greater extent than seemed advisable ; and it is to be hoped that the prismatic effect will, in time, be noticed less by him.

I decided against tattooing corneæ.

Spectacles for reading were not ordered, for obvious reasons.

CATARACT, FROM A PATIENT'S POINT OF VIEW.

By Dr. Andrew B. Orr, M.D.

Darier (*vide La Semaine Médicale*, June 5th, 1895) is credited with describing a new method which enables the patient himself to determine the smallest opacities in his lens and notice the progress of his cataract. It is only necessary for him to look through a strong concave lens, *e.g.*, – 40 D, at a distant light in a darkened room. Opacities, spots, striæ, &c., cast shadows on his retina and are seen by him.

Further particulars of this method are no doubt given in his article "de la possibilité de voir son propre cristallin. Utilité pratique de la 'phakoscopie' pour la diagnostic des fine opacités cristalliniennes et pour l'étude du developpement de la cataracte" ("Annales d'Oculistique," tome 114, page 198): but this I have not read. Those of you who care to take the trouble of observing their own lenses can easily do so by this method, or that given below. I hope they will discover the formige Linsenstern only, and no opacities.

The second method, a simpler and better one, I learnt from Mr. E. J. B., October, 1871, who consulted me in November, 1899, stating that he had cataract in both eyes and wanted spectacles for reading.

Corneal astigmatism—90 + 1 : 90 + 1. Vision—Longe. O S 90 — 1 + 3 V = 6-12 O D 90 — 1 + 3 V = 6-36; prox. O S 90 — 1 + 7 Jaeg 1 90 — 1 + 7 Jaeg 16. On lenticular opacities, more in O D than O S. He then showed me drawings of his cataracts, stating that he had made them himself—not by Darier's method, but by looking through a minute opening in a sheet of metal, copper or tin, towards a cloudless sky. The opacities which I noticed in his lenses corresponded with the drawings when inverted—sufficient proof of his assertion.

Four months later the vision of the right eye was practically unaltered, that of left considerably diminished, and the lenticular opacities increased. I have had the drawings of his cataracts photographed, and lantern slides made therefrom; the date of the first drawing being 1887, and the last 1905. Particulars of the conditions under which they were made and date are shown on the slides themselves, of which there are sixteen, numbered 1 to 16. A circular tin plate with eight perforations—the largest being No. 1, the smallest No. 8—is used by him when making his drawings, and the figures on the slides show the opening used. These slides and tin plate accompany my paper. I need hardly add that Mr. E. J. B.'s drawings are the work of a skilled draughtsman, and such can hardly be expected from the majority of persons suffering from cataract. That of Mr. John Henry, with immature cataracts, slide No. 1B, and J. H., with clear dislocated lenses, No. 1C, is what one usually obtains.

"A method, not generally known, by which obstacles to distinct vision situate within one's own eyes may be observed" is the title of a paper by Mr. E. J. Bennett, communicated by Henry Tryon and read before the Royal Society of Queensland on October 14th, 1887.

A CASE OF MELANOTIC SARCOMA OF CONJUNCTIVA TREATED BY X-RAYS.

Dr. Pockley read a short note of a case which, though still uncured and under treatment, he thought worth bringing under the notice of the Section, in case other members might think the treatment worth trying.

Mrs. X., aged 40, consulted me three years ago with a growth in the conjunctiva of left eye. It was nearly black, about ¼ inch in diameter, and almost touched the limbus. It was removed, and pronounced a sarcoma by Prof. Welch. Eleven months ago the patient returned with a large growth of same character on the former site encroaching about 3mm. on the cornea. The proposal to enucleate did not meet with the patient's approval, and, purely as an experiment (as the patient was informed), X-ray applications were tried, and have been continued on and off ever since. The first effect noticed (in about two or three weeks) was the splitting up of the pigmented patch into an upper and lower mushroom-like patch, joined by the stems. Soon after the corneal portion disappeared; then the stem of the mushroom went, leaving two separate but smaller areas. The lower of these has now quite disappeared, and the upper has broken up into several smaller areas, each about a pinhead in size, and it looks as if the growth were going to disappear altogether. I was indebted to Dr. Clarence Read, of Chatswood, for the patient, and he has watched with me, with much interest, the changes noted.

A CASE OF MYASTHENIA GRAVIS IN SPECIAL RELATION TO EYE AND THROAT CONDITIONS.

By G. H. Hogg, M.D., Launceston, Tasmania.

Miss ———, aged 27 years, consulted me in December for slight difficulty in swallowing. (For some months she had suffered from repeated attacks of mild tonsillitis, for which I had removed a slightly enlarged tonsil). She stated that she had felt much stronger, and that she no longer suffered from the attacks of tonsillitis, but that for the last week or two she had noticed some difficulty in swallowing fluid.

I found on examination paralysis of the soft palate (fluids, if a large mouthful was taken, occasionally returning through the nose), and also weakness of accommodation, which became more marked in a few days.

The patient was nervous and anæmic, with a quick feeble pulse: the various systems were otherwise normal. She had been subject to business worry and overwork, and had always been of a shy nervous temperament.

With the history of previous sore throat in the past, the possibility of diphtheria as a causal factor was kept in mind, although never accepted; and the case was regarded with suspicion.

Treatment by iron, strychnine, generous diet, &c., was carried out, but in a week the symptoms became more marked; then the patient began to mend, the anæmia and weakness disappeared, and swallowing became almost normal. After a time she went to the seaside, and good accounts of her improvement were forwarded from time to time.

Towards the middle of January unfavorable reports were given; and, on her own responsibility, she stopped the medicine and tried cod liver oil. When I saw her in the beginning of February she stated that she felt weaker,

had had occasional attacks of faintness and shortness of breath, had once or twice some difficulty in moving the little finger, and had noticed some weakness in the legs.

The following condition was then present :—Paralysis of soft palate, slight ptosis of right eyelid, pulse quick and feeble. Next day no ptosis was to be seen. In three or four days it was again marked, and was accompanied by slight paresis of the corrugator supercilii and frontalis, and in the course of a week the right external rectus, orbicularis palpebrarum, risorius, and zygomaticus were affected, and the laryngoscope showed weakness of tension and adduction of the cords. Fluids no longer regurgitated through the nose, but slight difficulty in swallowing, causing occasional attacks of coughing and choking, indicated that pharyngeal and laryngeal paralysis was threatened. Notwithstanding that active treatment had been resumed, strychnine pushed, electricity, massage, &c., carried out, the patient rapidly went downhill.

She was taken to Melbourne and saw several physicians, some of whom evidently regarded the case as one of simple bulbar paralysis. Dr. Maudsley, however, believed it to be the same as I had come to regard it, viz., myasthenia gravis.

On her return she seemed much worse, and the various paralyses were more marked ; attacks of dyspnœa began to supervene, swallowing became impossible ; tube-feeding was attempted, but had to be abandoned in favor of rectal ; speech became more and more difficult to understand. She again rallied, the power of swallowing returned somewhat, and the attacks of dyspnœa became less frequent and less severe ; but she relapsed again, the dysphagic and dyspnoic attacks becoming worse. The patient lost most of the power of moving her tongue for three or four days, and then regained it considerably. For some time she had been having the strychnine administered hypodermically, on Dr. Maudsley's suggestion, but without any apparent benefit. As the dyspnœa became very trying, recourse was had to injections of morphia and inhalations of oxygen, which gave great relief and afforded the patient the euthanasia which was so much desired.

EYE CONDITIONS PRESENT.

1. Paresis of accommodation noticed in the early stage of the disease disappeared in a week or two, returning at a much later stage, and disappearing the last few days before death.

2. Ptosis of right side noticed in February absent next day ; in three days' time much more marked, continuing for a week ; again almost disappearing, to return later and become so severe that the lid had to be raised by the finger, a few days before death becoming much less ; occasional weakness of the levator palpebræ of the left side, never amounting to marked ptosis.

3. Paresis of the right external rectus noticed a little later than the ptosis, disappearing completely in a few days ; reappearing in about a fortnight to a more marked degree, subsequently becoming less, and, towards the end of the illness, being very slight and variable.

4. Paresis of corrugator supercilii noticed towards the end of Ferbuary, disappearing, reappearing, finally becoming constant ; only noticed slightly on the left side.

5. Pupil reaction, vision for form and color, field of vision, and fundus, normal.

Little attention has apparently been given to myasthenia in works on ophthalmology, although Bielschowsky has shown that in one-third of the cases paresis of the external muscles of the eye is the first symptom, ptosis,

varying in degree, being especially common. In fact a rapidly appearing and fleeting paresis of the ocular muscles produced on exertion is almost pathognomonic, and is often marked before any of the other symptoms of myasthenia present themselves.

THROAT.

1. Paralysis of soft palate was the first noticed sign of disease, and it continued more constantly than any other symptom, varying, however, very much in degree and apparently less severe towards the end of the illness.

2. Paralysis of pharyngeal muscles, and probably of epiglottis, varying very much in degree; sometimes food swallowed with comparative ease, at other times impossible to do so. Less marked the last few days of life than the preceding three or four days.

3. Paresis of adductors and tensors of vocal cords noticed in February. Dr. Maudsley thought that he noticed weakness of adduction. I had not observed it before her departure to Melbourne, and on her return to Tasmania throat examination gave the patient too much discomfort to continue it; but I think that, with the very variable conditions present, repeated examinations might have confirmed Dr. Maudsley's observation.

4. Tongue was apparently untouched until late in the disease, then it began to feel "clumsy," as the patient put it, and soon got so bad that it could not be protruded from the mouth; then she regained the use of it to a great extent, and retained it in a lesser amount to the end.

The most striking feature in the case was the continual remission and exacerbation of symptoms with the corresponding varying degree of paralysis; in fact, on one or two occasions, as regards the ptosis, one felt almost inclined to doubt one's observations, for a droop which was there one day was gone the next.

The symptoms were all increased at the menstrual period, which was followed by a temporary amelioration. Any fatigue or exertion also seemed to increase the amount of paralysis, which varied not only from day to day but at different times of the day, being usually worse towards evening.

The various reflexes were never impaired, no R.D. was present, no perfect myasthenic reaction was elicited although the reaction to faradism varied on different occasions, and after the use of the faradic current voluntary movement seemed impaired.

Treatment seemed to have no effect on the condition except during the first stage of the disease, and even then it seems doubtful whether the temporary improvement was not rather a stage in the natural history of the disease.

Tubal feeding—which seemed indicated at a later period, and is, I believe, recommended by some—gave rise to the most alarming symptoms, even when carried out with the greatest care; the passage of the tube setting up attacks of dyspnœa and faintness, which became so alarming as to make one desist from further attempts. Drs. Bramwell and Campbell have, I believe, found the use of the feeding tube to be dangerous.

As to the pathology of the disease much doubt exists. The theory of an auto-intoxication has been advanced by some, dynamic changes in the lower motor neurones have been assumed by others. Buzzard has lately described certain conditions which have been found present in five cases of the disease which he examined *post mortem*. These consisted of "lymphorrhages"—the presence of small foci of cells resembling lymphocytes scattered irregularly between the fibres of muscles, or between the cells of a glandular organ. The term "lymphorrhage" was applied to these collections both on account of the character of the cells of which they are composed and because their general appearance was suggestive of a capillary hæmorrhage, with

the distinction that the cells were not red blood corpuscles. A small empty capillary vessel lined by a single layer of endothelium was sometimes observed in the neighborhood of these lymphorrages. The lymphorrages were found in a smaller or larger number of the muscles of each case, but, in some instances, only after a prolonged search. The ocular muscles were most fruitful in the three cases in which they had been examined. The only other changes in the muscles were presented by the fibres themselves; here and there were seen a few fibres which had become swollen and rounded, some hyaline and granular in appearance, and often showing a tendency to stain a paler blue with eosin or fuchsin. "Lymphorrhages" were also found in the cardiac muscle, thyroid gland, adrenal bodies, and in a posterior spinal ganglion in one case.

The fact that in these five cases similar microscopical changes have been found in the muscles and other organs goes some way towards establishing a definite morbid anatomy for the disease.

NOSE, EAR, AND THROAT DIVISION.

THE INDICATIONS FOR THE RADICAL MASTOID OPERATION—THE METHODS OF ITS PERFORMANCE AND THE VALUE OR OTHERWISE OF EPITHELIAL GRAFTING.

Dr. A. J. Brady (Sydney), President, in the Chair.

The President thanked the Executive Committee of the Australasian Medical Congress for the honor which had been conferred on him in electing him President of this Section. He thought the members would agree with him that the short time at their disposal would be better spent in discussing the important practical work which would be brought before them than in listening to any formal address from the chair.

The subject for discussion to day was " Indications for the Radical Operation on the Mastoid: the Methods of its Performance, and the Value or otherwise of Epithelial Grafting." It was important to be able to judge when the radical operation was necessary. In his opinion, before this operation was undertaken a full trial of the usual treatment for the cure of middle-ear suppuration should be made, and no case should be subjected to operation which could be cured by simpler means. It appeared from the published statistics of operations that some surgeons in London found this procedure more frequently necessary than we do in Australia. Whether this was due to the difference of climate, ear cases doing better in our more genial temperature, or to a difference in surgical opinion and practice, he was not prepared to say. On the other hand, there were some surgeons who had an unreasonable dread of the operation: the bogie which scared them was facial paralysis, following injury to the facial nerve. To a skilled operator this was not a danger sufficient to deter him: those who had not the necessary skill had better let it alone. In his own experience, although he had several times seen a temporary facial paralysis, which had taken from one to two months to fully recover from, he had never had a permanent paralysis follow this operation. The temporary paralysis is easily brought about by even slight contusion of the facial nerve, which may happen if the fallopian canal is destroyed by disease. It is of no importance, as recovery always follows. He asked the

members present to state their experience on this point. The operation was a valuable one, and was the only means of cure in a certain number of cases. The methods of its performance were fairly well fixed. There was a difference of practice in regard to the formation of flaps, and as to the epithelial grafting, or otherwise; on this latter point there was room for useful debate.

Dr. Webster was called upon to open the discussion, followed by Dr. Lockhart Gibson and Dr. Arthur.

Dr. Percy Webster (Melbourne): In the limited time at our disposal it would not become one to give a lengthy account of the aims of the radical mastoid operation, or to indulge in an extensive analysis of the methods of its performance.

I will content myself with a few remarks having a practical bearing, hoping thereby to limit the scope of the discussion to a few interesting and debatable points, recognising that the members of this Section cannot differ materially on the main features of this important subject.

I would at the outset state that by the radical mastoid operation I mean the removal of so much of the temporal bone as will throw the antrum, the tympanum, the attic, and the meatus into one continuous cavity. The operation is a combination of the two operations described by Schwartze and Stacke; and any modification of this combination is not a new operation, but only a change in some detail, and is at once a tribute to the work of these surgeons and an indorsement of their principles.

The chief modification, and one to which I hope each member will address himself, is that of grafting the post-operative cavity, communicated by Ballance to the Medico-Chirugical Society in 1900.

Towards the end of last year C. Heath, of London, communicated to the Otological Society an account of his method of operating, which appeared to be a new operation; but, on critical examination, turns out to be Stacke or Schwartze-Stacke, at any rate, in principle.

As to the indications for operating, the cases which admit of controversy are those of chronic suppuration of the middle ear, with or without much impairment of hearing, and without acute or a recurrence of acute symptoms. Two men of equal ability and experience may very well, and do often, disagree in such cases. The symptoms are not urgent, and the only circumstances which induce the patient to seek advice are the persistence of the discharge, which may or may not be offensive, the trouble entailed in keeping the ear clean, or the defective hearing. I think in the end the patient himself often turns the scale for or against operation, either being sick of the discharge or anxious at all costs to improve his hearing; or, on the other hand, caring nothing for the discharge, or indifferent about his deafness, and entirely unwilling to take the risks of any operation. Where there are recurring attacks of pain, of vertigo, or general malaise, none of which may be attributed by the patient to his ear, our duty is not merely to advise but to urge an operation. Where, however, without these symptoms, the discharge has existed for twelve months, and the patient has been under our personal supervision and treatment for two months, and we are satisfied that the case is standing still or getting worse, we should point out to the patient the risk of general septic infection and of cerebral abscess, and advise an operation; provided always that, before advising the radical operation, the remains of the drum, the ossicles, and part of the margo tympani have been removed. It must have occurred to all of us to have seen a case cured by the lesser operation when we had fully expected to have had to do the greater. I am aware that many circumstances may occur to induce one to change one's practice in individual cases; but I should like to hear from each member what general rule they lay down for their own guidance in these cases.

As to the method of operating, my practice is to follow fairly closely that of Ballance, without, however, proceeding to grafting. He makes a large skin flap, the incision following the line of the hair, and thus fully exposes the mastoid. Heath, on the other hand, cuts close to the auricle; this seems to me unsound surgery unless the disease of the mastoid be very limited, and we cannot tell this when making the skin incision. The larger incision does not leave an unsightly scar—the only objection I can find recorded against it.

I am in the habit of using the mallet and chisel, sharp curettes, and, to a limited extent, the gouge. The sets of Schwartze's chisels I have found too small for one to obtain a firm grip of. A chisel should be at least 5 inches in length, and straight to the cutting edge, whether that be flat or grooved. Working only with a burr is very slow, and when worked with a treadle the burr is apt to be unsteady from the alteration in pace: an electrically worked burr has not this disadvantage. Hand burrs are useful for smoothing down the walls of the cavity, a matter which should always be most carefully attended to. In removing the bridge over the iter it can be cracked with a light blow from a high-pitched or blunt chisel, either above or below, and then broken off with forceps. A Stacke's protector is of service, but the same object can be gained by pushing a small roll of gauze under the iter from the antrum.

I have not used any grafts. Very soon after Mr. Ballance's paper appeared I operated on a lady according to his directions, except that I preserved the whole of the posterior wall of the membraneous meatus in the concho-meatal flap. On the tenth day I was intending to open up the wound again for the purpose of grafting, but my patient—who had suffered exceptionally severely from chloroform sickness—begged me not to operate again. On examining the cavity I found healing progressing so satisfactorily that I acceded to her request. Much to my surprise the whole of the surface was healed over at the end of a fortnight. I attributed this successful and rapid result to the fact that I was able to remove all the diseased bone, and to the extensive covering that the inclusion of the membraneous meatus gave to the denuded surface. My further experience has been the same, and I have not seen the necessity for employing a graft.

It has been advised that the cartilagenous portion of the meatus should be removed instead of being folded back, on account of it containing the ceruminous glands, and thereby favoring accumulations of cerumen. I do not think this a serious trouble. In the case referred to I have had to remove cerumen only once in four years.

I believe that delay in healing will not take place if the whole of the diseased bone is removed, and as much as possible of the meatal lining is preserved for covering the cavity. As aids to healing, the application of alcohol, in the form of drops, and of nitrate of silver to granulations, are obvious details of treatment; and the drops may be begun as early as the third or fourth day. The difficulty of manipulating the graft, should one be employed, would appear to be much reduced by the device suggested by Waggett, who says, "I have varnished the prepared area of the limb with new skin, a proprietary article of the nature of collodion, a fairly good imitation of which can, I believe, be made by dissolving celluloid in acetone."

As soon as the antiseptic dressing is removed from he prepared thigh, a few drops of "new skin" are poured over the part and quickly spread with the finger. In one minute this has dried to a firm varnish. A method of dressing which I believe will come into more general use is that of using a layer of thin perforated rubber, which is made to line the wound cavity and packed with little balls of gauze.

With regard to the opening remarks of our President, it may be that we do not have to perform the radical operation here as often as they do in the old country, on account of our milder climate not favoring such severe cases of aural suppuration ; but one reason leading to the more frequent performance of the radical operation there is the advantage they have over us, especially in hospital work, of a large supply of skilled assistants—a very important circumstance in this operation.

Dr. Lockhart Gibson (Brisbane): The object of such a discussion is, I take it, to bring forward our own beliefs and conclusions, with some of the premises upon which these have been founded ; to indicate the signs and symptoms which would suggest radical interference to us ; to outline why we prefer one method of operation to the others ; and to formulate what we have reason to hope for, as the result of such radical interference.

At the present time aural surgeons appear to be divided into three groups in their attitude to the radical mastoid operation. Members of one group operate only very seldom, members of another group operate on almost every case of otorrhœa which has continued for many weeks, perhaps even without their taking the case thoroughly in hand themselves ; and, between these two, is a group composed of those who advise operation after making a serious and unsuccessful endeavor, prolonged if necessary, to stop the discharge by treatment of and from the nose and naso-pharynx and through the meatus. Members of the first group must often withhold their hands from conferring great benefit ; members of the second group report an enormous number of successful mastoid cases, and suggest to one the thought that a considerable minority of these would have recovered as well without such radical interference. Still, its members belong to the class of practitioner who, by frequent practice, develops a natural into a special expert in operative manipulations, whose patients in consequence run few operative risks, and who may be more easily forgiven for his, at times, unnecessary interference. It is well to remember, however, that all unnecessary operations are bad practice. We shall, I fancy, all agree that every case of persistent otorrhœa which has failed to yield to treatment should be subjected to a radical mastoid operation. Each, probably, will have his own views regarding the length of time during which efforts should be made to stop the discharge, and the methods to be employed. I am a very strong advocate, where radical interference is not urgently needed, for putting the nose and naso-pharynx—the last including the fossæ of Rosenmüller—into as natural a state as possible prior to operation. I favor this in spite of the fact that interference with the nose and naso-pharynx is often so belated as to partake of locking the stable after the horse is out. If, in spite of a healthy nose and naso-pharynx, and in spite of careful attention through the meatus and eustachian tube, we fail to stop the discharge, even although there be no evidence of necrosed bone, a complete mastoid operation should be done. It can hardly be that every case in which suppuration has extended to the mastoid antrum fails to recover with ordinary treatment, but a stubborn otorrhœa without evidence of necrosis most probably owes its continuance to the state of the mastoid antrum and to the attic. It is in the highest degree unlikely that a case with necrosed ossicles, and consequent long standing otorrhœa, would have an antral cavity likely to yield to treatment through the meatus. Still in some cases, after removal of necrosed ossicles, it may be right to persevere with treatment in the hope of avoiding a radical operation. Necrosis of the tympanic walls calls for a radical operation. The existence of headache and of giddiness in a case of chronic otorrhœa calls for operation. I look upon curetting of the middle ear and attic, through the ordinary meatus, as

likely to be in a majority of cases unsatisfactory in its results, because it cannot be complete. It is attended sometimes by serious risks, because with a curette in the attic we are groping in the dark. If the tegmen tympani be perforated and the membranes be pouting through it, we add little fresh danger and remove great danger by a complete mastoid operation ; whereas by curetting such pouting membranes through the meatus, and leaving, as we then must do, an imperfectly disinfected middle ear and antrum, we add fresh dangers while removing none. If, then, we are satisfied that the danger of a complete mastoid operation is less or even not greater than the operation for removing remains of membrane and unnecrosed or necrosed ossicles in a case of persistent otorrhœa, we have next to inquire what we may expect as regards hearing from the one or the other. In my opinion the more complete the operation the better is the hearing result. The explanation of this appears easy. Removal of the membrane through the meatus can only be imperfect ; traces are left, cicatrices and some re-growth occur, and the sound waves have to that extent an obstructed passage to the fenestræ. With the drum membrane and ossicles away, useful hearing can occur only if the round and oval windows are free to vibrate, and if the sound waves are unimpeded in their access to them. If either window has been fixed, the other must be very restricted in its vibrations, and consequently the perilymph of the vestibule and cochlea cannot be made to oscillate between the two windows. If with persistent otorrhœa we have indications from the surface of the mastoid of implication of its cavity, or of its cells, a radical operation is of course indicated. If an otorrhœa, instead of being persistent, is only occasional, with intervals of apparently complete health, we would not as a rule be justified in recommending an operation, unless the attacks were frequent and incapacitating. Often attention to the nose and nasopharynx, accompanied by more fresh air, night and day, and more exercise, will lead to a cessation of such attacks. Still, if in such cases hearing be very defective, operation, I believe, would be good treatment—provided other treatment had received a good trial.

Sometimes attacks of acute otorrhœa, with the pus discharging through a small perforation, have acute mastoid periostitis with suppuration. To do a radical mastoid operation in all such cases would be to often operate quite unnecessarily, as better drainage to the middle ear and a Wilde's incision over the mastoid will generally meet the requirements of the case. Cases suffering from recurrent attacks of otorrhœa and earache whose drum membrane has largely disappeared, whose unnecrosed ossicles are retracted and bunched up with remains of membrane in the posterior segment of the middle ear—and whose hearing in each ear is considerably below that of a successful radical mastoid case—should have the advisability of operation explained to them.

Cases of cholesteatoma of the middle ear and of the antrum should be subjected to a very thorough radical operation.

Another class of case upon which I have been anxious to try the effect of a complete mastoid operation is the class containing cases of extreme deafness with good bone conduction, complete but retracted and thickened membranes, and evidence of exudative catarrh : I speak of cases which are to all intents and purposes deaf mutes. It is, of course, impossible to be sure prior to operation that the stapes is not fixed. But there are some cases that have appeared to me to deserve the chance, which such an operation would give. The necessity one feels for giving an unhopeful outlook has so far deterred the parents of cases from having them operated on. It does not appear to me far-fetched to assume that, in some of these cases, the drum membrane and ossicles are themselves sufficient obstacles to the conduction of sound waves, without there being anchylosis of the foot-plate of the stapes.

Most aural surgeons appear to be now agreed that where it is necessary to open the mastoid antrum it is right and advisable, except, of course, in some acute cases to make antrum middle ear and enlarged meatus one cavity. Only by this means can we hope to eradicate the disease and give the patient permanent relief, and only by this means can we have the parts permanently open for inspection. The names of the three pioneers in this branch of surgery, Schwartze, Stacke, and Zaufal, have been attached to their operations. More recently Ballance's operation has combined their advantages.

I should like to again pay a tribute to Macewen's work on pyogenic infective diseases of the brain and spinal cord. It is a book which an aural surgeon can scarcely afford to be without. Ballance has modified various steps in his procedure, but the essential steps, it is pretty generally agreed, bring the operation up to date. Charles Heath has lately led a crusade against skin-grafting, and against the considerable exposure of the mastoid which Ballance's operation requires. He has reported a very unusual number of operations. Heath's assumption that aural surgeons have not been sufficiently careful to protect the fenestræ during, and to secure good hearing after, operation, and that they have largely failed to do so, is unwarranted; and it has very naturally been resented. There appears to be little really new in his method, but his skill as an operator appears to be acknowledged and undoubted. I have been extremely satisfied with Ballance's skin incision in the line of the hair, with the large field for operation and with the displacement forward of the auricle and cartilaginous meatus.

Since the publication of Macewen's book on pyogenic infective diseases of the brain and spinal cord I have used burrs driven by a strong dental engine (a reliable motor would be better) for the whole operation, and would only use a chisel and hammer on the head if burrs were not available. A gentler and more accurate instrument to work with than a burr it would be difficult to imagine. Its use reduces the danger of damage to a misplaced sigmoid sinus, or misplaced middle fossa, to a minimum. They are, even when misplaced, exposed so gently that no harm to the non-bony wall of sinus or to the dura mater results. I have had personal proof of this. The use of a burr throughout makes the enlarged bony cavity beautifully smooth, leaves none but the natural irregularities, and helps, I think, in preserving the skin lining of the meatus, whose preservation very materially helps the grafts—if we use these—to quickly line the enlarged bony cavity with skin. I have found the supra-meatal triangle an accurate guide to the antrum. All operators dwell upon the importance of avoiding injury to the facial nerve, and upon the necessity for gentle curetting of the middle ear. Charles Heath is inclined to think that injury is often done to the fenestræ by the curette. It appears to me that the situation of these in their niches makes injury to them with a curette in the highest degree unlikely. By the use of burrs, and of a Stacke's protector or a bent probe in the aditus, injury to the facial nerve may be prevented.

Ballance's treatment of the meatus has given me good results. I am careful, however, to preserve as much as possible of the skin of the bony meatus with the cartilaginous meatus. Ballance incises the floor of the meatus in its long axis well into the concha, and then curves the incision upwards and backwards in the concha to a level with the anterior extremity of the helix. The stitching of the raw surface of the conchal flap to the raw surface of the mastoid flap has prevented any alteration in the appearance of the auricle from that of the other side. Nothing but good appears to result from packing with either strips or balls of gauze. Thiersch grafting of the granulating bony surfaces a week or more after the original operation has given me excellent results, and I should be sorry to discontinue it. The

difficulty in applying the graft may possibly be much diminished by Waggett's modification of Deansley's suggestion to apply sticking plaster to the skin of the thigh before cutting the graft. Waggett uses a proprietory article called "new skin." It is like collodion, and a good imitation of it can, he believes, be obtained by dissolving celluloid in acetone. "As soon as the antiseptic dressing is removed from the prepared thigh a few drops of 'new skin' are poured over the part and quickly spread with the finger. In one minute this has dried to a fine varnish." The graft can then be easily and quickly cut, and has no tendency to curl. Waggett has found such a graft easy to apply and very satisfactory. The only doubt that would occur to me is whether it would be as likely as an unvarnished graft to apply itself closely to the irregular surfaces of the bony cavity. Its use does away with any necessity for gold leaf, and I am inclined to give it a trial, although pleased with the use of gold leaf. Packing after applying the graft should be very accurate, and probably this would specially apply if "new skin" be used. The tendency is to use little balls of gauze instead of strips of gauze. Waggett attaches a silk thread 3 inches long to each aristol ball, to diminish trouble in removing them. The shed portions of the graft are said to be easily removed on the eighth day, in one or two large pieces, when "new skin" is used. I have found the grafts to shed layers of epithelium sometimes for months after operation. Surgeons differ regarding the advisability of not closing the tympanic orifice of the eustachian tube. Some endeavor, by curetting the orifice carefully and by care in applying the graft, to obtain closure. It can hardly influence the hearing, but on the whole appears to be an advantage. The patency of the tube may, of course, lead to catarrhal attacks spreading upwards from the naso-pharynx, and I have noticed during such a catarrhal attack without noticeable discharge that hearing was distinctly less good than at other times. Where packing has to be changed, either prior to or subsequent to grafting, I have found weak peroxide solution useful for mopping out the cavity, and where granulations are troublesome spirit drops are useful.

The objection that Thiersch grafting is likely to diminish hearing by interference with the fenestræ is, I believe, theoretical. The grafts could only apply a thin layer of epithelium to the membranes in the two windows, and no epithelial graft would live over a fenestra unless it applied itself closely and became part of the membrane. This will not occur unless the membrane of the fenestra has lost its own epithelium.

My own experience is that the graft does not increase the thickness of the membranes in the round and oval windows, and that it does not interfere with a good hearing result. After a radical mastoid operation we have an enlarged meatus—we have, I fancy, a resonance chamber in the large antro-tympanic cavity; and if the fenestrae be free to move, the perilymph is put in motion by the sound waves, though of course less well than if the drum membrane and ossicles are in a healthy state: much better, though, than is possible if with an apparently healthy drum membrane we have the ossicles clogged by exudative catarrh, and acting as a bar to the transmission of sound.

It is quite true that in the great majority of cases the object of operation is to remove a danger to life: but if, in addition to this, we can obtain useful hearing, it makes operation more worth while, even if the other ear possesses normal hearing.

The hearing which, I believe, we have reason to expect in cases whose internal ears are healthy, and where there is no bony occlusion of either window, is distinct speech at 8 yards. I have obtained this in childhood, young manhood, and middle life.

Dr. G. H. Hogg (Launceston) stated, in response to the President's invitation, that he had had one case of permanent facial paralysis in a patient suffering from tubercular disease of the ear, in whom he removed the whole mastoid process for extensive disease; he had had one case of temporary palsy, developing on the third day after the operation, but making a good recovery. As regards the after results, both in his own and other surgeons' cases, he had very frequently found months or years afterwards that in cases which had been regarded as complete cures there was present a slight fœtid secretion or discharge.

Three years ago, in Hobart, he had condemned operations on the attic through the external meatus as absolutely unscientific, and had been rather snubbed for his views. He was very pleased to hear that Dr. Lockhart Gibson was of the same opinion, and that others had seen fit to change their former views

Dr. S. A. Ewing (Melbourne) said—It is important, in treating chronic suppurative disease of the middle ear, to eliminate nasal and naso-pharyngeal factors.

The minor operation of ossiculectomy is often successful and worthy of a trial. If the radical operation be decided upon, it should be thoroughly done; for example, in long standing cases the dura mater is not infrequently exposed, and a careful search often reveals areas of necrosis not evident unless a free opening be made. The more thoroughly the operation was performed the less necessity there existed for skin-grafting operations. Lake's method of placing small pieces of sponge beneath the bridge of the aditus was preferable to the use of Stacke's protector, which, unless skilfully employed, might injure the facial nerve.

In performing the radical operation, good assistance and light were essential.

In reference to the hearing after the operation, he was of the opinion that though this was often unaltered and sometimes improved, yet it was impossible to definitely predict the result.

The President thanked the openers of the discussion for the capable manner in which they had dealt with the subject. He was glad to know that the experience of those present showed that the danger of permanent facial paralysis following the operation was very remote. The solitary case mentioned by Dr. Hogg, where he had to deal with a destructive tubercular disease of the mastoid, was rather the result of the disease than of the operation.

With regard to the relative merits of burrs or gouges, if an efficient surgical engine such as the S. S. White were employed the bone could be removed rapidly. The drawback was the weight of the machine, which, however, was a very useful instrument, if kept in the private hospital of the operator. Heath's incision gave sufficient room in ordinary cases; but it did not suit so well where there was much œdema of the soft parts. One feature of the incision was good, namely, the manner in which the upper part of the same was carried out. Here, after making the skin incision, the pinna was dissected downwards till the external auditory meatus was exposed—the temporal facia was not opened, and the risk of pus infection of the muscular planes was lessened. This modification could be made in any of the usual incisions, instead of carrying them down directly to the bone through the facia, as was generally done.

Dr. Webster, in reply, said—I am gratified at the way this operation has been discussed, and I think most of us have benefited to some extent from the discussion. Some matters have been introduced which I purposely excluded from my paper; but these have, perhaps, added interest to the subject. Dr. Ewing's remarks were very much to the point, and the com-

parison he was able to make of the work at two London hospitals in which methods differed has special interest and value. I am glad to find him agreeing with me as to grafting being generally unnecessary, and he properly emphasized the importance of leaving a smooth surface on the bone, a point to which McEwen devotes much pains. His comparison of the operation to that of sinking a shaft is most apt; it brings out most clearly the necessity of a good assistant to keep the field of operation clean and dry.

With regard to Wilde's incision, I do not think it should be mentioned in connection with the radical mastoid operation. I look upon it as an entirely useless proceeding. It was introduced at a time when the anatomy and pathology of the temporal bone was little understood, and when surgeons were afraid of operating on it, and it ought to be given up.

I listened with pleasure to Dr. McCallum, who has had the advantage of working with McEwen. The latter had a different object in view to that which we strive for. He endeavored to obtain a barrier of dense tissue behind the ear to form a protection against further infection when the whole of the disease could not be removed; and his dressing was done behind the auricle. Our object is to leave a large cavity which can be dressed and inspected, and remain open for inspection through an enlarged meatal orifice. McEwen's cases were, of course, almost all those with intra-cranial complications.

Dr. Arthur mentioned pain around the ear as sometimes seen after a radical mastoid operation. I have seen two such cases. In one there is still a little diseased bone in the floor of the middle ear, which might account for the pain; in the other I can find nothing to account for it.

THE DIAGNOSIS AND TREATMENT OF SUPPURATION OF THE ACCESSORY SINUSES OF THE NOSE.

Dr. A. J. Brady, President, in the Chair.

The President said the discussion to-day was on "The Diagnosis and Treatment of Suppuration of the Accessory Sinuses of the Nose." In the treatment of these affections the methods of operation were not yet so fixed as in the case of the mastoid. This particularly applied to operations on the frontal sinuses and ethmoidal cells, which gave room for a good deal of discussion as to the best procedure. In suppuration of the antrum of Highmore, the radical operation in chronic cases was undoubtedly the best. If, in addition to a counter-opening in the floor of the nose, a portion of the anterior end of the inferior turbinal were removed, the patient could wash the antrum with a ball syringe from the nose without the use of any special tube, and the opening was not so likely to close. The wearing of drainage tubes was done away with; and uncomplicated cases, as a rule, ended in speedy recovery.

Dr. Kirkland and Dr. Hogg opened the discussion.

Dr. Kirkland (Sydney): Mr. President and Gentlemen—To be asked to open a discussion on diseases of the accessory sinuses of the nose was an unexpected honor. I feel that the task is one requiring rich experience and frequent opportunity of observing the pathological consequences as well as the clinical signs.

At the very beginning I am encountered with the difficulty of deciding what should come within the scope of a discussion intended to bring forth

what is best fitted to help us in our daily practice. We are always striving after progress, and if we succeed in adding even one new fact to this department of specialism our discussion will not have been unfruitful. One could traverse the whole domain, beginning with the historical review, the clinical, anatomical, and pathological phases, and end with the surgical; but such, I deem, would be too comprehensive for one paper. I therefore shall confine myself largely to my own experience, touching upon points in correlation thereto.

The etiology concerns both acute and chronic cases; but as the first is the precursor of the latter, they can be considered as having a common cause. There are four hypotheses for the occurrence of antral suppuration. First, that the disease begins as a primary affection of the sinus; second, that the disease begins in structures in direct continuity with the sinus, *e.g.*, in the nasal mucous membrane; third, in the root of a tooth which is in direct contiguity; fourth, that the secretion of the nose, nasopharynx, or some part of the respiratory tract, through sneezing or coughing, is forced into one or more of the spaces, just as secretion sometimes finds its way from the nasopharynx into the middle ear and sets up an acute otilis media. It has been proved that there is no specific bacillus. The discharge may contain staphylococci aurus, albus pneumococci, and even streptothrix. I suspect that the pneumococci and influenzal bacillus are perhaps the most active factors in the production of suppuration of these cavities. Luc mentions the case of a student attending the *post mortem* of a patient dead of pneumonia, and who suffered afterwards with suppuration of the frontal sinus from which a pure culture of the pneumococcus was obtained. At the last Congress I showed that, in a series of *post-mortem* examinations of fifty cases dying of pneumonia, twenty-five of them had suppuration in one or more accessory sinuses.

The shape of the nose and the character of the constitution are mostly responsible for acute cases becoming chronic; the strumous diathesis is the one which favors pan suppuration. I take as granted that many cases, acute in origin, recover within a period varying from one week to three. Those becoming chronic have usually some anatomical or pathological stenosis of the ostium.

The theory of dental origin of maxillary empyemata is one which requires our earnest consideration. Tilley is a strong advocate of this theory, and dentists, acting upon it, have ventured in a good many cases to open and drain the cavity. In my experience only a very small number are due to this cause, and these I would distinguish as pure empyemata, having very slight pathological degeneration of the lining membrane, and capable of being treated through the maxillary process. I would here enter my protest against dentists undertaking work quite outside their sphere, and which is, in the nature of things, beyond their therapeutic capabilities. I am sure those who do usually forfeit the confidence of their patients, as the patient drifts into the hands of the specialist finally.

The pathological contents found in maxillary antra may be enumerated as serous fluid with a cyst wall, thick creamy pus, inspissated pus, polypi, new growths, a loose tooth, and sequestrum. In many cases the pus in the antrum is associated with nasal polypi and hypertrophy of the middle turbinate. We meet with cases which have all the characters of antral suppuration—pus in the nose, nasopharynx, absence of the light spots under the eye, &c., and which, in being opened, show no obvious suppuration. In some of these cases I have curetted a large amount of polypoid tissue, representing a degeneration of the lining membrane. One is forced to ask the question, in the absence of microscopical examination, are these cases of the same nature as those found with unmistakable evidence of suppuration?

There are also some cases which, on being opened, leave one in doubt as to the correctness of the diagnosis—no pus, no polypoid tissue ; yet, a few days later, secrete pus. Are those cases of drained empyemata, or has the antrum become infected through an operation conducted on perfect aseptic lines ? I believe they are empyemata found empty at the time of operation. With reference to the diagnosis, the symptoms are so clearly understood that I need only refer to cases which cannot be diagnosed if we rely on the ordinary classical symptoms. There is a class of cases to which Dr. Brady drew attention in an article in the *Journal of Larynxology*, viz., those in which no secretion is seen in the anterior part of the nose, but in which, on a rhinoscopic examination, a muco-purulent secretion is visible extending from the lower aspect of the middle turbinate and running over the soft palate. We explain those cases by the natinal ostium having a situation more posterior than usual. My experience is that these cases are less purulent in their contents—possibly due to a larger opening.

Having decided in a given case that there is suppuration of a chronic nature going on, we next ask the question, what is the cause ? If it is a tooth, remove it, and make a wide opening for drainage through the socket and carry out antiseptic irrigation for a time. If this fails, the conclusion is arrived at that there is extensive degeneration of the lining membrane, for which all operations singularly fail except a radical one. The patient is more or less annoyed that a second operation is necessary ; and this has forced me to examine the symptoms carefully from a differential point of view, and in many cases perform at the outset a radical or Caldwell-Luc operation. When this is done the results are excellent, and in 90 per cent. of the cases a complete cure is obtained within a short time. I assume that all pathological conditions in the nose which would retard a cure have been in the first instance removed. The disadvantages of this operation may be summarised in the greater loss of bone in the anterior wall and the ensuing numbness, which generally disappears in a short time. There is a point in this connection to which hitherto no attention has been drawn, viz., the interference with the nerve supply of the teeth. Patients have pointed out that the teeth after this operation feel dead in the immediate neighborhood, and I am informed by a capable dentist that the teeth really decay later on ; personally I have not observed such to take place, and if this really happens it would be a dreaded drawback to the performance of this particular operation.

The Caldwell-Luc operation is the most satisfactory operation, both for the doctor and the patient. It involves a greater degree of destruction of the bony wall of the antrum, it inflicts a greater shock to the patient ; but, on the other hand, it secures to the patient, in most cases, a lasting and permanent cure. In this operation an opening is made with a hammer and gouge, a trephine, or a bar worked by an electric motor, large enough to admit the forefinger into the cavity, which can be used to feel every nook and corner. The maxillary antrum is irregularly pear-shaped, with the apex above. As this point has not yet received a name, I would suggest that it be called the "orbital recess"; the extension of the cavity towards the malar bone might be called the "malar recess"; and the space above the maxillary process might be called the "maxillary groove." The next step in the operation is the curettage of the lining membrane. This should be thorough, leaving no space with a thickened degeneration of the membrane untouched. Good light and frequent cleansing are necessary for a thorough inspection. The next step is a counter-opening into the nose, which is intended to remain more or less permanent. Small openings soon close up through the continuous effort on the part of the tissues to return to their former position. Whether this opening should be situtated in the inferior meatus or represent an enlarged natural ostium is

much a matter of individual preference; no doubt nature placed the opening high up for a good reason. It certainly is not in the best position for drainage, which is the chief consideration for the surgeon. Placed in the inferior meatus it would be more open to infection, and would be covered by the inferior turbinate; nature had therefore no choice in her selection. Situated as it is, high up in the cavity of the nose, it affords an easy egress for the secretion from the tubular glands on the nasal wall, propelled as it is by the ciliated epithelium of the lining membrane. The lining membrane, as Lennox Brown points out, is not a true mucous membrane: it is largely composed of connective tissues. The opening in the nasal wall can be made by trephine or punch forceps. I introduced a punch forceps two years ago for this purpose; it can also be used at the same time after puncturing a portion of the inferior turbinate. At first I had an almost sacred regard for the inferior turbinate, but I am disposed to now think that its partial removal in this operation outweighs in advantage its functional loss. I have abandoned packing after the operation as unnecessary and somewhat harmful to the patient. The subsequent treatment consists in irrigating the cavity through the nasal opening with the aid of an eustachian catheter. As a rule the cure is established within a few weeks. In some cases I have found this even unnecessary, as no pus was present after the operation.

To sum up the different operations recommended for antral suppuration: First, maxillary process drainage; second, canine fossæ drainage; third, Caldwell-Luc operation, or radical operation.

The frontal sinus, by virtue of its communication with the nose being a small opening, is very difficult to cure. Coupled with this anatomical difficulty the sinus is frequently large in size, extending outward towards the outer canthus of the eye, and not infrequently downward towards the orbit; now and again it is divided up into compartments, the division being incomplete. The median division between the two sinuses is occasionally perforated, leading to bilateral infection. In addition to this we have, in most cases, involvement of some or all the ethmoidal cells. It is easy to see that, however much we enlarge the infundibulum opening, drainage can only be very imperfect. It is frequently recommended that a large opening be made through the infundibulum; and no doubt, if this could be done and maintained, our efforts would be in most cases successful in producing a cure. Nature asserts itself in trying to re-establish an opening of the former size: granulations spring up, and, unless we are constantly watching the progress of the cure, our efforts are frustrated. I take it for granted that most men recognise that some cases are best left alone; and, unless we can promise a cure, are we justified in recommending an operation? In most cases a cure could be obtained with the exercise of great care on the part of the surgeon in the after-treatment, combined with patience on the part of the patient. In the case of men, time is of great consequence; and in the case of women, deformity or scarring should be avoided as much as possible. What are the conditions which determine our decision in a case of frontal sinus suppuration? I am guided by the condition of the infundibulum: if it is large, and can be kept open and the whole of the ethmoidal cells can be removed, there is a reasonable prospect of cure. If the case is very chronic, and the patient suffers intermittent pain, it is probable that polypoid degeneration of the lining membrane has taken place, leading to temporary retention of the discharge. Here we are confronted with the possibility of extension of the disease towards the brain. An operation in this case would be clearly indicated, as it would provide for drainage, give the patient a chance of a cure, and lead to nothing worse than a small infra-orbital scar. If we are consulted in a case, say, of a patient about middle-age with bilateral frontal

sinus suppuration, with no hindrance to drainage, and no inconvenience beyond the discharge, what should the surgeon advise? My advice in a case of this kind is to tell the patient that an operation would be necessary only in the case of the supervention of pain, another symptom indicating a possible extension beyond the sinus. It is surprising how many cases go on for years without danger arising. I know of no data telling us what percentage of cases terminate fatally through extension to the brain, or by the generation of a new disease of the chest or blood. Constant antiseptic cleansing of the nose may keep some cases quite comfortable for years. I am disposed to think, however, that in the end these conditions must shorten life. The consequences of a chronic suppuration of this kind are many, both locally and remotely. In the nose it produces hypertrophy of the structures over which the pus flows, polypi alteration of the ethmoid in the direction of rarefaction or condensation through a chronic osteitis: remotely it produces anæmia, leucocytosis, gastric derangement, bronchiectasis, and pneumonia. Conservative methods of treating sinus suppuration nearly always fail—such as politzerisation, syringing through the infundibulum, and removal of nasal obstructions. The operations in vogue are Ogston Luc, Kuhnt's, Jansen's, and Killian's; but I think most men confine themselves to the Ogston Luc, the Kuhnt's, or Killian's. The latter I have not yet performed. The Ogston Luc operation consists in making a curvilinear incision under the eyebrow extending from the naso-frontal suture to the supra-orbital notch, the knife cutting to the bone. The periosteum is raised and a large opening is made in the frontal wall of the sinus by means of a gouge trephine, or burr (personally I always use the gouge). The cavity is carefully curetted, the infundibulum enlarged, all granulation tissue removed by a piece of gauze passed into the nose to which a sawing movement is imparted. The sinus is most carefully antisepticised with pure carbolic acid and packed with gauze passed down into the nose. Preparatory to the operation all polypi in the nose are removed and the ethmoid cells cut away by Grunwald's forceps; stitches are inserted, leaving, however, a free opening through which a piece of gauze is passed. The gauze is removed on the second or third day, the sinus is irrigated with HgCl. lotion, formalin, or a weak solution of Ag. NO_3 and thereafter packed daily. This operation has undergone many changes in its solution. Some years ago the textbooks advised the insertion of a drainage tube; this was found more or less unsatisfactory, producing a copious discharge, growth of granulation in the neighborhood of the infundibulum, and a free purulent discharge in the nose. The next change was to do away with the drainage tube and close the wound at the time of the operation. My colleague, Dr. Brady, performed this operation on many occasions: he tells me that it was frequently successful in curing the discharge. It, however, has many disadvantages. Firstly, it may fail, and this subjects the patient to an operation without any good results; secondly, it favors the occurrence of a septic osteomyelitis, which is almost invariably fatal; and, thirdly, should it fail, the sinus refills with pus, which now and again causes the weak cicatrix to yield and the pus to trickle over the face. The operation which Tilley recommends is the open one, with the insertion of a drainage tube or silver wire which is removed about the third week. The soft tissues are pressed against the posterior wall and the sinus closed. The essential conditions for the cure of chronic suppuration of the frontal sinus are, firstly, complete removal of all pathological conditions of the ethmoid cells undertaken anteriorly to the external operation; secondly, free drainage into the nose after the operation, and thorough removal of all diseased membrane within the sinus itself.

In what way is the removal of the ethmoidal cells together with polypi accomplished? Tilley and Lask recommend the use of Meyer's ring knife.

This will succeed in certain cases where the higher region of the nose is fairly wide ; in some cases even a small knife is too large, and in these cases I find Hajek's oblong curette more useful. We are, however, I think, exposing our patient in the use of either of these instruments to an unnecessary risk of septic phlebitis. It is clear that curettage in the presence of pus incurs a risk of some septic organisms finding their way into one of the veins. For this reason I more frequently use Grunwald's forceps, in the use of which I think there is less chance of accident. From Tilley's account of suppuration of the frontal sinus one is led to the belief that a cure is easily accomplished provided attention is given to all the details. A statement later on in the same chapter tells us that he has seven cases under his care for which he does not recommend an external operation. This, I think, shows that he is not quite as confident of producing a cure as the earlier part of the book would make us believe.

Grunwald's method of opening the sinus through Ogston's incision, followed by irrigation, takes from six weeks to six months to produce a cure. The great disadvantage of this method is the growth of granulation tissue in the infundibulum, and the constant tendency of the wound to close. Whatever method of an open kind is undertaken, it is absolutely necessary that the after-treatment should be carried out by the operator himself. The dressing of these cases requires special skill, and to leave the subsequent dressing to an assistant is to court failure. I have endeavored to produce a cure in these cases by filling the cavity with paraffin and iodoform after an interval of two or three weeks of aseptic irrigation. In only two cases have I succeeded. Lately I filled the sinus at the time of operation, but the whole mass was extruded within ten days. I think something in this direction is possible, and I hope to experiment with it a little further. Success achieved by a method of this kind would be a great boon to the surgeon. We sometimes obtain a speedy cure quite unexpectedly. Not long ago I operated upon a woman for frontal and maxillary sinus suppuration. The gauze was removed on the third day and the wound closed. The patient thereafter disappeared. Two months later she returned with a slight affection of the ear. I then found that a complete cure had taken place in both cavities, there was no pus visible in any part of the nose or naso-pharynx ; and, as I have seen her several times since with an aseptic nose, I regard the case as a complete cure.

The most satisfactory operation is the radical operation, in which complete obliteration of the sinus can be obtained. This is the operation of my choice in any case suitable for its performance. We hesitate to do this on women more especially, and in men to whom a conspicuous scar or deformity is a disadvantage. The cases in which it is really the operation *par excellence* are those in which the sinus is small. The after-treatment of these cases differs in no way from the other cases—a gauze drain inserted down the infundibulum until complete coaptation of the surfaces of the soft parts and the posterior wall has taken place. In this operation, in my first case, I erred in not removing sufficient bone in the region of the nasal opening, so that I found the upper part healed quite well, and left a small canal below which continued to secrete longer than if it had been partially obliterated.

English writers content themselves by dividing the ethmoidal cells into two sets—an anterior and a posterior. On the Continent a much more elaborate division is attempted, especially by Killian. For practical purposes the English classification is all that is desired, but perhaps, from a scientific view, the continental is to be recommended. In the higher mammals the nose is more highly developed than in man, and the olfactory sense is sharper ; so that the nasal organs of man have undergone retrograde changes. Killian

distinguishes at least four turbinates, together with accessory ones. For surgical purposes we can content ourselves with the division given above if we remember a third set, viz., those in connection with the frontal sinus or fronto-nasal cells. Disease may involve these cells in suppuration, or give rise to a mucocele. I have also found these cells much extended without any abnormal contents, representing a form of rarefying osteitis. The pathology of these cases is more or less unknown. The patient presents in some cases an extended nasal bridge, complains of continuous pain—which is probably due to pressure on the nerves—and yet removal of these cells, which opens up a wide expanded attic, fails to give relief. Dr. Brady will remember a case of this kind in the Sydney Hospital, which was under my care in the earlier stages, and later on under his treatment. We meet with cases in which this state of rarefaction is limited to one cell. I have on many occasions, when removing the middle turbinate, found a large empty cell. We meet with cases in which no abnormal state is present in the ethmoid, and in which pain is the only symptom complained of. After excluding all reflex conditions likely to produce pain in this region, what is our diagnosis? There is no pressure of the middle turbinate against the septum, and nothing really within the range of sight to explain the pain. I have sometimes wondered if there is some hidden cell producing pressure; we do meet with unexpected conditions if we are patient enough to search into the hidden recesses. Killian introduced his long specula for this purpose. The most important and interesting condition is that of suppuration of these cavities; suppuration of these cells brings with it polypi, and more or less osteitis. The removal of these conditions is a necessity for the patient's comfort, and to prevent further extension; continuous suppuration in this region more than any other produces morbid conditions of the pharynx and larynx. The pharynx is not infrequently glazed over with a thick layer of secretion, which, in time, tends to produce a pharyngitis sicca. As it trickles down the larynx it begets a chronic laryngitis with extensive changes in the vocal chords; its remote effects go further in some cases by producing bronchitis, and even bronchiectasis. Shall we remove these cells piecemeal by Grunwald forceps, or adopt Lack's heroic method, which he says is unattended with danger? Personally I cannot believe an operation performed in the dark, as Lack's is, with very free hæmorrhage and only the sense of touch to guide you, as one free from great risks. It is true that I have performed the operation many times with no untoward result; but, rationally, I believe the operation by Grunwald's forceps conducted within the range of vision a safer and quite as satisfactory operation. A curetting operation is more likely to open up veins in a lacerated state, into which septic organisms can pass, than a cutting operation. In the cutting operation no pressure is made in an upward direction. I have a case at present under my care in the hospital in which access to an ethmoidal cell could only be obtained by an external operation. The ethmoid cells were all removed, pus continued to flow, and the patient complained of pain near the inner canthus. I suspected the frontal sinus, which, on exploration, was found absent. I then embarked on a voyage of discovery lower down, and found a small cell full of pus, which I regarded as an ethmoid cell; when this was opened the pain disappeared.

Dr. G. H. Hogg: Mr. President and Gentlemen—First of all permit me to express to you my sense of appreciation of the honor you have done me by inviting me to open this discussion; together with the diffidence I feel in venturing to do so in the presence of yourself, Sir, a rhinologist of more than Australian reputation, and of the other distinguished representatives of this section of surgery.

Clinically, as well as anatomically, the accessory sinuses of the nose may be divided into two groups :

1. The anterior group, consisting of the maxillary and frontal sinuses and anterior ethmoidal cells, all of which have openings leading into the middle meatus of the nose—*i.e.*, below the level of the middle turbinal :

2. The posterior group, consisting of the posterior ethmoidal cells and sphenoidal sinuses which open into the superior meatus—*i.e.*, above the level of the middle turbinal; and this classification is, as will be seen later, valuable from the point of view of diagnosis, for according to the position which the pus occupies in the nose the rhinologist forms his conclusions. Clinically, too, suppuration has been classified as acute and chronic—as suppuration with retention of the inflammatory products, and suppuration with free drainage.

In the present discussion, which deals with diagnosis and treatment, it seems to me advisable to adopt the acute and chronic classification. Acute suppuration is met with most frequently as a complication of the exanthemata ; as a sequel to influenza it is not uncommon, and demands a moment's notice.

In the maxillary antrum its presence may be indicated by general malaise, rise of temperature, perhaps rigors, pain in the cheek and forehead, perhaps tenderness over the cheek and gum, stiffness in the nose, with muco-purulent discharge. A free discharge of pus from the nostril of the affected side may occur with relief of all the symptoms ; sometimes eye symptoms, such as pain on the eyeball, photophobia, &c., may be present.

When the frontal sinus is affected, the symptoms may be much the same ; the pain may be felt more in the forehead, and may be accompanied by a feeling of oppression and fulness over the eye, tenderness on the floor of the sinus, with sometimes redness and œdema of the upper eyelid. The pain or discomfort may abate or disappear with the escape of pus into the nose. I have met with several such cases following influenza ; and know of one case which passed from the acute into the chronic stage, and which subsequently had a fatal termination from perforation of the posterior wall of the sinus and abscess of the frontal lobe.

The ethmoidal cells and sphenoidal sinus may also be affected ; such cases, though much less frequent, are accompanied by similar general symptoms. In addition, eye complications and cerebal symptoms may develop. In the sphenoidal cases recorded by Schäffer, the headache was intense over the vertex and occiput, and giddiness was marked. Acute suppuration of the ethmoidal cells has been met with as a sequel to gonorrhœa, exophthalmos and orbital cellulitis developing. In these acute forms the methods of examination by rhinoscopy, trans-illumination, &c., are to be applied as described in a later section.

Treatment will be that of the causal disease and the concomitant nasal inflammation. Should the suppuration not subside, or threatening symptoms develop, operative measures will be required.

Chronic Suppuration.—The general symptoms are :—(1) Purulent discharge, with offensive taste or smell ; (2) nasal obstruction ; (3) pain and tenderness ; (4) eye symptoms ; (5) cerebral symptoms ; (6) throat and chest symptoms ; (7) gastric symptoms ; (8) neurasthenic symptoms.

Purulent discharge may have lasted weeks, months, or years ; may be continuous or intermittent, coming once or twice a day, and is often affected by the patient's posture. It is usually unilateral, but may be bilateral, even when only one sinus is affected, as the pus may pass from one side of the nose into the other. The discharge may be muco-purulent or purulent, of a yellow or greenish color (it is said to be greenish when it comes from the frontal sinus, but this is not diagnostic). It is often fœtid, and the patient complains of the sweetish fœtid taste and smell (contrast it with ozæna) ; it may come from the

nose or run into the nasopharynx, and be expectorated. Sometimes there may be little or no discharge; I have seen suppuration of the antrum where there was neither discharge nor accumulation of purulent fluid, the pus apparently drying and forming a fœtid deposit on the antral walls, the patient complaining of the offensive smell and taste. Possibly some such cases correspond to what is called "ozæna of the antrum" by some writers.

Nasal obstruction may be due to the presence of enlarged turbinates and congestion of the nasal mucous membrane, or, more frequently, to the presence of polypi.

Pain and tenderness may or may not exist; if present, it varies very much in character and degree, and may be continuous or intermittent, being relieved by the free escape of pus. In maxillary trouble it is described variously as like toothache, neuralgia of the face, or frontal headache. Possibly the presence of more or less severe and continuous neuralgic pain in some of these cases may be due to irritation of the infra-orbital nerve, which may become exposed by disease of the bony canal or by natural defect thereof. In frontal cases supra-orbital headache may occur, or the headache may be diffuse or even occipital; in ethmoidal disease the pain may be frontal, or deep-seated, and referred to the back of the orbit; while in the sphenoidal cases vertical headache has been described as characteristic; but occipital and diffuse headache, as well as pain behind the eyeball, may occur. The seat of pain is seen, therefore, to be of no constant value in fixing the site of the sinus disease, although, combined with other symptoms, it may be helpful in diagnosis.

Eye symptoms and complications are rare in maxillary disease *per se*, and I have never met with them; but upward displacement of the eyeball, œdema of the lower lid, lachrymal obstruction, &c., are said to occur. In frontal disease the upper eyelid may become œdematous; there may be swelling at the inner and upper angle of the orbit, and orbital abscess may develop, perforation taking place about the middle of the upper eyelid. In the posterior ethmoidal and sphenoidal cases, eye complications are more frequent; orbital abscess, sudden loss of vision from compression of the optic nerve, optic neuritis, thrombosis of the central vein of the retina, cavernous sinus thrombosis, exophthalmos, may occur; the third, fourth, and sixth cranial nerves may be pressed on at the sphenoidal fissure, and various paralyses result.

Brain symptoms and complications are very rare in maxillary disease, although they have occasionally been recorded. When they occur in the other forms of sinus suppuration, they may be caused by direct infection through perforation of the bone or through the vessels of the diploe.

In frontal disease abscess of the frontal lobe is the most frequent, infection usually taking place through a perforation of the posterior wall of the sinus. In this, as well as in ethmoidal and sphenoidal disease, orbital abscess may also cause brain trouble.

In ethmoidal disease abscess of the frontal lobe, extra-dural, sub-dural abscess, meningitis may occur; while in sphenoidal disease these, as well as thrombosis of the cavernous, petrosal, and longitudinal sinuses, may follow; in some cases giddiness, tinnitus, and nausea have been prominent symptoms.

Throat and lung symptoms—cough, huskiness, and irritation of the throat—may be caused by the escape of pus into the naso-pharynx, while the cough and profuse purulent expectoration have in some cases given rise to a diagnosis of phthisis, bronchiectasis, &c. Asthma has occasionally been associated with sinus disease.

Gastric symptoms, sickness, indigestion, foul tongue and taste, loss of appetite, &c., may be complained of.

Neurasthenic symptoms have been most marked in several cases which I have met with—lassitude, mental depression, inability to do mental work, &c.

—and have persisted even after the local trouble has been practically cured. These symptoms have been attributed to the septic absorption from the purulent products of the sinuses; but in one or two cases of my own the quantity of pus has been so small as to be almost a negligible quantity.

Objective Phenomena.—Rhinoscopic examination may reveal redness and swelling of the mucous membrane of the nose; and it is often useful to reduce this swelling by the application of cocaine and adrenalin, so as to allow of a better examination. Polypi may be present, and may have to be removed to allow of a proper view; and, for the same reason, it is sometimes advisable to remove the anterior part of the middle turbinate. Such measures assist the examination, and serve often to aid the preliminary treatment.

Maxillary Antrum.—Pus can be seen in the middle meatus anteriorly; on wiping away the pus, and making the patient hold his head over so that the suspected side is uppermost, the pus will be seen to reappear. What is known as Fränkel's test may also be tried: this consists in cleaning out the nose, leaning the head forward so that the forehead is down for a few minutes, and re-examining. The reappearance of pus is in favor of the antral source. This test is not conclusive, however, Greville MacDonald stating that this posture test does not exclude frontal sinus suppuration. Sometimes, where accessory ostia exist, little or no pus may lie in the middle meatus, but may drain back into the naso-pharynx.

Trans-illumination, the details of which are too well known to require mention, is of value when considered in combination with other symptoms and physical signs. Taken by itself it is not an absolutely reliable diagnostic phenomenon. Turner has pointed out how non-illumination of the antrum is due more to infiltration and thickening of the mucous membrane of the antrum than to the pus contained therein, and likewise how the amount and consistency of the pus bear no relation to the density of the shadow. (I was much impressed once by a case where, with typical non-illumination, there was only a suspicion of pus present.)

Pressure and percussion may reveal tenderness over the cheek or gum, or diseased teeth may be found in the antral region of the mouth.

Exploratory puncture may be made through the socket of a carious tooth, the canine fossa or the inferior meatus of the nose, and is really the only single positive means of settling the problem. It will be seen, therefore, that a careful consideration of symptoms and physical signs will usually allow of a reasonably certain diagnosis being made. Briefly stated, a unilateral purulent nasal discharge, with the pus lying in the middle meatus of the nose, and answering to the posture tests, is sufficient reason for diagnosing antral disease and for positively proving it by an exploratory puncture.

Frontal Sinus.—Rhinoscopy may reveal much the same condition as in the antral cases; and, as has been seen, posture tests are not absolutely diagnostic. Pressure may reveal tenderness over the floor of the sinus. Trans-illumination is of little or no practical value; as Turner has clearly shown, it is the exception rather than the rule to find opacity of the diseased sinus, and even where there is absence of illumination it may be due to absence of the sinus, to a thick-walled sinus, as well as to disease of the cavity. In doubtful cases, therefore, it is advisable to open the antrum. If there is no pus in it we can eliminate the antrum; if, however, it contains pus, after syringing and carefully cleansing the antrum, we examine the nose in order to see whether pus is running into the middle meatus. If this is the case the frontal sinus is probably involved. Frontal sinus suppuration, however, is complicated often, not only with antral but also with anterior ethmoidal suppuration. Suppuration of the anterior ethmoidal cells is frequently associated with frontal and

antral disease. The removal of the polypi, which are often present, and of part of the middle turbinates, with careful use of the probe and curette to remove any carious bone, will enable the source of the pus to be detected.

Posterior Ethmoidal Suppuration.—In every unequivocal case, according to Greville MacDonald, the pus does not flow over the posterior naso-pharyngeal wall—which, indeed, can only happen if the patient lies on his back; it may be seen in the superior meatus, between the middle turbinate and septum. According to other authorities it often may be seen in the post-nasal space; and it must be remembered that the pus may find its way into the middle meatus.

In sphenoidal disease the pus obviously flows over the posterior naso-pharyngeal wall (G. MacDonald), while the only position in which it gains access to the nasal fossæ is with the patient lying prone or bending the head forward.

Other rhinologists refuse to accept these views, and it is, I think, quite possible for pus to find its way into the superior meatus. The discharge is sometimes liable to dry and form crusts, and cases may be confused with naso-pharyngeal catarrh, complicated by crust formation.

An important point to keep in mind in the diagnosis of sinus suppuration is the frequency with which more than one sinus is affected, as it tends to further complicate an already complex subject. It is only by a careful consideration of symptoms and physical signs (it is often advisable to submit the patient to repeated examinations), and, if need be, by a process of exclusion of different sinuses by exploration, that an accurate opinion can be arrived at.

Treatment.—In the treatment of chronic suppuration of the maxillary antrum, two methods may be followed:—(1) Treatment by lavage; (2) the radical operation.

Treatment by lavage consists in the opening of the sinus through the socket of an extracted tooth, followed by a continued course of irrigation of the antrum by various antiseptic and astringent lotions; any nasal or dental complications, such as polypi or caries, being attended to. This lavage may have to be continued for weeks or months, and in the simpler cases may give good results; but where complications are present—such as nasal polypi, antral polypi, diseased bone, &c.—or where the disease is of long standing, the radical operation is to be preferred. Into the details of this and other operations I do not propose to enter, as they are too well known to you to require mention; suffice it to say that a very free opening should be made sufficient to admit of exploration by the finger, and that all diseased bone, membrane, and polypoid growths should be carefully removed by the curette.

The variation in the size and shape of the antrum, due to the varying degree of approximation of facial and nasal walls and of absorption of cancellous bone during the period of growth, must be kept in mind in these operations, as well as the position of the infra-orbital canal with its contents, and the presence of the recess above the canal, where disease may escape the operator's attention. The radical operation gives good results in many cases, but occasionally fails to cure the disease. Amongst others I may mention one case where four, if not five, operations had been performed by different surgeons, with years of intermittent applications and dressings; and when the patient died, ten years afterwards, of another disease, the antral trouble still existed.

The operation through the mouth with a counter-opening through the inferior meatus of the nose (Luc), with or without the subsequent suture of buccal opening, seems to be of value in some cases. The possibility of disease of other sinuses must be kept in mind, as failure may result through the implication of these sinuses, and the reinfection of the antrum.

Frontal Sinus Suppuration.—All nasal disease must be removed, and, in addition, it is advisable to cut away the anterior portion of the middle turbinate, to allow of freer inspection, access, and drainage. Some surgeons recommend that, after this has been done, the diseased ethmoidal cells be broken up by the curette or forceps, and the infundibulum so enlarged. A frontal sinus cannula can be subsequently passed, and lavage carried out. The passage of the cannula is often easier in theory than in practice; even if practicable, treatment by lavage has to be carried on for a prolonged period.

In chronic cases this treatment may fail, and the question of external operation arises. It must be remembered, moreover, that suppuration of the frontal sinus is not altogether free from risk—septic meningitis, inter-cranial abscess, orbital cellulitis, &c., being possibilities; although what percentage of cases develop dangerous complications is, as far as I am aware, unknown. The fact that the external operation may leave a more or less marked scar must also be kept in mind, and must be explained to the patient—more especially if that patient is a woman.

The supra-orbital incision is perhaps the best. If, on opening the sinus, the lining membrane seems healthy, it may be sufficient to wash out the sinus and effect free drainage; should, however, the membrane be diseased, more of the anterior wall must be removed, and the sinus wall carefully curetted. A probe is to be passed from the sinus into the nose as a guide, and the anterior ethmoidal cells removed by the curette; by this means the fronto-nasal passage can be enlarged, and made to allow of free drainage. The external incision is not to be closed completely, but room left for the end of a gauze packing placed in the sinus.

Numerous modifications of the operation exist—thus, a vertical incision is proposed by some, a median by others. Luc completely closes the external incision, and brings the end of his gauze packing into the nose: Jansen opens the floor of the sinus by an incision below the supra-orbital margin, and claims better access to the ethmoidal cells as the advantage of his method. Another advantage of Jansen's method is the absence of risk of opening into a healthy sinus of the opposite side, where the septal obliquity is marked. Kuhnt advocates complete removal of the anterior wall; which may be possible in small sinuses, but would cause great deformity if the sinus was large. Milligan suggests that, in uncomplicated cases, the infundibular passage be closed by means of ivory nails, thus cutting off all communication with the nose. The frontal sinus thus becomes a bony cavity communicating with the air by the supra-orbital incision only, and may be allowed to granulate up.

Skin-grafting, after the methods employed in mastoid cases, seems to me to be possibly a valuable adjunct to some of these operations on the frontal sinus. Whether they have been tried, I am not aware; if not, they are worth trying.

It is important to bear in mind in all operative work in the frontal sinuses their very variable extent and shape, the septal obliquity which may result in the wrong sinus being opened, the imperfect partitions which may subdivide the sinuses and produce irregular recesses and pockets, where disease may escape notice.

Into the details of treatment of ethmoidal and sphenoidal disease I shall not enter, as Dr. Kirkland deals with them in his paper. Suffice it to say that it consists in removing any polypi which may exist, together with the middle turbinate, then carefully curetting and punching out the diseased bone, so as to allow of free drainage; and that any operative measures in the ethmoidal or sphenoidal cells should be undertaken with the greatest care.

It will be seen from my preceding remarks that the treatment of suppuration of the necessary sinuses is hedged round by not a few difficulties. Patients,

therefore, should not be buoyed up with too confident assurances of speedy cure, for, in the words of Milligan, the intricate arrangement of the sinuses, the difficulty encountered in securing at any one operation a complete eradication of disease, and of maintaining free drainage, together with the risks of reinfection, are factors which militate in no small measure against a successful issue.

Dr. Barrett said that the discussion on the treatment of empyema of the maxillary antrum had interested him extremely, as, for some reason or another, he had had a very large number of these cases pass through his hands. What had most impressed him was the fact that empyema of the maxillary antrum, discharging foul pus into the nose, might exist for months, or even years, without causing any very grave symptoms. Poor health it certainly caused, digestive troubles, loss of weight; but he had not been able to discover one case in which a fatal result could be directly attributed to it. It had been brought home to him in this way, that he had urged patients to have an operation done, when they declined to have anything whatever done—even washing out through the nose. They had continued to follow their business, and, so far as he was aware, none of them had lost their lives. The operation he usually practised in the first instance was to drill a hole through the alveolus—preferably of the second or first molar, with a cylindrical saw, driven by an electrical motor or a dental engine. The operation did not usually require any anæsthesia, and, if the appliances were in good order, took but a few seconds to complete. A cylinder of bone was removed, and the antrum could be readily washed out. Treated in this way, the bulk of the cases got perfectly well, though the recovery was sometimes very protracted; but the gain in weight and general health immediately following the washing out was very striking. The position then reached was this:—If the antrum is being regularly washed out, and the discharge does not disappear, undoubtedly the right course is to explore the antrum through the canine fossa, and to perform one of the various so-called radical operations. But many patients, realising that they were running no risk whatever, and were suffering nothing but the inconvenience of washing out, declined to have any operation performed, and one could not in fairness indicate to them that they were taking any serious risk in doing so.

He had pushed the radical operation as far as he could, usually removing the whole of the anterior wall of the antrum, down to the floor and to its anterior edge, then making an opening through the inner wall through the thick bone at the bottom of the partition of such a size that a finger passed into the nostril and a finger passed through the opening met, the corresponding portion of the middle turbinate being removed. And yet he had found that after such a radical operation as this, with complete curetting of the antrum, some amount of discharge frequently continued, exaggerated by any catarrh, nearly disappearing in between. He had further noticed that, immediately after a radical operation was performed, in which the whole of the lining membrane of the antrum had been carefully curetted, trans-illumination still gave dulness on the affected side. All this seemed to indicate that by the time an antral empyema was fairly set up the changes in the mucous membrane and the underlying bone were tolerably profound, and he doubted very much whether in a marked case a complete restoration to normal condition was ever attained by any surgical method. Such physiological restoration, however, was not of very great importance. As long as it was drained, no risk apparently existed. The practice he had indicated seemed to him to be the sound one: first an opening through the alveolus, and, in obstinate cases, the radical operation.

Dr. S. A. Ewing (Melbourne)—The many difficulties of successful treatment of chronic suppurative diseases of the accessory cavities of the nose

emphasize the importance of early treatment of acute suppurative diseases of these cavities. Such conditions are not infrequently found as sequelæ of influenza and infectious diseases in which the mucous membranes of the nose are involved. Sprays of weak solutions of adrenalin, in order to widen the openings of the cavities, followed by appropriate posture for drainage and use of a saline nasal douche, are often sufficient to effect a cure in acute cases, and are of great service in chronic cases where a radical operation is inadvisable. In chronic suppurative disease of the sinuses removal of obstacles to drainage, such as polypi and anterior part of the middle turbinal, often yields good results.

In radical operations on the maxillary antrum, the opening into the nose should preferably be situated on a level with the floor of the nose ; this favors drainage in the erect position.

Lambert Lack's operation for ethmoidal sinus disease was not devoid of risk ; he had known of one death, from fracture of the cribriform plate and subsequent meningitis. As acting House Surgeon at Golden Square Throat Hospital he had assisted at these operations, and had known of no such accidents under Dr. Lack.

Sphenoidal sinus disease can generally be effectively treated by removal of the middle turbinal where the opening of the sinus cannot be found, and washing it out by means of a long channeled tube. Diagnosis of suppurative conditions in the nose occasionally presents difficulties. He recently, in a case where an operation had been performed on the frontal sinus, found polypi and pus in the ethmoidal region; sections of the polypi showed that they were innocent. On performing Lack's operation on the ethmoidal sinuses, large masses of growth were removed which proved on examination to be carcinomatous.

Dr. Herbert Marks (Sydney) said I do not altogether agree with the views expressed by Dr. Barrett and Dr. Arthur with regard to non-interference in the case of chronic suppuration of the maxillary antrum—that is, by substituting a drain only through the alveolus without removal of the diseased lining membrane of the antrum, the cause of the trouble. It is true that we cannot promise a complete cure in every individual case ; but still many cases are completely cured by the radical operation, and we are perfectly justified in performing the complete operation and doing our best for our patients, in the hope that the cure will be permanent. These cases must, or should, be attacked on general surgical principles ; and I advocate in every case the radical operation with drainage through the inferior meatus of the nose, removing a portion of anterior end of inferior turbine if necessary.

With regard to ethmoidal disease, I agree with other gentlemen who have spoken, that Lack's method of operation is to be condemned. His operation is unnecessarily heroic and dangerous. Whatever is necessary can be done by cutting forceps, such as Grunwald's, and the careful use of a small curette. If the nostril is too narrow, the condition may be attacked from the outside, as in the common operation for frontal sinus disease.

The ideal operation in chronic frontal sinus suppuration is to clean out the sinus and establish as complete drainage as possible into the nose, and to close up the external wound ; but drainage, as a rule, is so uncertain—and having seen Dr. Herbert Tilley's case of septic osteomyelitis, which proved fatal, I always feel it safer to pack the sinus with gauze through the lower angle of the wound, and gradually remove it as healthy granulations appear. This procedure gives very satisfactory results in the majority of cases.

The President, commenting on the debate, said the members were much indebted to the gentlemen who had opened the discussion. With the majority of Dr. Kirkland's conclusions he was in agreement, but he could

not indorse the opinion of the dentist, as quoted by Dr. Kirkland, that "as a rule the teeth died and decayed after opening the maxillary antrum in the canine fossa." This did not accord with his experience. Such a result would practically condemn the operation. As had been pointed out, the dead teeth would be liable to re-infect the antrum. To avoid injury to the nerve supply of the teeth, the opening should be made in the thin bone of the canine fossa, and the alveolus and the roots of the teeth should not be encroached upon. He could not agree that we should not operate in cases of suppuration of the frontal sinuses except we were sure of a cure. It was in the worst cases—where we could not make sure of a cure—that an operation was most required, and we ought to give the patient a chance, if not of a cure, of alleviation of his symptoms. In his earlier operations he had frequently succeeded in curing frontal sinus suppuration by a very simple operation ; but increased experience had led him to treat this disease with more respect. He had come across cases where, on account of anatomical peculiarities in the sinus, the cure was very difficult. The rule which he now followed in chronic cases was, after turning back soft parts and periostium, to completely remove the anterior wall of the sinus, and to carefully remove all traces of polypoid lining membrane. In a few cases, after separating the pulley of the tendon of the superior oblique muscle from the bone, he had removed enough of the inferior wall of the sinus to allow the orbital fat to rise into the same. It is claimed that this tends to a more rapid filling up of the cavity and consequent healing. In the few cases where he had used this method there was no permanent disturbance of vision. The after-treatment consisted in packing with gauze—two or three weeks—till the cavity was lined with healthy granulations : it was then allowed to close, and a pad and bandage assisted in bringing the two walls together. The deformity following removal of the whole of the anterior wall of the frontal sinuses was not so great as might be anticipated. In the case of small sinuses the sinking was hardly noticeable ; where the sinuses were very large the patients had prominent brows, and the operation produced a flattening which did not greatly exceed the normal condition. The incision which he used where both sinuses were involved was carried under the line of the eyebrows, and across the root of the nose in the natural crease which is there. This allowed a flap to be turned up, giving plenty of room, and, when healed, was not disfiguring.

Regarding Dr. Lack's method of dealing with the ethmoidal cells in cases of suppuration and polypi, they would remember, when this method was introduced, it was looked upon by rhinologists with a good deal of alarm. He thought the operation was not so dangerous as it appeared. In one very bad case of nasal polypi, and profuse discharge of pus, the procedure was followed by a septic osteomyelitis ; this was the only case in his experience where a bad result had followed the operation. In some cases the removal of the diseased ethmoidal cells was the only means of cure. We had to choose between Lack's method and the slower one of working with Grunwald's forceps. In Lack's method the attic of the nose was to be avoided.

Dr. Barrett's case, where the radical operation on the antrum failed to cure, as there was a mucopurulent discharge after every cold, could hardly be used as an argument against the operation, as people with healthy noses were so affected after a cold. He mentioned a case which he had treated, where the radical operation failed to bring about a cure ; subsequently he found the prong of a tooth imbedded in a mass of granulation tissue on the posterior part of the floor of the antrum. The removal of this was followed by a permanent cure. The failure of antral operation was generally due to some complication—as ethmoidal or frontal sinus suppuration.

NASAL AND NASO-PHARYNGEAL FACTORS IN CHRONIC MIDDLE EAR CATARRH.

By S. A. Ewing, M.R.C.S., Eng., D.P.H., Cantab.,

In the first place, in treating this subject on which so many conflicting opinions are and have been held, it will be useful to make some preliminary reference to the term chronic middle ear catarrh. The term catarrh is not a happy one, since, as a general rule, excess of secretion—generally accepted as the chief criterion of catarrhal diseases—is not evident in the cases classed under this category. From this point of view it would seem preferable to describe chronic catarrhal disease of the middle ear as a group of dry non-suppurative diseases of the middle ear; but, though in reference to the condition of the middle ear, deductions have to be drawn from the state of the tympanic membrane, an examination of the eustachian tube—which is practically an extension of the middle ear—usually reveals evidences of catarrhal changes. This, I think, is sufficient justification for adherence to the older descriptive title.

From the deficiences in our knowledge of chronic catarrhal diseases of the middle ear, a satisfactory definition is difficult, and must be more or less conjectural. That given by Lake (1) is generally applicable. Lake states that chronic catarrhal otitis media is a progressive, insidious, and sub-inflammatory affection of the middle ear, consequent upon an extension of so-called catarrhal inflammation from the mucosæ of the naso-pharyngeal space, which involves, firstly, the eustachian tube, and eventually the middle ear. Exceptionally, however, cases occur in which the tympanic evidences of chronic middle ear catarrh alone are present.

I am leaving out of consideration many forms of middle ear deafness which are often erroneously classed as chronic catarrh; typical of these is the deafness, generally intractable, resulting from adhesive processes supervening on neglected acute and sub-acute exudative catarrhal attacks. Also another class of cases, which Politzer has named "otosclerosis," in which we now know—largely owing to the pathological labors of that distinguished otologist—that bony changes affect the stapes and labyrinth; though, owing to the difficulties in diognosing this condition, especially when co-incident with evidences of catarrhal processes in the ear or naso-pharnyx, differentiation is not always possible.

In studying cases of chronic middle ear catarrh, the larger number can be naturally grouped into two classes:—Firstly, those in which the catarrhal inflammation of the nasal and naso-pharyngeal mucosæ, inclusive of the eustachian tube and middle ear—and, probably, in some cases, of the middle ear alone—is independent in the beginning of any other local abnormality or disease. Secondly, those in which the chief factor is local, and the catarrhal condition of the mucosæ secondary to this. Such local conditions as excess of pharyngeal lymphoid tissue, hypertrophies, tumors, irritants, disease of accessory sinuses of the nose, septal deformities, and generally any cause leading to nasal or naso-pharyngeal obstruction or irritation.

This classification is of importance from the point of view of prognosis, which, generally, is better in the latter division, as the rectification of a local condition is more within our power than a dyscrasia, which may be the inheritance of generations. The age incidence is also noticeable; in the first division the onset of chronic middle ear catarrh is chiefly in early adult and middle age, in the second at any period from childhood to old age. Statements of patients are rarely to be relied upon in reference to onset of deafness.

Ostmann, as the result of a series of examinations with his objective audiometer, found that a diminution of from one-third to one-half of the hearing is regarded as of slight consequence ; and frequently it is only on the supervention of tinnitus in the early stages, or marked deafness in the later, when irreparable damage has been done, that the patient is seized of his condition.

In connection with secondary catarrhal disease, striking differences are manifested in different individuals. In one, for example, there may be marked reaction to the presence of a septal deflection ; in another, the resultant irritation is at a minimum. The influence of local abnormalities is most marked when there is lowering of bodily tone, and tends to establish chronicity in acute rhinitic attacks, such as are associated with colds and certain infectious diseases.

It is not always possible to draw a hard-and-fast line between primary and secondary catarrhs. For example, as the result of the continuance of a primary catarrhal condition of the nasal mucosa, hyperplasias result ; these, by increasing the nasal obstruction, tend to increase the catarrh. In this way a vicious circle is established, which, unless broken, often leads to grave local and constitutional disease.

In the consideration of the onset and cause of catarrhal disease certain pathological and physiological influences require attention. Gouty tendencies and plethora are often associated with congestive mucosæ, and the influence of diet—more especially in the above states—is often very definite. Perverted sexual states are not infrequently reflected in congestion of the nasal mucosa. Puberty and pregnancy occasionally cause a similar condition. I have seen several cases of partial nasal obstruction developing in the third and fourth month of pregnancy, progressing till childbirth, and then subsiding afterwards. The tendency in youth to growth, in age to atrophy, requires to be remembered.

Mayo Collier (2) has recently drawn attention to what he has named latent or intermittent nasal obstruction, in which the nasal passages are partly occluded during sleep. As Dr. Collier points out, statements of patients are rarely to be relied upon when they are questioned as to mouth-breathing. He relies upon the presence of a groove upon the septum opposite the inferior turbinals. Though this is sometimes present, a better guide is to ask the patient to test each nostril on waking, before rising, and at short intervals on assuming the erect position. Each nostril should be alternately closed with light pressure, and the air gently inspired through the free nostril : this should be contrasted with the breathing before lying down. Where nocturnal obstruction is well marked, a hypertrophic condition of the posterior ends of the inferior turbinals and adjoining part of septum is almost invariably present.

Dr. G. Oliver (3), in a paper entitled "Recent Studies on Tissue Lymph-circulation," affirms that, as a result of many investigations, the venous pressure rises to a high point during sleep ; whereas, during the waking states, the venous pressure is as a rule only about one-fifth of the arterial pressure, in sleep it rises to a little over half. This venous rise of pressure may play some part in causing nocturnal obstruction.

Gravitation is also a factor, for patients frequently answer on inquiry that the nose is only blocked on the side they lie on in sleeping ; and, in some, rest in a horizontal posture for a short period will cause partial obstruction. The obstruction sometimes persists for some time after rising, disappearing some hours later ; this may be due to transudation of lymph into the tissues of the mucosæ ; possibly the diseased state of the vessels in hypertrophic conditions of the nasal mucosæ may alter their permeability. Nocturnal obstruction is more marked where there is ill health, or want of tone ; it is not uncommon in overworked city men, and, in such, frequently disappears on sending them away for a rest.

How do nasal and naso-pharyngeal conditions lead to catarrhal changes in the ear ?

It is still a matter for discussion whether there is any direct connection between the nose and the ear apart from extension by way of the eustachian tube. Politzer (4) states that "after the cauterisation of the inferior turbinal a decrease, rarely a complete cessation, of the subjective noises in the ear follows ; from this fact it may be concluded that there is a connection between the turbinated bones and the ear. Up to the present such a connection has not been discovered." Dench (5), relying on Zuckerkandl's observation of the communication of the venous sinuses in the eustachian tube with those of the posterior ends of the turbinals, states that in cases where eustachian tube remains patent we must conclude that the turgesence of the turbinated bodies interferes with the venous return current from the labyrinth ; but it should be remembered that patency of the eustachian tube to instrumental methods is not necessarily a patency that secures ventilation of the ear under normal conditions.

Support would seem to be given to Dench's contention by the fact noted by some observers that decrease in hearing, during a cold, is present in some patients upon whom the radical mastoid operation has been performed. I have only noticed this decrease where the eustachian opening into the middle ear has remained patent, and some extension of the catarrhal condition present in the tympanic wall. Whatever doubt there exists in reference to direct connection between the nose and ear, there is none in regard to the dependence of the middle ear upon the integrity of the eustachian tube. In considering this portion of the auditory apparatus, it is important to remember the variations in the position of its opening, length of the isthmus, and patency. In children, and adults in whom the naso-pharynx is small, adenoid growths play a more important part in setting up catarrhal changes in the eustachian tube than in adults with normal-sized naso-pharyngeal spaces. The roomy naso-pharyngeal space, and the adhesion of crusts to the vault of pharynx of ozœna patients, may explain their comparative immunity from ear involment. In adults the eustachian tubal opening is continually immersed during the action of swallowing in the bacteria-sodden secretions that collect in the back part of the inferior nasal strait, and on the roof of the soft palate in catarrhal diseases of the nose.

Charles J. Bond (6), in a recent contribution on ascending currents in mucous canals and gland ducts, and their influence on infection, states that, after insufflation of indigo particles into the naso-pharynx in a case of suppurative otitis, these were found a few days afterwards in the discharge from the ear. Though this has not been proved where the tympanic membrane is intact, sufficient evidence is brought forward by Bond, in his researches on other mucous passages, to establish the probability of the transference of catarrhal infection in this way from the naso-pharynx to the eustachian tube and middle ear. A similar transference probably results from continuity of structure, and from forcible expiratory efforts. That the presence of irritant secretions in proximity to the ostia of the eustachian tube is a powerful factor in the causation of secondary catarrh of the middle ear is shown by the temporary improvement resulting from washing these away, by the use of the nasal douche, and the permanent relief resulting from successfully removing the cause of these secretions. The influence of nasal obstruction requires consideration. This may be complete without any apparent change in the ear.

I examined last year, at the Victorian Eye and Ear Hospital, a patient, age 24, in whom both nares as far back as the choanæ were completely occluded by large polypi, the naso-pharynx was slightly hyperæmic, but both ears were

normal, and the hearing above the average. Such obstruction generally leads, sooner or later, to aural disease. Cases of unilateral obstruction, more or less complete, are not uncommon. In such, chronic catarrh is frequently met with, and usually the ear that is affected is on the same side as the free nostril; where both ears are involved, the ear on the obstructed side is generally affected last. Tested by the mercurial manometer, the negative pressure behind the obstruction is the same in inspiration as in the free nostril. Examination of the anterior nares shows that the inferior turbinal on the freer side of nose is invariably hypertrophic: examination of the posterior nares generally discloses excessive posterior turbinal hypertrophy on the free side, with excess of mucus secreted by it in the immediate vicinity of the eustachian tube of the same side. During the examination, if the patient gags or swallows, the mucus will be seen adherent to the eustachian opening. Where the mucus is abundant, the opposite eustachian orifice is similarly affected. Removal of the hypertrophy and the catarrh in these cases checks the progressive loss of hearing.

Nasal obstruction, when partial, causes a great increase in the negative pressure in the nose when forced nasal inspiratory efforts are made. This increase in negative pressure, owing to the hyperæmia produced, is one of the disadvantages of the system advocated by Arbuthnot Lane, of nasal breathing exercises in cases of nasal obstruction associated with adenoids. Nasal obstruction causes a marked increase in the negative pressure in the naso-pharynx during deglutition; during this act the eustachian tube is open, and after a slight rise a marked fall of pressure occurs in the middle ear. This explains the retraction of the tympanic membrane in some cases of deafness, and also the sudden increase of deafness during eating where there is nasal obstruction.

Reference has already been made to the conveyance of infection to the middle ear by the eustachian tube. The effect of obstruction of the eustachian tube is to set up negative pressure in the middle ear; this leads to impairment of function in two ways—firstly, by its effect upon the vessels of the tympanic mucous membrane, causing hyperæmia; and, secondly, by its effect upon the tympanic membrane, causing retraction.

It has been estimated that 20–40mm. of mercury is the average pressure necessary in normal conditions in Valsalva's experiment to open the eustachian tubes, while, with a slight swelling of the mucous membrane, a rise of 100-120mm. of mercury is required.

Normally the establishment of ærial equilibrium between the middle ear and the naso-pharynx is involuntary, and is independent of great pressure, and any interference with the maintenance of this ærial interchange is apt to disturb the integrity of the ear. Forcible methods of inflation of the middle ear, by Valsalva's method, or by Politzerisation, are often relied upon in order to determine the functional patency of the eustachian tube. Politzer states the amount of pressure used in his method of inflation varies from 0.1 to 0.4 atmospheres and over. Such tests are fallacious, as these high pressures are only present in the naso-pharynx during strong expiratory efforts.

The mode of treatment by inflation of the middle ear in chronic middle ear catarrh, which is dependent upon catarrhal conditions that are remediable, has many disadvantages. Temporary relief is sometimes gained, but the rapid re-absorption of the air in the tympanic cavity requires repeated renewal of the inflation. The catarrhal disease of the ear meanwhile goes on unchecked, and the continual forcible distension of the tympanic membrane leads to permanent stretching of its upper portion, rendering it less capable of transmitting vibrations.

In some cases of chronic middle ear catarrh the temporary improvement in hearing caused by successful inflation is rendered permanent on removing

the causes of the catarrh. In other cases the improvement resulting from removal of the catarrhal causes is greater than the temporary improvement got by inflation. In a third class, where inflation of the middle ear gives no improvement in hearing, yet improvement—sometimes marked—may result upon removing the catarrhal conditions affecting the ear. This I have only found where the catarrhal causes are excessive.

I have endeavored in this brief sketch to indicate some of the ways in which catarrhal processes are set up in the nose and naso-pharynx, and the manner in which they affect the ear, and also the importance of the nasal passages in reference to the production not only of naso-pharyngeal catarrh, but also of chronic catarrh of the middle ear.

References.

(1) "Handbook of Diseases of the Ear," p. 110.
(2) *Lancet*, February 12th, 1904.
(3) *Lancet*, May 7th, 1904.
(4) "Handbook of Diseases of the Ear," p. 671.
(5) "Diseases of the Ear," p 681.
(6) *Lancet*, July 29th, 1905.

SECTION OF PATHOLOGY, ANATOMY, PHYSIOLOGY, AND THERAPEUTICS.

SOME RECENT OBSERVATIONS ON CANCER AND TUMOR GROWTH.

By D. A. Welsh, M.A., B.Sc., M.D.

Of all the great scourges of mankind, only one remains untempered by that scientific study of disease which we term Pathology. Smallpox, except in isolated outbreaks, is a thing of the past. The pyæmia that was wont to render perilous every surgical operation yielded to antiseptic and aseptic measures. Diphtheria and hydrophobia have been robbed of half their terrors. Bubonic plague, despite the colossal failure of India, may be successfully combated in an intelligent community. Outbreaks of enteric fever and of cholera are in every sense of the word preventable. The prevalence of malaria, with its long tale of disablement and death, is conditioned by the facilities provided for the breeding of mosquitoes. And that tuberculosis should continue to be of all diseases the most wasteful of human life is a fact that is not creditable to us as a profession, nor to us as a people, for it is due wholly to the disregard of what pathology with no uncertain voice has taught. But cancer and tumor growth are as rife to-day as ever. If there be any change, it is probably not in the direction of abatement; and, even if there be abatement, pathology has contributed nothing to it. No means of prevention is known, nor certain means of cure. The only relief—that afforded by careful, free, and prompt excision—is at the best uncertain,

No problem in pathology, therefore, is more urgent, or more elusive, than that of the nature and origin of tumor growth, and within recent years there has been awakened interest and renewed activity in its investigation. Under the gracious patronage of His Majesty the King, the Imperial Cancer Research Fund has been inaugurated on a scale unprecedented in the history of medicine, while the able workers who are more directly responsible for its scientific application have made excellent use of their unique opportunity. Elsewhere in Europe and in America many similar institutions have been started, and under their auspices, as well as independently, much good work is being done. The result is that since the last meeting of this Congress, when papers on cancer, of great interest and value, were read by Professor Allen and other members, there have been published observations and hypotheses of a most noteworthy and suggestive character. That as yet their full significance is uncertain, and their practical bearing unknown, does not detract from their present interest as illuminating conceptions, nor from their permanent value as fresh scientific truths. It is with the object of inviting your attention to some of this recent work that I have framed my address, although the time at my disposal will necessitate a very fragmentary review.

It is unnecessary to introduce subtleties of definition or to discuss the relation of tumor growth to those conditions known as inflammatory. I have throughout used the words tumor, new growth, and neoplasm as interchangeable, and the word cancer as connoting all the more malignant forms, irrespective of structure. I have occasionally used a general term instead

of the more restricted term cancer or malignant growth, because, in my judgment, any explanation of the nature and origin of the most malignant forms (cancer and sarcoma) must be ultimately applicable to the least malignant (benign) forms also. The varying degrees of malignancy and the various factors that condition malignancy are mainly dependent upon attributes that are common to all neoplasms, though not always present in the same degree. The distinction between a malignant and a benign tumor, all-important in clinical practice, depends not on any essential difference in kind, since all neoplasms alike are independent of the needs of the parent organism, and independent of the laws that regulate the growth of the parent tissue, and exhibit, moreover, a certain mutual independence and intrinsic vitality in their constituent cells. The distinction depends rather on the degree to which are developed these fundamental attributes, and also depends upon such accidental conditons as the ease with which the individual cells may be mechanically detached from the tumor mass.

ARTIFICIAL PRODUCTION AND TRANSPLANTATION OF NEW GROWTHS (NEOPLASMS).

Half a century ago the scientific genius of Virchow laid the foundation of modern experimental pathology in two general biological principles—(1) that every cell in the body is descended from a pre-existing cell, and (2) that the reactions of the living cells of human tissues are essentially similar to the reactions of other animal cells—so that it is legitimate to argue from what occurs in animals to what occurs in man. Both of these principles became of fundamental importance in relation to cancer research, since it became known that animals, too, suffer from new growths of which the structural details are often identical with those of tumors occurring in man. In view of these principles and facts it has long been recognised that the artificial production and transplantation of tumors in animals was essential to the proper study of neoplasia, or tumor growth.

To this end innumerable experiments have been made, and, when investigation flagged under the deterrent influence of consistently negative results, a fresh stimulus was given by the development of bacteriology. The resemblance between the invasion of a cancer and other infective processes which had been compelled to yield the secret of their origin fostered the expectation that the cause of cancer also would be found in some form of microbe. Many different parasites were described within cancer cells, several were obtained in pure culture, and a few inoculated into animals gave rise to cell-proliferations somewhat resembling tumor growth. But such resemblances were only superficial and misleading. Notwithstanding the great amount and the occasionally brilliant character of such work, it must be conceded that no one has yet been able to originate an unequivocal new growth by the agency of any parasite.

When healthy tissue is transferred from one animal to another, the graft may " take " or may fail to " take." The probability of success is greater when the graft is made into another animal of the same species. Such an artificial graft may form a hypertrophic overgrowth when transplanted from a less vascular to a more vascular site, but it never acquires the independent and unregulated growth characteristic of a tumor in the sense in which we are using the term. The oft-quoted experiment of Lambert Lack, who in 1900 disseminated the ovaries of a rabbit throughout its peritoneal sac, and more than a year afterwards found an extensive cancerous infiltration of the abdominal and thoracic viscera, is open to other interpretations. This particular experiment, moreover, has been many times repeated with invariably negative resnlts. In July, 1903, I instituted a series of trans-lantations of

epithelial cells from the chorion, ovary, thyroid, adrenal, and epidermis in such animals as guinea-pigs, rabbits, cats, and dasyures, the transplantations being made into other parts of the same animal, into other animals of the same species (of the same and of opposite sex, and at different ages), and into animals of different species; but, with one doubtful exception, I succeeded only in adding another to the long list of failures to produce tumor growth by this means. The exceptional case was that of a guinea-pig into whose peritoneal sac chorionic epithelium from another guinea-pig had been introduced, and which later died, having in its pelvis a large mass of recently extravasated blood in which proliferating chorionic epithelium was found.

When introduced into animals of different species, the transferred cells are usually, though not invariably, destroyed by cytolysins, the living tissues reacting towards them as towards other alien albuminous molecules. Occasionally, however, the alien cells are tolerated, and may even be adopted as integral portions of the new organism. Blood corpuscles, for example, do not invariably undergo hæmolysis when transfused into animals of different species, and grafts of epidermis from the rabbit and the puppy may thrive on denuded surfaces in man. It is important to recognise these facts, since they indicate no reason why cancers should not occasionally be transmissible to alien species. It is known that epitheliomata tend to arise in the cicatricial tissue of old burns and in other scars, and it would be interesting to ascertain whether the liability is greater when no grafts have been made, or when grafts have been taken from another person of earlier or later age, or from animals whose term of life is shorter than that of man—whether, in fact, the presence of an epithelium of a different tenure of life has any relation to the incidence of epithelioma.

The secondary dissemination (metastasis) of certain malignant new growths may be regarded as a process of natural transplantation, and illustrates the possibility of transferring such tumors to other parts of the same individual. That this may be accidentally effected in the excision of a malignant growth has been repeatedly emphasized. If a cancer be incised during removal, cancer cells may escape from the cut surfaces, or be carried by the knife into adjacent tissues, and, becoming there implanted, give rise to a local recrudescence. Moreover, a growth of restricted malignancy may accidentally be given an opportunity of extensive infiltration, as when a cystic adenoma of the ovary is ruptured and the liberated epithelial cells infect the peritoneum.

Since, therefore, a natural or artificial dissemination of certain tumors is so readily effected in the individual in which they originally grow, it might be expected that such growths would bear transplantation at least to other similar individuals. Such an expectation is far from being realised. Human new growths have never to my knowledge been transmitted to other animals, notwithstanding many attempts, of which a most extensive series was undertaken by Shattock and Ballance in 1890. Nor am I aware of any authentic instance of the successful transference of a new growth to an animal of different species. Even when inoculations are made into animals of the same species, except in the case of certain infective granulomata which are not true neoplasms, the usual result is failure, and at the date of the last meeting of this Congress, in February, 1902, only a few isolated successes had been recorded. Obviously such a record of failure has constituted a serious bar to the investigation of tumor growth.

The first to achieve systematic success in the transplantation of tumors was Jensen, of Copenhagen, whose results were not made generally known until 1903. In the same year Borrel, of Paris, published another series, and in 1904 there appeared the first scientific report of the Imperial Cancer Re-

search Fund, in which further examples were recorded. All these successes were obtained by transplanting tumors of mice to other mice. In no other animal, with the single exception of the rat, which is of the same genus as the mouse, has there been effected a successful transplantation of a neoplasm, even from another individual of the same species. Attempts to transfer malignant new growths from the horse, dog, cat, rat, and carp to other animals of the same species have at the hands of the Cancer Research Fund resulted in failure, although in the case of the dog, cat, and rat over 900 inoculations were made. In December, 1904, I made an endeavor to transfer a squamous epithelioma from a dasyure (*Dasyurus Viverrinus*) by inoculation into fifteen other dasyures and into ten cats, but nearly all the animals died within a week of a virulent septicæmia, and of the few survivors the dasyures prematurely fell victims to the misplaced zeal of another worker in the University, while the cats (three in number) show no sign of new growth to this day.

Earlier in the same year I had, through the kindness of Mr. A. S. Le Soef, of the Zoological Gardens, Sydney, obtained specimens of cancer from a lioness and from a tigress, both of which were old animals, and, by a remarkable coincidence, had died within a few days of each other. I have now under investigation a growth from a frog (*Hyla Aurea*) which I received a few weeks ago through the kindness of Dr. Chapman, and with it I have been able to infect other frogs, but I have not yet been able to determine whether it is true neoplasm or merely some form of infective granuloma. I have not yet been able to obtain a new growth in a mouse, but that is not surprising, since out of 30,000 tame mice examined by the Cancer Research Fund only twelve were found to have malignant new growths.

Jensen's original tumor was an epithelioma growing in a white mouse. Introduced into other mice, it yielded at first 30 to 60 per cent. of successful transplantations, and it has proved to be one of the most readily transmissible of all tumors hitherto tested. It is still growing, like a pure culture of bacteria. in various laboratories on the Continent of Europe, in England, and in America, the medium on which it thrives being successive generations of mice. In its duration of life (three and a half years) it has already exceeded the natural term of life of a mouse, and in bulk it has already exceeded that of many thousands of mice. This great power and persistence of growth is paralleled only by the natural propagation of successive generations of mice, and indicates an enormous potential vitality in the cells of the new growth, a vitality comparable to that which secures the continuance of the race. Nor is this an isolated phenomenon. The production of tissue by transplantation of other cancers of mice may proceed no less rapidly.

Jensen made so careful and accurate a study of the conditions of successful transplantation and of the nature of the process that his results have been abundantly confirmed, notably by the Cancer Research Fund. He found that the cells of the new growth could be isolated from the body for a considerable time without losing the power of originating a fresh tumor. When kept in a sterile condition on ice the cancer cells could still give rise to a new growth even after eighteen days, whereas an exposure of less than forty-eight hours to the body temperature rendered all inoculations abortive. "The tumor received by the Cancer Research Fund from Professor Jensen was forwarded by post from Copenhagen, under sterile precautions, and used for inoculation on the fifth day after it left Copenhagen." (First Scientific Report, p. 14.) These facts illustrate from another point of view the remarkable vitality with which the cells of the new growth are endowed.

In their general characters such transplanted growths resemble very closely the secondary (metastatic) disseminations naturally occurring in the course of many malignant tumors. The constituent cells of each are the direct

descendants of the cells of the primary growth. In the case of epithelial growths which alone have been transmitted the stroma is variable in amount, and is produced by the reaction of the invaded tissue. These facts are demonstrable in conditions of natural metastases also, as in the development of embolic foci in the liver. Throughout the longest series of sub-inoculations yet attained each successive tumor has been composed of cells structurally identical with those of the original growth.

The transmissibility of new growths from mice is conditioned to a remarkable extent by racial distinctions among the mice inoculated. Thus a cancer originally growing in a German mouse meets a subtle antagonism when introduced into an English or into a French mouse, and gives a very low percentage of successes ; but, as it becomes accustomed to the new race, the percentage of successes rises, and may ultimately equal that attained by direct inoculation of the original growth into German mice. Such specific distinctions between different races of the same species reveal the possibility of a reaction even more subtle than that elicited by the specific precipitins which can distinguish the corresponding albuminous molecules of different races within the same species.

When introduced into other mice of the same race, the new growth appears to be independent of such factors as the age or sex of the mice inoculated, and the frequently high percentage of successes contrasts very strikingly with the relative infrequency of primary new growths in the same animals. The conditions required for the successful transplantation of a new growth cannot therefore be identical with those necessary to its primary origin, a fact which indicates that in becoming a new growth the cells have acquired potentialities which enable them to continue growing in circumstances inimical to their original development.

PHENOMENA OF CELL-DIVISION EXHIBITED BY PATHOLOGICAL NEW GROWTHS AND THEIR SIGNIFICANCE.

Some of the most significant of recent observations on tumor growth relate to the phenomena of cell-division. In order that I may be better able to make myself intelligible, allow me to recall to you the main features of the process as it occurs in healthy tissues. As a rare and exceptional occurrence a cell may divide by a process of amitosis, or direct division, in which the nucleus becomes simply constricted, and separates into two parts, the cell likewise becoming constricted and later separating into two cells, each containing a portion of the original nucleus. This simple mode of bi-partition is limited to such highly specialised or degenerating cells as are found in glandular epithelium or temporary embryonic envelopes. (Wilson.)

The cell-divisons most characteristic of the development of animal and vegetable tissues are exceedingly complicated and constitute the process of indirect divison, or mitosis, of which only the briefest outline can be given. Here the nucleus undergoes elaborate preliminary changes, both of a physical and of a chemical nature, the prophases of the mitosis. The chromatin reticulum becomes gradually transformed into a convoluted filament, the spireme, of more intense staining reactions. At first fine and closely interwoven, the filament forms the close spireme ; later it shortens and thickens to form the open spireme ; ultimately it breaks across at intervals, giving rise to separate masses of chromatin, usually in the form of short bent rods, known as the chromosomes. These chromosomes become arranged equatorially upon the threads of a spindle-like structure which has in the meantime made its appearance within the cell. Each chromosome splits lengthwise (metaphase) into two exactly similar fragments, which gradually separate (anaphase) along the spindle-threads, so that precisely one-half of each chromo-

some diverges towards each pole of the spindle. The terminal phase, or telophase, consists of the division of the cell itself in the equatorial plane of the spindle, so that each daughter-cell contains one of the groups of chromosomes, from which the corresponding daughter-nucleus is reconstructed. The fundamental event in mitosis is the rigid distribution of one-half of each chromosome to each pole of the spindle, since it ensures that each daughter-nucleus shall contain exactly one-half of the chromatin originally present in the mother-nucleus.

From the time that the fertilised ovum first divides, and throughout all the cell divisions that go to make up the body of the embyro, this type of mitosis is maintained, and may be termed the somatic mitosis. Moreover, the number of chromosomes formed by each nucleus preparatory to division does not alter. It remains constant not only throughout the development of the individual but throughout all the individuals of the same species, and it is invariably an even number. Thus in man, the ox, the guinea-pig, and the onion the number is said to be sixteen; in the mouse, the salamander, the trout, and the lily it is twenty-four; in some nematodes it is as low as two or four, and in some crustaceans as high as 168. (Wilson.)

But not all the cells resulting from repeated segmentation of the fertilised ovum go to make up the body of the new individual. After a certain number of divisions the newly formed cells are called upon to play different parts. Some go to form the embryo, and by combined somatic divisions build up the body of the adult organism. Others go to form the embryonic envelopes and appendages, and yet others are early set apart as the precursors of the reproductive cells, and become incorporated within the embryo. It is to the cell-divisions met within the third group that I now invite your attention.

At first the reproductive cells multiply in the same way as do the body cells, but, as they approach maturity, they exhibit types of nuclear division characteristically different from that of the body cells. The critical mitoses that precede the maturation of the sexual elements are known as the reduction divisions, since one of the more obvious of the points of divergence consists of a reduction of the number of chromosomes to exactly one-half of the number characteristic of the somatic mitosis. Of these divisions there are at least two, and in the higher animals only two, the first being distinguished as the heterotype, the second as the homotype mitosis. (Fleming.) In the heterotypical division the reduced chromosomes assume peculiar forms, often appearing as loops, or rings, or tetrads, and their division on the spindle is not longitudinal but transverse. It is needless to enter into further detail. It is sufficient to emphasize the facts that reduction divisions are distinguishable from ordinary somatic divisions; that, in natural circumstances, they appear only in one tissue—the reproductive tissue—and there only at one phase of its development that preceding the ripening of the sexual cells.

That this phenomenon is in some way not understood, an essential prelimininary to the propagation of new individuals is indicated by its almost universal distribution. It has been found in all the higher organisms examined, animals and plants alike, marking the maturation, or a definite stage in the maturation, of the cells set apart for reproduction. In animals at all events, when all the critical mitoses have supervened, the cell is fully prepared for union with a matured cell of opposite sex, and until this union is accomplished no further development can, save in abnormal and exceptional conditions, occur. A new generation is originated by the union of two mature sexual cells, or gametes—the spermatozoon and the mature ovum. Each gamete contains within its nucleus half the number of chromosomes that are characteristic of the somatic mitoses of the organism arising from their

fusion, and the union of the two gametes restores the full number of chromosomes to the resultant cell—the fertilised ovum—which is the first of the new generation. And throughout all the cell-generations of the new organism the full number of chromosomes will be retained, except in the maturation divisions of its reproductive cells, when they in turn make their final preparations for that union which will introduce the next succeeding generation.

We are now in a position to consider the phenomena of cell-division as they appear in cancer and other pathological new growths. Cytologists have for many years been familiar with the fact that derangements of the mechanism of mitosis are common in cancer cells, and that in tumors of rapid growth the processes of cell-division may exhibit extreme disorder. (Klebs, Hansemann, Galeoti, quoted by Wilson.) Such derangements include asymmetrical mitoses, the chromosomes being unequally distributed to the daughter-nuclei, and multipolar mitoses, in which more than one spindle is formed. Even amitosis, or direct division, is not infrequently met. Similar abnormal mitoses may be artificially induced by the action of irritants or poisons, *e.g.*, in the epidermal cells of salamander by antipyrin, potassium iodide, &c. (Galeotti, quoted by Wilson.)

But it was left to Farmer, Moore, and Walker to discern a certain orderliness in this disorder, and their discovery, communicated to the Royal Society in December, 1903, was as simple as it was significant. They found that at the primary site of origin, and within a zone behind the advancing margin of malignant growths, the cells of the tumor were dividing after the manner of the heterotypical mitoses of reproductive tissues. Not only were the chromosomes of the dividing nuclei reduced to a number approximately half that of the somatic divisions, but the resemblance extended to other and often to minute points of detail, including the disposition of the chromosomes in the form of loops, rings, and tetrads, and their transverse division on the spindle. In other words, they demonstrated in the newly formed cells of primary cancerous growths processes of cell-division which all biological research has shown to be characteristic of the maturation, or of a definite step in the maturation, of the reproductive cells preparatory to conjugation.

Indorsed by the high authority of Professor Farmer and his colleagues, the essential similarity of cancerous tissue, at a certain stage of the primary new growth, to reproductive tissue is a fact of the utmost moment in relation to tumor growth. The attainment of the critical mitoses indicates that the cell has entered upon the terminal phase of its existence as a cell of the old generation, and that it is now ready to initiate a fresh life cycle, a new growth of cells, an independent organism. It does not indicate that a new generation of cells has yet arisen.

A further significant resemblance between the cells of cancerous and those of reproductive (gametogenic) tissues has recently been described by the same observers. In cancer cells there are frequently encountered clear vesicular bodies containing small deeply-staining granules, but their nature has long been an enigma. Occasionally they have been described as some form of protozoon parasite, whose presence stimulates the cell to undue proliferation and is the excitory factor in the ætiology of tumor growth. The presence of such cell-inclusions has formed the basis of many hypotheses regarding the parasitic origin of cancer. The parallel raised by Professor Farmer and his colleagues suggests a wholly novel solution. They point out that, in the final metamorphoses of the spermatozoon in the vertebrate class of animals, a vesicular body, the archoplasmic vesicle, containing one or more darkly-staining granules, is developed alongside the nucleus, and may

attain such a size as to indent and deform the nucleus. The vesicle with its contents ultimately persists as the so-called "cephalic cap" of the spermatozoon. This phenomenon is a conspicuous, and, so far as is known, a constant characteristic of spermatogenesis in the vertebrata, and is known to have an even wider distribution. In its general appearance the archoplasmic vesicle strikingly resembles the vesicular inclusions of cancer cells, and, as these authors conclude, "it is not, perhaps, accidental that, just as in the case of nuclear divisions, so also in the cellular inclusions, a parallelism between the cells of reproductive tissues and those of cancerous growths should be found to exist." But they do not on this acocunt regard cancer cells as identical with sexual cells.

Of the many fresh observations for which Drs. Bashford and Murray, of the Imperial Cancer Research Fund, are responsible, the majority deal with the nature and distribution of the mitoses in cancer cells. The work of Jensen and of Borrel on the transmissibility of cancer in mice, and that of Farmer, Moore, and Walker on the resemblances between certain cells of cancer and those of reproductive tissues, provided an opening of which they were not slow to avail themselves. While emphasizing the difficulties that attend the interpretation of the numerous forms of abnormal mitoses in cancer cells, they showed that heterotypical forms occurred not only in cancers of human origin but also in those of lower animals, including such widely divergent types as the horse, cow, dog, cat, mouse, and trout; that, in fact, the resemblance of cancer cells at a certain stage in their life history to reproductive cells approaching maturation is a phenomenon inferentially present in all cancers of vertebrate animals.

While thus confirming and extending the observations of Professor Farmer and his colleagues, they indicate other significant phenomena. I understand that, in their experience, somatic types of mitoses in which the nuclear divisions exhibit the full unreduced number of chromosomes are in fully developed tumors more common than reduced divisions, and that, whatever be the age and extent of the cancer, ordinary somatic mitoses are almost invariably present at certain stages and in certain conditions of growth. It is thus that the cells multiply at the advancing margins of malignant growths, whether primary or secondary, and thus that they multiply when, detached from the tumor mass, they undergo the initial proliferation that starts a natural metastasis or an artificial transplantation. Not only, therefore, does the heterotype and post-heterotype tissue form only a small proportion of any extensive new growth, but the cells which are youngest in time and presumably oldest in descent almost invariably exhibit somatic mitoses. Once the reducing divisions have appeared in reproductive tissues, the cells that are youngest in time and oldest in descent are in animals represented only by the mature and maturing sexual cells, and in animals and in plants alike are known to multiply only by mitoses in which the number of chromosomes is reduced by one-half. Not all the cells of cancerous growths can, therefore, be regarded as comparable to reproductive cells. After a time there must intervene some circumstance which restores the somatic mitoses, and, apart from parthenogenetic development, in which the ovum is fertilised by one of the polar bodies, there is only one event which is known to have this effect.

It is, as we have already seen, the conjugation of two cells, already prepared for union by a sequence of reducing divisions, that restores the full number of chromosomes to the resultant cell, and originates a new generation. This phenomenon, too, has been described (Bashford and Murray). In transplanted cancers of mice nuclei are found staining more darkly than usual, and drawn out into blunt processes as if amœboid. Frequently one of these

processes becomes closely applied to an adjacent cell wall. Occasionally the nucleus of the cell may be seen in contact with that of an adjacent cell, and apparently continuous with it. So far as I am aware, this conjugation of resting nuclei has been observed only at certain stages in the growth and development of transplanted tumors in mice. Though it has not yet been observed at the primary site of cancer, the reports of the Cancer Research Fund insist on the view that some circumstance equivalent to conjugation is both requisite and adequate to explain the salient phenomena of the growth, as distinct from the origin, of cancer. It is argued that only a new generation of cells, constituting in all essential respects a new organism, could be endowed with that independent and autonomous growth characteristic of a neoplasm.

Both Professor Farmer and Dr. Bashford draw a comparison between the intercurrent relations of certain cell-generations in plants and the relation of neoplasia to an animal organism. I propose rather to emphasize a relation between recurrent cell-generations in animals which constitutes a still closer analogy. In marsupials, and to a greater extent in the higher mammals, the new generation originating in the fertilised ovum is for a time definitely parasitic upon the old generation—the maternal organism—and the relation of the fœtal chorionic epithelium to the maternal decidua is essentially that of a neoplasm of restricted malignancy so far as local infiltration and independence of growth is concerned. The chorionic epithelium is already a new generation and already parasitic in relation to the maternal organism, but its growth is limited and its natural term of life restricted to the gestation period of the species. When it becomes cancerous, it is peculiar among cancers in being one generation further removed from the organism in which it grows—that is, altogether, two generations removed if the cancer itself represents a new generation, and, though embryonic in relation to other tissues, it is intrinsically senescent or decadent.

The new generation—neoplastic and embryonic alike—grows independently of the requirements of the parent organism, but is at the same time wholly dependent on the parent for its nutrition. In this relation of mutual dependence and independence no distinction whatever can be drawn between the new growth of a tumor and the new growth of a mammalian embryo. Where these two types of new growth fundamentally differ is in the regulated and orderly growth of the embryo and its envelopes, as opposed to the unregulated and disorderly growth of the tumor. Of this difference no explanation has been advanced. The origin of a cancer from the conjugation of cells would be sufficient to explain its independence in the sense above defined, but inadequate to explain its autonomy. A new organism, with laws of growth of its own, is an entity unknown in the whole range of normal biological development. Every new generation has most rigidly impressed upon it the laws of growth to which its parents have conformed, only minor variations being permitted. But in the development of a cancer we find the unprecedented phenomenon of a new generation of cells growing independently of, and often at variance with, the laws that regulate the growth of the parent tissue.

Recent observations in biological science have indicated a unit of living matter beyond the cell, beyond the chromosome—a unit consisting of the chromatin granule (chromomere). Boveri and Brauer (quoted by E. B. Wilson) regard the splitting of the chromosomes as an independent reproductive act of the chromatin. "In my opinion," adds Brauer, "the chromosomes are not independent individuals, but only groups of numberless minute chromatin granules which alone have the value of individuals"; and Wilson indorses this opinion in the words, "These observations certainly lend strong support to the view that the chromatin is to be regarded as a

morphological aggregate—as a congeries or colony of self-propagating elementary organisms capable of assimilation, growth, and division." (P. 114.) The chromatin forms the hereditary basis by which the processes of growth and differentiation are controlled, and from which they receive the specific stamp of the race (E. B. Wilson), and the accurate subdivision of the chromatin in mitosis is commonly regarded as the mechanism by which genetic continuity and consistency is maintained. In the normal development of the embryo an abnormal mitosis is rare; whereas in neoplasia, and particularly in malignant forms of rapid growth, pathological mitoses are abundant, and may indeed be so frequent as altogether to obscure the nature of the nuclear divisions. While I am fully aware that the significance of fertilisation and mitosis is one of the vexed questions in biology, it may not be unreasonable to correlate the regulated growth of embryonic tissue with the accurate subdivision of chromosomes, and the unregulated growth of cancerous tissue with the frequent irregularities in chromatin distribution.

So far as they have been observed, the mitoses in transplanted growths exhibit no abnormality such as would cause an unequal subdivision of chromatin even at the periods of gametogenic activity; and, further, the successive new broods of cancer cells show no deviation from the structure of the original brood throughout the longest period of observation yet attained, and appear to be subject to precisely similar laws of growth. Deviation in stucture to a slight degree, and liberation from the original restrictions of growth to a great degree, probably therefore depend on the extent of the uneven distributions of chromatin attendant on the birth of the primary generations.

THE RELATION OF NEOPLASIA TO SENESCENCE OF TISSUE.

The well-known fact that the liability to malignant new growth increases with the lapse of years, the so-called "age-incidence" of cancer, has acquired a fresh significance in the light of recent work. It is commonly stated that sarcomata are more frequent in the early years of life, while cancers are more frequent in the later. It is interesting to note that the recent statistical report of the Imperial Cancer Research Fund indicates that sarcomata, like cancers, increase in frequency as life advances. In my own experience I have been impressed with the fact that cancers form a much larger proportion of the malignant new growths occurring in young persons, and even in children, than is commonly supposed. It appears, therefore, that while either of the two main types of malignant new growth may occur in the young, yet the frequency of both increases with advancing years.

If this phenomenon be further analysed, it is found that the increase does not uniformly progress. Cancers of the stomach, for example, appear and attain a maximum frequency earlier than cancers of the skin; cancers of the uterus and mamma are more frequent towards the decline of reproductive activity than in the later years of life, and cancers of chorionic epithelium closely follow the appearance of that epithelium. It is not so much, therefore, the age of the individual in whom it occurs as the age of the tissue in which it arises that determines the maximum incidence of cancer. Even as regards the tissue, it is not, strictly speaking, its age, but its senescence that is the important predisposing factor. Tissues appear to be most liable to neoplasia when, having attained maturity, they enter upon the phase of decadence, or involution, and this phase is reached by different tissues at different periods in the life of the organism. Moreover, the same tissue (*e.g.*, mamma, uterus) may become effete at different periods in the life histories of different animals, and the age-incidence of cancer correspondingly varies. This is well illustrated by Professor Woods Hutchinson, who, in his "Studies in Human and Comparative Pathology," correlates the relative infrequency

of mammary and uterine cancers in the lower animals with the fact that these organs remain actively functional nearly to the end of life ; in other words, that the lower animals do not outlive these organs to the same relative extent as does the human subject. The reports of the Imperial Cancer Research Fund also insist on the importance of the "age-incidence" in the ætiology of cancer, and emphasize the fact that the same relation of cancer to decadence tissue obtains in man and in other animals, though in the latter, with their shorter tenure of life, the period of relative immunity is correspondingly shortened. The widely recognised influence of irritation and of injury in predisposing to tumor growth is conceivably and probably operative only in so far as it accelerates senescence of tissue.

The relative infrequency of new growths in animals other than man is probably due to various causes. If their brief tenure of life shortens the period of comparative immunity, still more does it abbreviate the period of maximum liability, and I am not aware that an average growth of cancer becomes more rapid in short-lived animals. Further reasons for this difference may be found in the facts that the habits of animals do not predispose so frequently as in man to premature senescence of tissue, and that organs which, in the human subject undergo involution many years before the natural term of life, in animals may remain functional to the end. And particularly in the case of wild animals in natural conditions is it true that those approaching decrepitude are either killed off or die of starvation. The important point is, not that new growths are less frequent in animals than in man, but that a consideration of the causes of this relative infrequency serves only to emphasize the significance of tissue senescence as a factor in the origin of new growths.

Following out this relation of the incidence of neoplasia to the age limit of tissue activity, Dr. Bashford and his colleagues were led to a remarkable observation. The animal in which transplantation of tumors from one individual to another has been most successfully effected is the mouse, whose natural term of life is about three years. Within this brief compass there are condensed the vital activities which in man may occupy more than half a century, and the processes are correspondingly shortened. Thus gestation in the mouse is completed in three weeks, while in man it occupies nine months. But new growths from mice had been propagated through generations of other mice over a period far exceeding the average duration of life in mice. It was natural, then, to inquire how the new growth came to be endowed with this apparently limitless power of growth without deviation from its original structure. A systematic examination of such transplanted cancers revealed the discovery that the growth was not continuous but interrupted ; that at periodic intervals there appeared a number of associated phenomena, including heterotypical mitoses and nuclear conjugations. The onset of these phenomena is related, I understand, not to the age of the animal or to the duration of the tumor growth within it, but solely to the number of cell generations of new growth that have intervened between the last preceding heterotypical mitoses, that is, in the strictest sense, to the age of the new growth. Just as mice after a few years would become extinct unless new individuals were generated by the union of reproductive cells, so apparently a maturation of gametes and their conjugation becomes at recurrent and definite intervals of time essential to the propagation of the cancer. This, if it can be substantiated, is the strongest argument yet adduced in support of the hypothesis that a new growth is essentially a new generation of cells arising after the manner of a new individual of the species. Nevertheless, it raises fresh difficulties.

That the senescence of the cells of the new growth should be the only apparent factor in determining the whole sequence of these phenomena in

transplanted growths is altogether remarkable. Consider for one moment what it means. There is abundant evidence that reducing divisions in non-reproductive tissues are not necessarily indicative of malignant new growth. They have been encountered not only in benign tumors, but also in proliferations of tissue of an inflammatory or irritative nature. Moreover, they may not be observed in some new growths of an undeniably malignant type. So far from their presence being a criterion of malignancy, it is not even a criterion of a new growth, nor does the new growth inevitably follow upon their appearance in a tissue. All that they signify is that the tissue is so far prepared for the initiation of a new growth that certain of its cells have in some way been induced to acquire the characters of reproductive cells undergoing maturation. Granted, then, that the senscence of the tissue, naturally or artificially produced, is adequate to determine the onset of the reducing divisions, nevertheless in the primary growth of a cancer it cannot be the only ætiological factor, since not every obsolete tissue gives rise to a new growth—since, even when maturation divisions have supervened, the tissue does not necessarily undergo neoplasia. Presumably the additional factor is some form of cell conjugation, and this in turn requires some stimulus not invariably present even when the conditions as to senescence of tissue and maturation of gametes has been fulfilled.

Not all cancers can be successfully transplanted ; but, if I interpret the observations aright, a successful transplantation does not depend upon the immediate initiation of a new generation of cancer cells in the new animal. I understand that the initial proliferation of a transplanted growth consists of cell generations continuous with the preceding cell generations, and that once the cancer cells have started to grow in a new animal they have never failed to continue to grow in that animal. Presumably, therefore, a transplanted cancer has never failed to propagate new generations of cancer cells when it approaches the age limit of its growth. There may be a failure to infect a new animal, but apparently never a failure to propagate a new brood of cancer cells, whatever be the age or sex or other conditions of the animal in which the cancer is growing. Why the origination of the secondary generation in transplanted cancer should be independent of that infrequent conjunction of conditions required to start the primary generation is a problem that awaits solution.

On the hypothesis that a new growth consists of a new generation of cells, it is not easy to understand why it should so infrequently follow upon the primary appearance of reducing divisions in somatic tissues, and so inevitably follow the recurrence of the reducing divisions in transplanted growths; whereas, on the view that a new growth is analogous to a mass of reproductive (gametoid) tissue, the difficulty disappears. Once the primary differentiation from somatic to reproductive (gametoid) tissue has been effected, then subsequent maturation of the gametoid tissue might be expected to recur at periodic intervals in transplanted masses, whether in the same or in other individuals, and the infrequency of a spreading new growth as a result of the primary differentiation presents no difficulty in view of the well-known and often extreme difference in cell-vitality.

Professor Farmer and his colleagues had noted the occurrence of somatic mitoses in the proliferating cells of the advancing margin, but regarded them, as doubtless they would regard similarly dividing cancer cells elsewhere, not as the descendants but as the precursors of the cells undergoing reducing divisions. Until it has been shown that the conjugation of resting nuclei is a constant phenomenon at certain stages of tumor growth, it is not possible to prove that the somatically dividing cells are the products of the union of gametes. The data accumulated by the Imperial Research Fund on the recurrence of appearances resembling conjugation in transplanted tumors of

mice affords a certain measure of presumptive evidence in favor of both views, though the hypothesis of the origin of cancer as a new generation is certainly more consistent with other known facts of tumor growth. It must be admitted, however, that the phenomena described by Bashford and Murray as effective nuclear conjugations are capable of bearing other interpretations, notably that of imperfect and abnormal forms of division, or merely that of abortive conjugations. It is undoubtedly a further objection that these reputedly effective nuclear conjugations occur between gametes matured not only in the same organism, but side by side in the same tissue—a circumstance which demands the assumption of a phylogenetic reversion to an extent unparalleled in biology.

The gametogenic tissue of animals does resemble a new growth in so far as its cells ultimately form highly independent individuals—independent of the needs of the organism and independent of each other. Moreover, although their growth is not unregulated and aberrant, as in cancer, it is, during the period of reproductive activity, unceasing and practically unlimited. If a similar twofold independence and unlimited growth could even for a time be acquired by a group of somatic cells, the product would not greatly differ from cancer with a limited tenure of life. And, if it be objected that the life of a cancer may, so far as we know, be unlimited, it must be borne in mind that a patient may outlive a new growth, and not rarely does outlive the less malignant forms presumably endowed with a smaller measure of vitality.

In this relation I should like to quote the views of that eminent biologist, Professor E. B. Wilson, who says : "As far as we can see from an *a priori* point of view, there is no reason why, barring accident, cell-division should not follow cell-division in endless succession in the stream of life. It is possible, indeed probable, that such may be the fact in some of the lower and simpler forms of life where no form of sexual reproduction is known to occur. In the vast majority of living forms, however, the series of cell-divisions tend to run in cycles, in each of which the energy of division comes to an end and is restored only by an admixture of living matter derived from another cell " (fertilisation) ; and he adduces evidence that even in unicellular animals, infusorians, the processes of growth and division sooner or later come to an end, undergoing a natural senescence, which can be counteracted only by conjugation. He concludes : " That fertilisation in higher plants and animals does, in fact, incite division and growth is a matter of undisputed observation. We know, however, that in parthenogenesis the egg may develop without fertilisation, and we do not know whether the tendency to senescence and the need for fertilisation are primary attributes of living matter " (pp. 178, 179). It is possible to find in these quotations some measure of support for each of the hypotheses under discussion. It is only fair to point out, however, that neither is regarded by its authors as more than a tentative working hypothesis, to be discarded when fresh discoveries have revealed its insufficiency, but meantime to be put to its legitimate use as a means of co-ordinating known facts and as a guide to future research.

In this relation also I would again emphasize the fact that the cells which ultimately form the reproductive tissue are of the same generation as those which go to form the embryonic envelopes (trophoblast) and those which go to form the body of the mammalian organism. They are all descendants of the fertilised ovum, derived from it by repeated mitotic division, and for a time indistinguishable one from another. It is perhaps not too much to assume that cells so intimately related in origin, however much they may ultimately differ in structure and in function, should retain certain tendencies in common, and in particular that, just as reproductive cells approaching maturity make preparations of a very special kind for conjugation with other

specially prepared cells, so something of the same tendency has descended to the non-reproductive cells also. It is not inconceivable that such an inherited trait should be evidenced only when the other vital activities of the cells begin to cease, when the onset of senescence reveals the need for a further renewal of energy. This is not inconsistent with the fact that the higher the differentiation of the tissue of origin, the more restricted is the differentiation of its progeny in the form of a new growth or otherwise.

The occasional occurrence of heterotypical mitoses in senescent tissues, with or without the subsequent conjugation of matured gametes, might thus be regarded as the fulfilment of a natural law, as the ultimate realisation of a potentiality latent throughout the greater part of the life history of the cell, being obscured by other, and for the time being more urgent, demands on its vital energy. It is not impossible that tumor growth will prove to be a natural phenomenon determined by a definite concatenation of natural circumstances, of which one is the senescence of the tissue, and that the nature and origin of cancer will be essentially a problem of biology, and only accidentally a problem of pathology.

THE RELATION OF CHEMICAL CONDITIONS TO TUMOR GROWTH.

The intimate relation of chemical to biological processes has been demonstrated most strikingly by Loeb, whose results have been abundantly confirmed by subsequent research. It may be accepted as proven that the development and growth of living cells may be profoundly modified by slight chemical alteration in their environment. Perhaps the most remarkable of all these observations relate to the development of the unfertilised ova of certain echinoderms, notably sea urchins and starfish. In these animals the sexes are separate, and development of a new organism naturally occurs only by the union of two mature sexual cells, suspended in sea water. Loeb, however, by minor, though definite, alterations in the saline constituents of the water, was successful in causing the unfertilised ova to segment and develop just as if they had been fertilised by the spermatozoon, and in suitable circumstances the segmenting ova could be induced to form actively-swimming normal larvæ. Not only in the echinoderms, but also in annelids (segmented worms), in which also the sexes are separate, an artificial parthenogenesis could be similarly induced, so that free-swimming larvæ were developed from unfertilised eggs. In this artificial parthenogenesis only one polar body is given off, so that there is no reduction in the number of chromosomes ; where there is a reduction the normal number soon becomes restored by a process of auto-regulation. (Delage.) I am indebted to Professor Haswell for this reference. That it should be possible, by a comparatively slight variation of chemical conditions, to induce in the ova of sexually developed animals a reversion to a non-sexual form of development (parthenogenesis) is not without possible significance in relation to neoplasia.

These phenomena are not merely scientific freaks. They acquire a new importance in view of the work of Professor Moore and his colleagues on the reaction of the gastric secretions in cases of malignant disease. As these authors indicate, it was one of the few definite facts known about cancer that, when the stomach was the seat of the new growth, the secretion of free hydrochloric acid was nearly always in abeyance, and it was also known that this might occur even when the growth was of small dimensions and had not produced any other noticeable alteration in the stomach. I do not propose to discuss the various suggestions that have been advanced in explanation of this phenomenon, nor the observations made on the effect of new growths in different positions of the stomach, nor the occurrence of diminished acidity in other morbid conditions.

It occurred to Professor Moore and his colleagues to investigate whether this phenomenon was independent of the site of the cancer, and they found—I give the findings in their own words—that " the absence of free hydrochloric acid in cancer of the stomach is not due to any local action in that organ, for hydrochloric acid is absent or greatly reduced in amount whatever may be the situation in the body of the malignant growth. The amount of free hydrochloric acid present in the stomach contents was determined in seventeen cases of malignant disease situated in different regions, such as the uterus, mamma, prostate, rectum, tongue, cheek, and mouth, and in about two-thirds of the cases the free hydrochloric acid was found to be entirely absent, while in the remaining cases the amount found was much below the normal, being reduced to a mere trace in all except one case, where it reached about one-fifth of the average amount obtained in control determinations carried out upon ourselves." Since the phenomenon is independent of any local action in the stomach, and since, moreover, it is so pronounced and constant, so far, at least, as the limited number of observations recorded can justify such a statement, it certainly suggests a profound chemical alteration in the body fluids, and, as the authors point out, this change may be a common cause of the new growth and of the absence of the acid, or it may be a result of the growth and the cause of the absence of the acid. The cause of the absence of the acid, however, is not yet determined. It may be that the oxyntic cells are atrophied, or inhibited by some toxic substance in the blood, and the presence of a ferment (malignin) within cancer cells, and liberated by their disintegration, has been described. (Petry and Blumenthal.) Nevertheless, it is more probable that the explanation must be sought in the chemical constitution of the blood itself, and that the elimination or arrest of free hydrochloric acid is an indication " through the mechanism of the oxyntic cells " that the essential acidity of the blood is decreased and its essential alkalinity increased. Dr. G. E. Rennie has suggested that an increased alkalinity of the blood plasma might be determined by some disturbance in the nervous control of metabolism such as is indicated in the contrasted condition of acidosis and diabetes. These facts and considerations further illustrate the extended scope of modern pathology and its relation to, and foundations in, biology, chemistry, and physics. I much regret that time does not permit me to discuss other recent contributions, notably those of Ribbert, of Beard, and of the many cancer research institutes.

INDICATIONS FOR THE TREATMENT OF NEW GROWTHS.

When the cells of one animal are repeatedly introduced within a second animal of a different species, it frequently happens that substances antagonistic to these cells are developed in the body of the inoculated animal. If, now, some of the blood serum from the second animal be injected into an animal of the same species as that from which the cells were originally taken, the serum may exert a destructive action on these cells. It is upon this general biological law, governing the ultimate reactions of living cells to alien albuminous molecules, that the principles of immunisation against bacterial infection are based.

Whether we regard a tumor as a new generation of cells or not, the fact remains that it constitutes an invasion of the organism by a brood of independent and hostile cells, and, in the sense in which we have defined the process, it is equivalent to an infection. It seems not unreasonable to hope that substances antagonistic to the cells of a new growth could be developed in the blood of an otherwise suitable alien animal by injections of the tumor cells, as in the process of immunisation, and that the blood serum could then be used with effect either to inhibit or destroy further growths of the same tumor or growths of other similar tumors. The extremely restricted specificity of the action of

living tissues to cancer cells, so that evidence of a difference is obtained among closely related cancer cells from the same species (Scientific Reports, No. 2, part ii., p. 33), indicates a possible difficulty in the preparation of a serum antagonistic to other new growths of similar structure to that used in its preparation, though much distinction might not be apparent when injections were made into alien animals. Already considerable attention has been paid to this possible development, but as yet no certainty has been attained.

The remarkable agencies revealed within the last few years by physical science have been impressed into the service of medicine, and their influence on tumor growth is now on its trial. The effect of radium on transplanted new growths of mice has been investigated by the Cancer Research Fund, and, since the life history of these tumors has been under continuous observation for several years, the results obtained may be regarded as independent of the numerous sources of error which discount the value of merely clinical experience. Moreover, as the experimental results corroborate the earlier clinical observations, a considerable body of evidence has now accumulated in support of the fact that radio-active bodies and solutions do in a most surprising and apparently capricious manner cause the disappearance of some tumor growths, though not of all.

Pieces of radio-active lead, solutions of radium-emanation, and sterile radio-active saline solutions were used. The duration of each exposure varied from three to thirty minutes, and was repeated for the same length of time on the following days. Within the second week after the first exposure, four of the tumors began to diminish in size, while others continued to grow. There was no noticeable relation between the length of exposure and the effect on the tumor. The influence of radium in producing absorption appeared to be directed to the connective tissue elements, which underwent marked proliferation, and split up the tumor into smaller islets of cells. Even at this stage the tumor cells appeared healthy, but within a few days they degenerated, then died, then disappeared, probably owing to cicatricial contraction of the fibrous overgrowth. These observations confirm the previous work of Exner, and controvert the more common assumption that radium exercises a selective action on the tumor cells.

The Röntgen rays (X-rays) have also been employed in the treatment of cancer, and some measure of success has been recorded in superficial forms. I am not aware, however, that their influence has been submitted to any such rigorous test as that undertaken for radium. It is known, through the extensive researches of Heinecke (quoted in the Second Scientific Report, part ii., p. 61), that exposure to the Röntgen rays or to radium produces alterations in the spleen and lymphatic glands, and several accounts have recently appeared of the successful treatment by X-rays of leucocythæmia—a condition recognised to be closely related to, if not identical with, neoplasia. Only last month Dr. Reissmann, of Adelaide, published an illustrative case.

I have grievously failed if I have raised any false hopes that the treatment of cancer by any other means than actual removal is yet practicable. It is obvious that to be effective the removal must be early, free, and careful; and the lamentable disregard of one or other of these precautions is responsible for many disasters. Patients should be encouraged to seek advice in the early stages of suspicious tumors, and the surgeon should remember that these are not cases for expectant treatment nor for conservative surgery. I do not advocate wholesale butchery, but I do advocate the common-sense application of known facts and scientific methods to the surgical treatment of tumor growth. In these days there is no reason why every doubtful tumor should not be submitted to a microscopic examination. If you are still in doubt whether the condition is inflammatory or malignant, give the patient the

benefit of the doubt and remove the suspicious mass. Remember that prolonged irritation induces senescence of tissue, and predisposes to neoplasia. If you are in doubt whether the growth is malignant or benign, remember that all are potentially malignant, that the difference is one of degree, not of kind, and do not hesitate to remove it. If you are satisfied that a malignant growth has really started, bear in mind the ease and rapidity with which secondary infections may occur, and do not risk losing a life for the sake of saving a limb. The knife should never be within an inch of the new growth, and, where possible, many inches should intervene. I do not need to warn this audience against those secret nostrums which, born of cupidity and nurtured in credulity, if they do not aggravate by irritation, serve only to delude the patient into waiting until perchance the time for operative interference has passed.

ADDENDUM.

Since this address was delivered, I have been repeatedly asked whether there is any foundation for the not uncommon belief or suspicion that cancer is contagious or infectious. While I am fully aware of certain recorded instances of the communication of cancer from one person to another (*cancer a deux*), and of the existence of so-called cancer houses and cancer districts, I cannot too strongly emphasize the fact that the difficulties attendant on the artificial transference of new growths sufficiently demonstrate that cancer is neither contagious nor infectious. The hypothesis that cancer is due to the invasion of parasitic microbes is far from being substantiated, and, even if it should later be proved that the phenomena of cancer are initiated by some form of parasite, it does not necessarily follow that cancer is contagious or infectious. On the contrary, it must be admitted that the distribution of the presumptive parasite and its mode of action are wholly dissimilar to those of any known bacterium or protozoon, and that in particular it is not capable of reproducing the disease in other persons after the manner of other morbific agents known or assumed to exist.

NOTE ON A LARVAL TAPEWORM FROM THE HUMAN SUBJECT (BOTHRIOCEPHALUS MANSONI, OR LIGULOIDES).

By Drs. MacCormick and Hill.

About three and a half years ago a young man, A. S., aged 27, married, a plumber and gasfitter, residing at 27, John Street, Tempe, a suburb of Sydney, was treated by me in the Royal Prince Alfred Hospital. This man never lived out of Sydney, with the exception of a fortnight's holiday at Penrith, some forty miles from Sydney, six or seven years ago. He was employed during the previous eight years in a cocoanut oil factory in Balmain. The cocoanuts came from the South Pacific Islands.

Twelve months prior to admission to the hospital what was said to be a fatty tumor was removed from under the skin in the upper part of the right lumbar region, at the costal margin. One month afterwards he noticed a similar swelling 2 inches lower down. This swelling was soft, and fairly circumscribed, and about the size of a hen egg. This tumor was removed by dissection, under chloroform. On making a section through the part removed it appeared to be soft granulation tissue, in the centre of which was a white body, which, on further investigation, was found to be a worm about one and a half inches long, of the color of lard, and possessing feeble movement. On August 22nd, 1905, patient was examined, and no trace of any

further disease could be detected. During the intervening three and a half years which had elapsed since the operation he enjoyed good health. The worm was sent to Dr. Hill, of the Zoological Department of the University, for investigation.

Manson, in his book on Tropical Diseases, mentioned the fact that this parasite is found in Bathurst, in Australia. The late Dr. Spencer published an account of two cases which had occurred in his practice in Bathurst; and possibly Manson may be referring to these cases. In Spencer's cases the worm apparently gave rise to a diffuse abdominal tumor, containing some fluid; and both of these cases ultimately died.

This parasite, according to Manson, has its habitat in the sub-peritoneal fat, and thence may find its way into the bladder or pleural cavities. It has been frequently passed per urethram. As far as I am aware Spencer's cases and my own are the only instances in which this parasite has been met with in Australia, and the tumor that it causes from its mere rarity must naturally be a matter of difficulty in diagnosis.

NOTES BY DR. J. P. HILL.

The worm proves, on examination, to be an immature sexless tapeworm, or larva of the "plerocercoid" type. So far as we are able to judge, our specimen is apparently identical with *Bothriocephalus Mansoni*, originally found by Manson (1) in a Chinaman at Amoy, and described by Cobbold (2) as *Ligula Mansoni*. Leuckart (3) has also examined a specimen of this parasite, and has given a detailed account of its structure. He showed that it was more closely allied to the genus *Bothriocephalus* than to *Ligula*, and gave it the name of *Bothriocephalus liguloides*: the parasite is, accordingly, now known as *B. Mansoni*, Cobbold. The adult form is still unknown, or, if known, its connection with the larval form has not been established.

On removal from the mass of granulation tissue in which it lay embedded, the worm exhibited distinct, but feeble, movement. On transference to alcohol it underwent strong contraction, and, unfortunately, broke into several fragments. In the living condition its total length is estimated at about 3.5 cm.: in spirit the fragments give a total length of 1.5 cm., and a greatest breadth of 1.5 mm. It is thus very much smaller than Manson's specimens, the length of which is given as about 30–35 cm.

Cobbold's figure gives a fairly accurate view of the general form of the organism. The narrow elongated body is flattened and unsegmented, and, in the preserved fragments, is seen to be crossed by numerous transverse wrinkles. It is broadest just behind the bluntly pointed anterior end, and tapers gradually posteriorly. The head is not marked off from the succeeding portions of the body: it is small, and only partially protruded. On its flat surfaces are recognisable the two longitudinal suctional grooves, or "bothria," characteristic of the family *Bothriocephalidæ* (cf. Leuckart's fig. 404).

Transverse sections through the mid-region of the body of our specimen show a general agreement with Leuckart's figure and description. The cuticle is a relatively thick layer, to the outer surface of which fragments of the enclosing granulation tissue have remained firmly adherent. Immediately within the cuticle the dermal musculature consists of the usual circular and longitudinal fibres, the latter being well marked. The subcuticular cellular layer presents the usual characters. The ground parenchyma tissue consists of a reticular vacuolated matrix, with scattered nuclei; cell outlines are not readily distinguishable in the preparations. It is uniform throughout its extent, there being no distinction into cortical and medullary zones, owing to the absence of distinct bundles of circular muscle. The muscle bundles present in it are small, and show no definite arrangement or

grouping—many of them run dorso-ventrally. There are no definitely arranged bundles of longitudinal muscles such as are found in the closely allied larval tapeworm (4) occurring amongst the superficial muscles of the thigh of the common frog, *Hyla aurea*.

The vessels of the excretory system are, as noted by Leuckart, extensively developed and of large size. The "lime" cells are readily recognisable in the sections—they are relatively large, and not very numerous. Unfortunately the calcareous corpuscles themselves were not observed, as all the fragments of one single specimen were inadvertently treated with acid after staining.

As regards the life-history of *B. Mansoni*, we are as yet completely in the dark. It seems probable that man is only an accidental, and not the regular, host of this larval form.

(1) Manson, *Lancet*, October 14th, 1882: Tropical Diseases, pp. 601-2, and fig. 98.
(2) Cobbold, *Jour. Linn. Soc. (Zool.)*, vol. xvii., 1884.
(3) Leuckart, *Parasites of Man*, p. 745, *et seq.*, figs. 402-4.
(4) Haswell, *Proc. Linn. Soc. N.S.W.*, vol. v., 2nd series.

TRYPANOSOMIASIS.

By E. Angas Johnson, M.D., M.R.C.S.

Mr. President, Ladies, and Gentlemen—In the course of my peregrinations into pathological laboratories abroad, nearly always have I found that parasites (with a few exceptions) are almost ignored. It may be that investigators do not care for the disgusting surroundings (not so for a zoologist) in which parasites are found, or have not opportunities; or, more frequently the case is they do not care about them.

In Australia only a few continue their study of pathology . the majority of medical men are content with the rudiments they learnt to pass their examinations. Why is this thus ? The simple reason is, this fascinating study takes time as well as money ; and, as Governments do not look with favor on expenditure of money for research, few are in the position to devote their time and money for investigation.

Parasitology is practically ignored. However, the Liverpool and London schools of tropical medicine are now awakening to the importance of parasitology and devoting much time, skill, and money to its elucidation.

It behoves us in Australia now to energetically direct attention to this subject, because communication with any part of the world is only a question of weeks, and soon, with turbines, it will be days ; besides, when the Panama Canal is opened no doubt many diseases of the tropics will find their way through this channel direct to Australia.

We know all over the world that history repeats itself, and "crass ignorance" on the part of so-called experts has frequently resulted in a disease getting a foothold in a country ; and then enormous sacrifice of life, with a corresponding necessary lavish expenditure of money to combat the epidemic, takes place. We know that malaria has been taken to virgin lands in the following manner :—A patient suffering from malaria is on board a ship ; in the water-casks are anopheles in all stages of development, and these become infected from the patient and then infect other people. The new land is reached, conditions are favorable, and so the disease takes root.

Frequently the source of infection is present in a land, but the intermediate host is wanting: and the contrary holds true.

Here in South Australia we have the anopheles, and, from Mauritius, we often get malaria subjects through the sugar trade; and unless we take great care this place will be noted for malaria. Tropical dysentery was unknown here before the South African war; last year there were several small epidemics near Port Adelaide. What diseases may the soldiers introduce in their saddlecloths, or in monkeys hidden under their greatcoats? Now the horses are not allowed to return, but the saddlecloths are. This is "crass ignorance" on the part of the Commonwealth experts.

Foreign animals are constantly being imported for domestic pets, circuses, and zoos. Think of this danger! Think of the diseases introduced into America from Africa 200 years ago when the slave trade flourished.

In 1850 there were as many centenarians, in proportion to the population, in Mauritius (in spite of its being a tropical country) as elsewhere in the world; then malaria and trypanosomes were unknown there. Now, fifty-five years afterwards, the place has almost been devastated through these agencies, in spite of its being an excellently ruled British colony; and so the proportion of centenarians has naturally diminished—in fact there are less in Mauritius, in proportion, than elsewhere in the world. What can we expect in Australia, unless drastic preventative measures be adopted at once?

As I remarked previously, parasitology has almost been ignored. When I reported a case of trichina here three or four years ago many medical men doubted it, although shown the specimens—and here they are!

Dengue fever has played havoc lately in Queensland, and possibly will come here, through the train; then the trypanosome will work havoc in our horses unless we protect them. Now, as this whole subject of trypanosomiasis is in its infancy, and still *sub judice*, it is not necessary for me to go over the whole of the ground; but, as I have had experience with it in three different laboratories abroad, and having had time to conduct original research work myself, I will endeavor to give you a few facts from an unbiased point of view, and let you draw your own conclusions. Where an observer has quoted a discovery I have used his own words as far as possible, but most authors in the case of the trypanosome have always referred me to Laveran and Mesnil, who have so exhaustively reviewed this subject in a book of 400 pages, and this work I have freely consulted.

PROTOZOA.

1. Subphylum. Plasmadroma.

Class I.—Rhizopoda orders 1-5.

Amoebina, Radiolaria, Helizoa, Foraminifera, Mycitozoa.

Class II.—Mastigophora. Sub-class Flagellata. Orders 1-5.

1. Protomonadina. Family Cercomonadina. Family Trypanosomidæ.
2. Polymastigna (Trichomonas Vaginalis). Lamblia.
3-5. Englenoidina, Chromonadina. Phytomonadina.

Class III.—Sporozoa. Sub-class 1. *Telosporidia.*

Order I.—Coccidiomorpha.
Sub-orders.—(1) Coccidia. (2) Hæmospoirdia. (3) Piroplasmata.
Order II. Gregarinida. (ii.) Neosporidia. (ii.) Subphylum. Ciliophora

Class IV.—Ciliata.

Class V.—Suctoria.

This is Professor Nuttall's classification, and shows where the trypanosome is placed.

THE MORE COMMON VARIETIES OF TRYPANOSOMES MET WITH IN MAN AND ANIMALS.

Common Name of Disease.	Species of Parasite.	Discovered by—	Geographical Distribution of the Disease.	Propagation of the Disease.	Pathogenicity.
Dourine (mal du coit)	Tr. equiperdum	Rouget, 1894	Nebraska, Wyoming, S. Dakota, S. Europe, Turkey, Asia Minor, N. Africa, Hungary, Spain, Java, and Switzerland	Coitus	Same as surra
Galziekte (gall sickness)	Tr. Theileri	Theileri, 1902	S. Africa, Rhodesia, Togo, Mafia (German E. Africa)	Hippobosca rufipes	Cattle only
Trypanosome fever.....	Tr. Gambiense	Forde & Dutton, 1901; Dutton, 1902	Equatorial and W. Africa	? Glossina palpalis. ? Tabanus dorsovitta. ? Fleas and lice	Monkeys, rabbits, guinea-pigs, rats, dogs, sheep, goats, &c.
Rat trypanosome......	Tr. Lewisi	Lewis, 1878	World wide	Fleas and lice	Occ. guinea-pigs
Mal de caderas.........	Tr. Equinum	Elmassian, 1901	S. America	? Tabanus ? Stomoxys	Same as surra
Nagana (tsetse fly disease)	Tr. Brucei	Bruce, 1895	S. and W. Africa	Glossina morsitans	Same as surra
Surra	Tr. Evansi	Evans, 1880	India, Burmah, Persia, Mauritius, Philippines	? Tabanus tropicus (horse flies) ? Stomoxys calcitrans (stable flies)	Rats, monkeys, all domesticated animals, and nearly all wild ones

The distribution of glossina palpalis and sleeping sickness in Uganda is identical. Where there are no flies, there is no sickness.

Classification of the genus Trypanosoma by Salmon and Stiles (Greek Trupanon—borer, soma—body).

Class, Mastigophora. Sub-class, Flagellata. Order, Monadida.

Independently living organisms, but in some cases they form colonies. The number and arrangement of the flagella varies. There is sometimes an undulating membrane. *Family*—Trypanosomidæ includes the genus trypanoplasma, parasitic organisms resembling the trypanosomes, but possessing two flagella, one of which is situated at each end. *Genus*—Trypanosoma. *Species*—Tr. rotato.ium (Mayer), Tr. Lewisi, &c. Trypanosomiasis is a group name, and it belongs to the protozoa. (1) The trypanosoma, characterised by having a longitudinal undulating membrane, which arises posteriorly from the centrosome, and it ends as a free flagellum anteriorly. Division takes place by longitudinal division. (2) The trypanoplasma has an anterior flagellum, which forms the thickened border of the undulating membrane, and a posterior flagellum, which curves round the posterior end of the parasite, and is free, being about the same length as the anterior one, and both arise from the same centrosome.

Trypanosomiasis is a term used to denote a specific infection in birds, fishes, reptiles, mammals, and amphibia, and it is universally distributed. I have collected eighty-two terms which designate it. It is endemic as well as epidemic over large tracts of tropical and sub-tropical countries, being worse during the rainy season.

In 1841 Valentine discovered the trypanosome in the trout (*salmo fario*), and in 1842 Grube gave it its definite name. From 1843 to 1850 trypanosomes were found in frogs, rats, moles, and field mice. In 1878, Lewis, whilst working in India, discovered the trypanosomes in rats, and this served as a starting point for investigation. Harmless in rats, and presents, as far as we know, no definite symptoms. In nature we only see the survivors of the fittest; so, if young rats are susceptible, they might die of an acute disease. It is distributed world wide. From 15 to 95 per cent. of rats affected. Rats may harbor the disease for two years and yet show no symptoms. Fleas transmit it in rats, as proved by Rabinowitsch, and she thinks it possibly may be a repeated infection. The rat trypanosome has been kept on ice for forty-five days and retained its virulence; at fifty degrees C. for two hours, it is still infective. It has been observed in fleas and bugs twenty-four hours after their sucking blood; acquired immunity is active, and due to antibodies; it has a specific agglutinating power on the trypanosome. Incubation period is three to five days.

The filtrates of cultures of Tr. Lewisi that have passed through a Berkefeld filter are infective, and Schaudin thinks that, as the trypanosome (male) of Spirochaeta Ziemanni is so small, it could also pass through a Berkefeld filter. (Smedley thesis, M.D., Cantab.) Cultural trypanosome of rats differ considerably from those of the adult parasite found in the blood. They are:—(1) Very motile and also spindle-shaped. (2) Centrosome placed anteriorly. (3) The flagellum is very long and active, and has but a short intra-cellular course, on account of the position of the centrosome. (4) No undulating membrane. (5) These trypanosomes form colonies, which ultimately contain many thousands of individuals.

Surra struck Mauritius in 1902-1903, and killed off 100 per cent. of the horses and 25 per cent. of the cattle attacked. It is limited in Java on account of very severe legislation.

Musgrave and Williamson (Trypanosome of Horses. Department of Interior, No. 3, p. 10) say that in 1901 some twenty-six horses came direct

from Australian ports to the Philippines, and one was ill at the time of arrival and three others died later from surra If this is correct we might at once start investigations. (Professor Watson denies this statement; he says the horses were Australian, and had been shipped to India and thence to Hong Kong, and from there to Manila, and that the infection took place in India.)

A number of extremely interesting immunising experiments have been made by several competent observers, and these have been most helpful in separating the types:—

Expt. 1.—Laveran and Mesnil immunised a goat successfully to nagana, mal de caderas, and surra, and yet it died with the horse trypanosome of Gambia (*tr dimorphon*).

Expt. 2.—Nocard, Vallee, and Carre infected a cow with surra, which was immune to nagana.

Expt. 3.—Nocard killed two dogs with nagana that were immune to dourine.

Expt. 4.—Lignieres killed two dogs with mal de caderas which were immune to dourine.

So that these immunising experiments have helped to clear up the difficulty in almost definitely distinguishing between surra and nagana, as well as between dourine, surra, and mal de caderas.

But there is this fact to be borne in mind, that there are variations of susceptibility of the same species of animals in different parts of the world that are confusing. Previously one had to depend on minute differences in the morphology between the various trypanosomes causing the various diseases; but there are differences almost as great existing between individual members of the same species.

Dourine (*mal du coit*).—Rouget discovered the trypanosome of this disease in 1894. This disease is only propagated by coitus, and some authorities affirm that the Tr. equiperdum can bore its way through an intact healthy mucous membrane. When an animal is affected the extremities become paralysed, especially in the hind limbs, and the penis and scrotum in a stallion gets very much swollen. If so, castrate all stallions and kill all mares affected.

Nagana (Tsetse fly disease).—First seen in the Zambesi basin, in 1857, by David Livingstone (*British Medical Journal*, May 1st, 1858), and described by him. Laveran and Mesnil say his description is very precise and scientific for the time at which he wrote. He used to treat his horses with arsenic in 2-grain doses daily for a week. This relieved them (he says cured) for a time; but they all died within six months afterwards. This disease is chronic in bovines, sheep, and goats, as well as in many wild animals in Zululand, although often the parasite cannot be found in their blood; but inoculate a tame animal from a wild one and usually one can reproduce the disease.

The Tsetse fly (*glossina morsitans*) (Austen tabulates eight varieties of the *glossina*).—A stinging fly, not much larger than our common house fly, is the actual carrier of the disease. It is a mechanical transference, because there is no sexual process in the fly, since forty-eight hours after sucking the blood of an infected animal they are non-infective. These flies are found in little hollows where wild animals go, and wild animals infect these flies. Sambon disagrees with this, and thinks that perhaps the fly is an intermediate host; otherwise why do not tabanus and stomoxys spread the disease mechanically?

Gray and Tullock have confirmed Sambon's views, that the parasites undergo a cycle of development in these flies.

Compare the mosquito (*stegomyia*), which is the actual carrier of yellow fever; it takes twelve days to become infective, and in malaria, where it is the anopheles, it does not become infective for ten days (Bruce).

Mal de caderas.—The centrosome of tr. equinum is very small, and stains very slightly, thus differing from nagana and surra. Fatal to mice, rats, hedgehogs, armadillos, otters, monkeys, field mice, dogs, rabbits, coati, sheep, guinea-pigs, and cats. The goat and ox seem to be immune, but their blood is able to harbor the parasite for months.

In South America there are epidemics of this disease concurrent with epidemics in animals, like guinea-pigs (*carpinchos hydrochoerus capibara*), which are natives there. These small animals live on the banks of the Paraguay; they are hunted for their skins; the dogs eat their carcasses and become infected with trypanosome equinum. The horses become infected, but not known how, as there is no biting fly known. Fleas and lice?

Human Trypanosomes.—Tr. Dutton, Tr. gambiense. In Bombay it is known as epidemic general congestive dropsy. In Africa as splenomegaly. Winterbottom first described it (1803). Clarke noted it in Sierra Leone (1843). In 1890 Nepveu at Algiers found flagellates in the blood of a malarial patient: he called them trypanosomes, but failed to classify the variety. Ross did not know whether Dutton's Trypanosome approached Tr. Lewisi or Tr. Brucei.

Lancet, vol. 1, January 11th, 1902, p. 107.—Dutton wrote from Bathurst (Gambia) to Ross about a European male who was under Dr. Forbes' care, which Dr. Forde thought was filaria, saying that:—"(1) The patient suffered from a relapsing fever although the temperature never was very high. (2) Was general weakness and wasting. (3) Local œdemas, especially around the eyes and legs, and general puffiness of the face. (4) Increased frequency of pulse and respiration. (5) Congested areas of the skin. (6) Enlargement of the spleen. (7) Chronicity of the case and no lesion of the heart or the kidneys detected."

Dutton then examined 150 apparently healthy children, between 1 and 15 years, in Gambia, and in the ninth case found the trypanosomes, although there were no clinical signs or symptoms present. He also found it in a child, age 3, whilst looking for malaria (ring forms), which was also present. This is the smallest of all known mammalian trypanosomes, its average length, including the flagellum, is 22 microns, average breadth 2–2.8 microns, *i.e.*, the breadth is greater in proportion to its length than in other animals. In fresh blood, with a one-sixth objective, one can see actively motile trypanosomes when present, moving between the corpuscles, and when they come to rest one can see the flagellum and the undulating membrane. The flagellum is intimately connected with the micro-nucleus, and runs along the free margin of undulating membrane situated at the anterior end; it stains a light-crimson, and is about one-third of the total length of the organism. Where does the anterior part of the body end, and the free part of the flagellum begin? Because one can always see a narrow streak of the protoplasm staining blue for some distance beneath the free part of the flagellum. The body is usually bent or curved at an angle opposite the nucleus. The anterior end is pointed, the posterior end blunter. The protoplasm does not stain evenly, taking on a basophile reaction; in some specimens one sees fine blue granules, which are usually situated around and in front of the macro-nucleus; several vacuoles may be seen as circular clear spaces. The undulating membrane is a narrow band, wavy, attached along one side, staining a faint pink. The nucleus (macro-nucleus of Plimmer and Bradford) is situated a little anterior to the middle of the body, round or oval, stains a dark-crimson, due, in older specimens, to the aggregations of chromatin

granules; sometimes it occupies the whole of the animal. The centrosome (micro-nucleus) (Bradford and Phimmer). Usually circular or elongated; about 2.5 inches from the posterior end it appears as a dark-red spot; there is nearly always a vacuole adjacent to it, sometimes close to the nucleus, sometimes anterior to nucleus (Laveran and Mesnil).

British Medical Journal, May 30th, 1903.—Manson had a case, the wife of a missionary from the Upper Congo (the tenth case on record). The blood was examined, negatively, daily for two weeks, until Dr. Daniels found the trypanosome. Manson thinks Argas monbata (a tick) is the carrier.

Of the sixteen cases reported in man, two were in an apparently healthy condition; six associated with malaria; eight had clinical symptoms of trypanosomes, and of these eight five gave similar symptoms to one another.

Castellani found trypanosomes in the cerebro-spinal fluid in twenty out of thirty-four cases. Bruce got it in lumbar puncture in thirty-eight out of thirty-eight, and, of these latter cases, in the blood in twelve out of thirteen. Bruce considers that all the stages of development of the trypanosomes take place in the human host. Leishman has reported the possible occurrence in the blood of trypanosomes in a case of dum-dum fever.

Blood examination showed no marked anemia, no dividing forms met: red blood corpuscles, 3,850,000; white blood corpuscles, 12,000 per c.c.m.; lymphocytes increased at expense of polymorphonuclears in proportion of 50 per cent. of latter to 40 per cent. of former. Cultivation of trypanosomes (Trypanosome Lewisi).

Journal of American Medical Association, pp. 1266 to 1268.—McNeal and Novy used a mixture of defibrinated blood and agar at 45 degrees C., equal parts. Slant the tube to get fluid of expression (trypanosomes only grow in this fluid of expression). Incubated at 37 degrees C. the cultures grow quickly, but rapidly degenerate owing to the alteration of the blood medium. At 20 to 25 degrees C. in the cool incubator, or 18 to 20 degrees C. in a dark room, get numerous colonies at the end of three weeks, and they remain alive for several months; put on caps to the tubes to prevent evaporation. Also have grown try. Brucei, surra in Philippines, during 1904. Tr. Lewisi has been cultivated for twenty-six generations during two years, and still retained its vitality. This is a most remarkable advance in science, to be able to grow protozoa artificially in a test tube, and opens up a very wide field in the future.

British Medical Journal, September 17th, 1904.—Rogers described the development of flagellated organisms from Leishman's bodies, in kala azar. He used sodium citrate; 5 ccm. of blood with 1 ccm. of 5 per cent. sodium citrate soln to prevent coagulation, and kept the mixture at 22 degrees C. He saw a multiplication of the parasite, and their conversion into flagellate-like organisms, without an undulating membrane.

Lancet, January 7th, 1903.—Chatterjee repeated Rogers' experiment successfully. On first day he saw three to four Leishman-Donovan bodies in every field of a stained specimen. On second day 200 to 300 in each field; the centrosome was larger, and the cell contents were more granular. On third day elongated bodies with flagella could be seen, and later fully-developed trypanosome-like bodies. The parasite differed from the ordinary trypanosome in the following respects:—(1) Micro nucleus was at anterior end. (2) Long thick anterior flagellum. (3) Absence of undulating membrane. (4) Rudimentary post flagellum. Compare Smedley's experiments at Cambridge, previously reported in rat trypanosome; practically identical with Rogers' kala-azar experiments.

Staining sections, Halberstædt—

1. Fixed tissues, twelve to fourteen hours in glacial acetic acid and a concentrated watery solution of corrosive sublimate, *e.g.* :—Mercuric chloride conctd. watery solu., aqua destillata a.a. 1500, acid glacial acetic 40 ; wash in running water twenty-four hours, embed in paraffin.

Stain thin sections with polychrome methylene blue ten minutes, wash in water. Absolute alcohol—Xyol very quickly. Tissues very deeply stained blue, trypanosomes pinkish.

2. Leishman's stain.—If the film is old, recoat it with human serum, stain with Leishman's stain ; can wash off, if not satisfactory, with absolute alcohol, and then restain with Leishman's stain ; also can apply it for tissues in section.

3. Hæmatoxylin.—The best for permanent specimens.

4. Weak carbol.—Fuchsin 1 to 10, stain ten minutes for diagnosis.

Prophylaxis.—(1) Kill all infected animals. (2) Kill all rats and mice. (3) Kill all stinging and biting insects. (4) Kill all game and wild animals. (5) Kill all fleas, lice, bugs. (6) Sunlight kills (Laveran and Mesnil).

Blood-sucking insects are the carriers and are not the intermediate hosts. Schaudinn says that the halteridum in the owl is the sexual stage of a trypanosome (Tr. noctuæ), which undergoes a complex form of multiplication both in the gnat and in the blood of the owl, giving rise to the sexual forms of Halteridum.

Treatment (*British Medical Journal,* Thomas, 1905, p. 1140).—Laveran uses sodium arsenite for animals. Boden Fowlers' solution in homo, with very promising results. Ehrlich trypanroth, especially in mal de caderas ; but it causes nephritis and necrosis if given in big doses. Wendelstadt malachite green ; too dangerous, on account of necrosis. Arsenic kills trypanosomes free in the blood, but it must be repeated if it gets eliminated or fixed in the tissues, else they start multiplying at once.

Atoxyl-aniline C6 H6, No. 2, is very good, because less toxic, and causes no necrosis. A combination of trypanroth and atoxyl in intra-muscular injections is very good, and very little necrosis results. Serum therapy—With ordinary human serum death is retarded. No cure for disease.

SLEEPING SICKNESS.

British Medical Journal, August 20th, 1904.—Dutton, Todd, and Christy classify cases according to symptoms. 1. Cases with no definite symptoms. 2. Cases with a few symptoms (temperature rises in evenings). 3. Fatal cases with marked symptoms, with sleep symptoms, no sleep symptoms. (*a*) Fever. (*b*) Lassitude. (*c*) Weakness. (*d*) Wasting.

The patient may harbor the trypanosome one year without symptoms. When symptoms develop the patient lasts two to four months. Secondary infections are common, thirteen out of thirty-two *post Mortems* showed this, especially purulent meningitis.

The number of parasites found bears no relation to the severity of the disease.

Post Mortem.—Trypanosomes found in pericardial, pleural, and peritoneal fluids ; not infective to animals (Stephens).

British Medical Journal, ibid.—Manson reported that the entire population at the southern end of Victoria Nyanza, in German territory, is affected with trypanosomes, yet no sleeping sickness there.

If this is so, then trypanosomes cannot be the only cause of this disease. It is a determining cause, the active cause being bacterial—a streptococcus in Castellani's case.

Ibid.—Bruce; trypanosomes found in the blood of natives on the west coast of Africa and in Uganda; those found in sleeping sickness are identical, proved by experiments on animals.

Trypanosome fever is the first stage of sleeping sickness; neither native nor European is immune. In fifty-four out of seventy-four cases parasites found in cerebro-spinal fluid.

In the Congo district the symptom of sleep is an inconstant factor of the disease, so the term is a misnomer. The patient's expression is dull and stupid. In very advanced cases the patient dozes over the fire; he can be roused by shouting, and then relapses; eats nil. If trypanosomes get into the cerebro-spinal fluid early, get mania; mortality 100 per cent. in natives and Europeans. Sometimes natives who have trypanosomes in their blood recover, because the trypanosomes seem to get diseased and die.

Manson.—Compare Dr. Bowden's case with trypanosomes for three years and now quite recovered.

Experiments.—Have infected rats, mice, guinea-pigs, rabbits, and monkeys. Used 1·5 ccm. of infected cerebro-spinal fluid; 50 per cent. of cases negative.

Prophylaxis.—Forbid natives from sleeping sickness areas moving into any part of the country where any species of the tsetse fly is found.

In Uganda the distribution of glossina palpalis and sleeping sickness is identical; where no flies, no sickness.

Treatment nil so far, as all cases die.

THE LEISHMAN-DONOVAN BODY.

This disease is widely distributed, particularly throughout India, in Egypt and Northern Africa, and in China. In 1885 Cunningham saw these bodies in the granulation tissue from a tropical ulcer. In 1903 Wright found them in a tropical ulcer.

British Medical Journal, May 30th, 1903.—Major Leishman discovered these bodies in a film of splenic blood from the *post mortem* on a dum dum, or dam dam, fever patient. Also found in Delhi boil, Aleppo button, and Scinde sore; and all of these parasites are identical. Marchand and Ledingham discovered (in 1902) these bodies in sections of spleen, liver, and bone marrow from a patient who had been a resident in the tropics and had an enlarged spleen and irregular fever. One is particularly cautioned to look for them in cases of malaria cachexia, with an enlarged spleen, when the malaria parasite is absent.

The Characters of the Fever:—(1) The spleen is markedly enlarged, descending to the umbilicus, or even to the pelvis. (2) Irregular pyrexia—reaching to 103 to 105 degrees F. (3) Emaciation. (4) Abdominal symptoms are common—(*a*) Diarrhœa is very constant; (*b*) peritonitis from perforation of an ulcer in the intestine, three to seven cases fatal (Christophers). (5) Local phagedænic ulceration fairly common complications, *e.g.*, cancrum oris and noma vulvæ. (6) Skin eruptions. A papular eruption in the region of the thighs and scrotum in advanced cases. Papules on trunk and limbs. Small ulcers occur. (7) Hæmorrhages. Epistaxis, bleeding from the gums, purpuric eruptions. P.M. serous membranes affected.

Post-mortem Lesions.—Spleen enlarged and of firm consistence, may or may not be pigmented. Liver usually not much enlarged; aborescence on section, due to deposit of macrophages containing pigment in centre of lobules. Large intestine.—Extensive multiple ulceration. Cancrum oris, noma vulvæ, purulent peritonitis, broncho-pneumonia, septic infarcts, &c.

British Medical Journal, September 17th, 1904.—Nature of parasite.—Rogers mixed the blood of kala-azar patient with sodium citras to prevent

coagulation, and kept the mixture at 22 degrees C.; he observed a multiplication of the parasite and their conversion into a flagellate, like a trypanosome without an undulating membrane.

Appearance of the parasite.—Very small, so do not confuse with blood platelets (slightly greenish tinge and more refactile than platelets).

Stain with Leishman's stain.—Round, oval, or oat-shaped bodies, 2 to 3 microns in diameter, with two chromatin masses, a large nucleus and a small centrosome compare with the trypanosome. These two chromatin masses are usually situated opposite to one another in the long axis of the parasite. The small mass is rod or dot shaped, and usually placed laterally in the protoplasm of the parasite (which is displaced by the vacuole which takes up most of the body substance). The protoplasm stains pink or blue, and sometimes see a tail of protoplasm between these two chromatin masses.

British Medical Journal, January 23rd, 1904.—The Zooglea mass of Manson is really fragments of large macrophages. Division forms.—Cases observed where division taken place in the large nuclear mass before it does in the smaller, sometimes get as many as six division forms, the large nuclei arranged peripherally and the small centrally. Distribution in the body.—(1) In peripheral blood, very rare. (2) Splenic blood, many, especially if some of the splenic pulp is aspirated at the same time. (3) In liver puncture, blood. (4) In the portal vein, *post mortem*. Distribution of parasites in the tissues.—Immense numbers in spleen, liver, and bone marrow; to a less extent in the lungs and testes, supra-renals, and lymphatics. In skin and intestinal ulcers, parasites always intra-cellular in—(*a*) Endothelial cells; (*b*) macrophages. Mode of entry (?) through the gut. Make smear preparations, or, better, thin sections. Stain with Leishman's stain or with hæmatin.

A FEW BRIEF NOTES ON THE CHANGE OF GENERATION AND HOST IN TRYPANOSOME AND SPIROCHÆTA (SCHAUDINN).

Schaudinn has done some most interesting as well as remarkable work on the above subject. His experiments were conducted on the (*athene noctuæ*) small owl, near Rovigino (Berlin), and mosquito (*culex pipens*). I will just give an abstract, and those who want to go more fully into this subject I refer to his original work, "*Arbeiten aus dem Kaislerlichen Gesundheitsamte Band XX.*, *Heft* 3, 1904." The developing trypanosomes undergo their fertilisation in the stomach of the mosquito (*culex pipens*), where an ookinet is formed by the fertilisation of the macrogamete by the microgamete. Ookinets develop in three ways—(1) As indecisive hermaphrodite forms; (2) as female forms; (3) or as male forms. The hermaphrodite forms may, under certain conditions, become either males or females. These three forms may multiply asexually, the hermaphrodite forms by dividing into two. Through parthenogenesis, the females can reproduce the three series of forms; but, in the case of the males, their powers of development are very limited: if they do not copulate with the females they perish.

I shall reproduce Schaudinn's pictures, as by their aid at once is the development clearly understood, and I shall let the pictures speak for themselves, as no description I could give—or even giving Schaudinn's text complete—would make it so clear.

Fig. I.—*a* to *h* shows schematically the transformation of an indifferent hermaphrodite ookinet into the trypanosome stage, by a process of heteropolar mitosis. In (*a*) the complete synkaryon lies in the centre: there are also to be seen eight chromosones surrounding the darkly-stained karyosom. In the broad end are seen pigment and four darkly-stained bodies, the residue

of the process of reduction. In (*b*) the ookinet is seen to be getting rid of the residual body and pigment. In (*c*) the nucleus undergoes division, and a diaster is seen, and so two nuclei are formed. In (*d*) the left daughter nucleus is seen to be reconstructing ; the eight chromosomes are now twisted filaments. Each nucleus contains a central granule, which is connected by a fine achromatic filament with the sister nucleus. The smaller nucleus becomes the blepharoplast of the trypanosome. In (*e*) the smaller nucleus re-divides, the three nuclei being connected by a filament. In (*f*) the smallest of the three nuclei at once forms a spindle, and this spindle becomes transformed into the locomotor apparatus of the trypanosome, the undulating membrane developing at the thickened border, the central spindle being excentrically situated. The eight mantle fibres (corresponding to the chromosomes) become eight myonema, four of each of which run along either side of the flattened anterior part of the trypanosome in the ectoplasm, and, by uniting with the central spindle at the anterior end, produce the conical flagellum. The locomotor apparatus (myonema and undulating membrane) is therefore a product of the nucleus. The development of the ookinets into the female form. The female ookinet has its plasma filled with dark staining bodies, *i.e.*, reserve stuffs. The formation of the heteropole spindle takes place in the same manner as the indifferent forms.

Fig. II.—In (*a*) see the division of the nucleus, and the smaller nucleus leaves the larger, and, quickly dividing and re-dividing, forms eight small compact nuclei ; these move at once into the posterior section of the cellular body. (*b*) Each of these nuclei now produces a smaller nucleus, which remains connected with it. (*c*) Now the eight groups of nuclei gradually perish, and the large nucleus develops two smaller ones by heteropolar mitosis (*d*). This becomes the blepharoplast by forming the flagellate apparatus, as in the case of the indifferent one. All the locomotor apparatus is less developed then in the indifferent type, hence the movements of the female are sluggish, and during their growth they keep on producing more reserve stuff, thus allowing them to resist external influences, and so, under adverse circumstances, remaining the longest alive. They remain alive in the ovaries of the mosquito during winter, and develop in the eggs deposited in spring time. Another remarkable fact is through parthenogenesis the female can go back to the gregarine (ookinet) stage should the males and hermaphrodite forms perish, and from this ookinet form both male and female forms are developed.

Fig. III.—The origin of the male trypanosome from the ookinets. No reserve stuffs in the ookinets, and their plasma is more hyaline and coarsely vacuolar. As in the case of the female ookinet, a heteropolar spindle is produced, and the smaller nucleus multiplies into eight. Now in the female type these eight nuclei died, and the large nucleus divided into two : in the male type the large nucleus dies, and the eight nuclei re-divide, each forming a blepharoplast, or micro-nucleus, or centrosome (*a* to *d*). The ookinet now becomes globular, and the eight nuclei move to the periphery, the blepharoplasts being directed towards the upper surface (*e*). The blepharoplast of each group then forms its flagellate apparatus through fission of the nucleus, and eight small trypanosomes are set free (*f*). The males have a very well developed flagellate apparatus. The trypanosomes agglomerate by their posterior, and sometimes by their anterior, extremities into rosettes. All forms of the parasite affect the red blood corpuscles, growing at the expense of the hæmoglobin.

Fig. IV.—(*a* to *g*)—showing the indifferent trypanosomes in the blood of the owl. (*a*) Attachment of the youngest stages to the red blood corpuscle by the anterior extremity. (*b*) Stage of rest. (*c*) Forty-eight hours after attachment. (*d*) Migration in the gregarine-like condition ; development

of the flagella-like apparatus. (*e*) The free motile trypanosome attaching itself to a red blood corpuscle. (*f*) Condition of rest five days after the first attachment. (*g*) Fully developed trypanosome.

Fig. V.—Schematic growth of an indifferent ookinet from Spirillum Liemanni, and the formation of the indifferent trypanosomes therefrom.

Fig. VI.—Various stages of an indifferent Spirilla, from the body of a mosquito. (*a*) A single parasite greatly enlarged. (*b*) Stage of fission. (*c*) Two parasites which have remained attached by their posterior extremities after division (the actual spirillum stage). (*d*) Rosette, agglomeration by posterior extremities. (*e*) Much smaller agglomeration. (Schaudinn thinks, owing to this decrease in size of the spirilla by multiplication, that spirilla may be the exciters of yellow fever.) Supporting this close relationship of Schaudinn's between spirochætæ and trypanosomes, Theiler has found both in red water and in Rhodesian coast disease. Another case :—G. F. Petrie discovered spirochætes in the blood of a martin, and trypanosomes in its bone marrow. Schaudinn thinks that the trypanosomes, when very small, could pass through a Berkefeld filter, because it is only when they are agglomerated that they can be seen. A spirillum is a bacterium made up of a large number of conjoined vibrios, each helping to make up a spiral. The spirillum moves with a flagella. A spirochætæ is a single long thread, one cell, with pointed extremities, movement due to the contractility of its protoplasm. Since the above paper was delivered Schaudinn, working further, has shown that the probable cause of syphilis is spirochæta pallida.

In conclusion, I wish to say that I have reduced very largely the volume of my original paper on account of space for the printer, so that it reads more as an abstract: and all medical journals bearing on the subject have been freely consulted, as well as numerous authorities, the chief being—Laveran and Mesnil, Stephens and Christophers, Manson, Daniels, Smedley, Liemann, Stiles, Musgrove, &c.

The following specimens were shown :—*Macroscopically*—Lingulatula rhinaria, male and female. Pentastomum denticulatum. Tænia echinococcus infection of the intestine of a dog (artificial). Tænia medio-canellata, five worms from a patient *post-mortemed* at the Adelaide Hospital, collectively 105 feet long. Simple cyst of a liver. Multiple cysts of a liver. Multiple tubercular strictures of the small intestine. Multiple submucosal herniæ in small and large intestine. Trichina spiralis. *Microscopically*—Trypanosome Duttoni (Sir Patrick Manson). Tr. Evansi. Tr. Brucei (Dr. Low). Tr. Lewisi. Tr. Danielwyski. Schistosomum Cattoi (Dr. Catto). Leishman Donovan bodies (Dr. Low). Tænia echinococcus. Taæni echinococcus alveolaris. Bilharzia specimens, &c. Guinea worm embryoes. Filaria (five varieties). Filaria nocturna from a case in my out-patients. Trichina spiralis. Trichina spiralis, intestinal form, from Professor Virchow's original case, &c.

AN UNUSUAL FORM OF FIBRO-CELLULAR GROWTH.

By Dr. J. Thomson.

Ramah Ram, *aet.* 24, single, Punjaubee, was admitted to one of my beds in the Brisbane Hospital in June, 1905. He had a very large growth covering the top and right side—practically half—of his head, and it overhung his right eye, obstructing its vision. It was for this interference with sight he

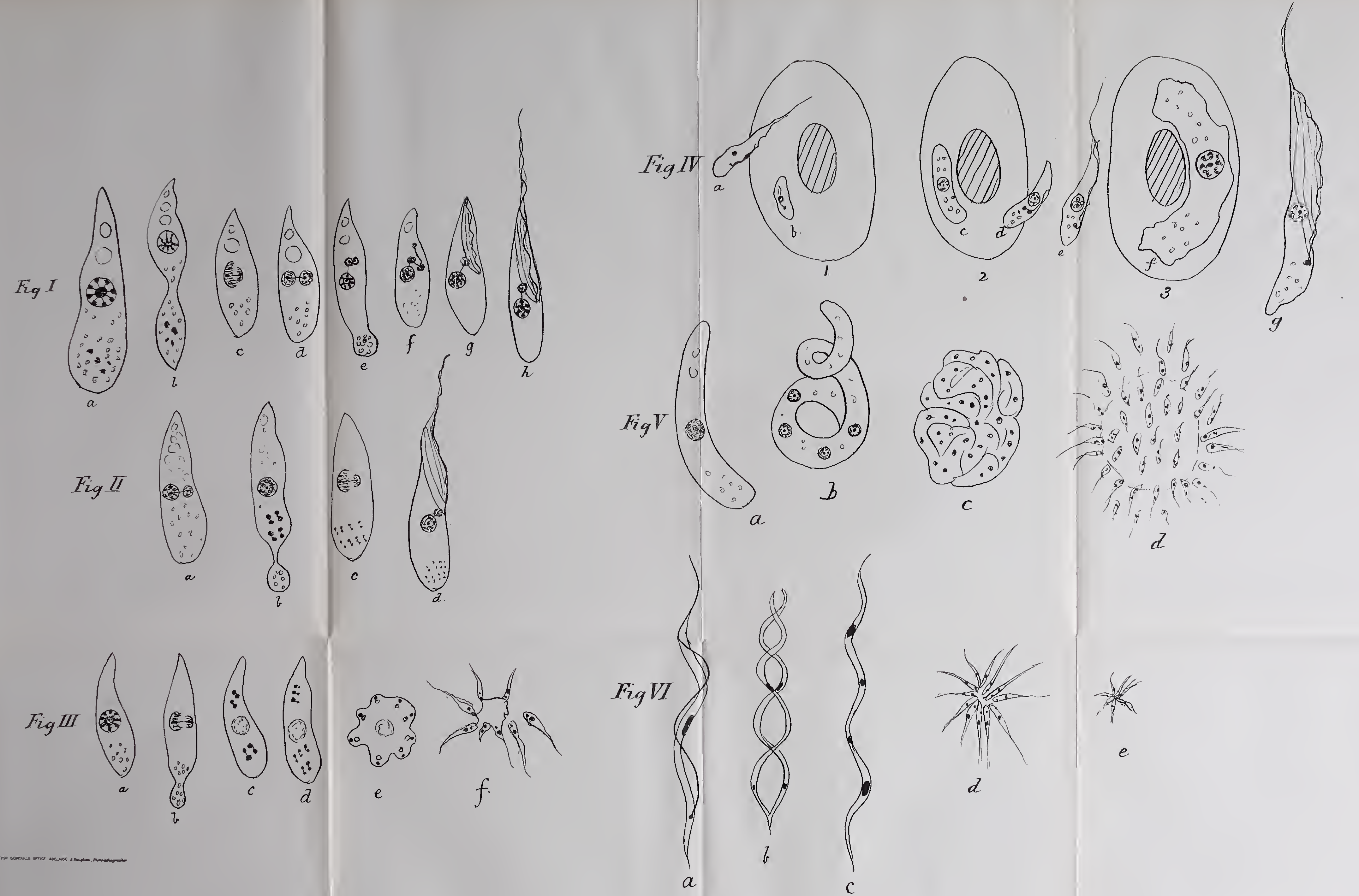
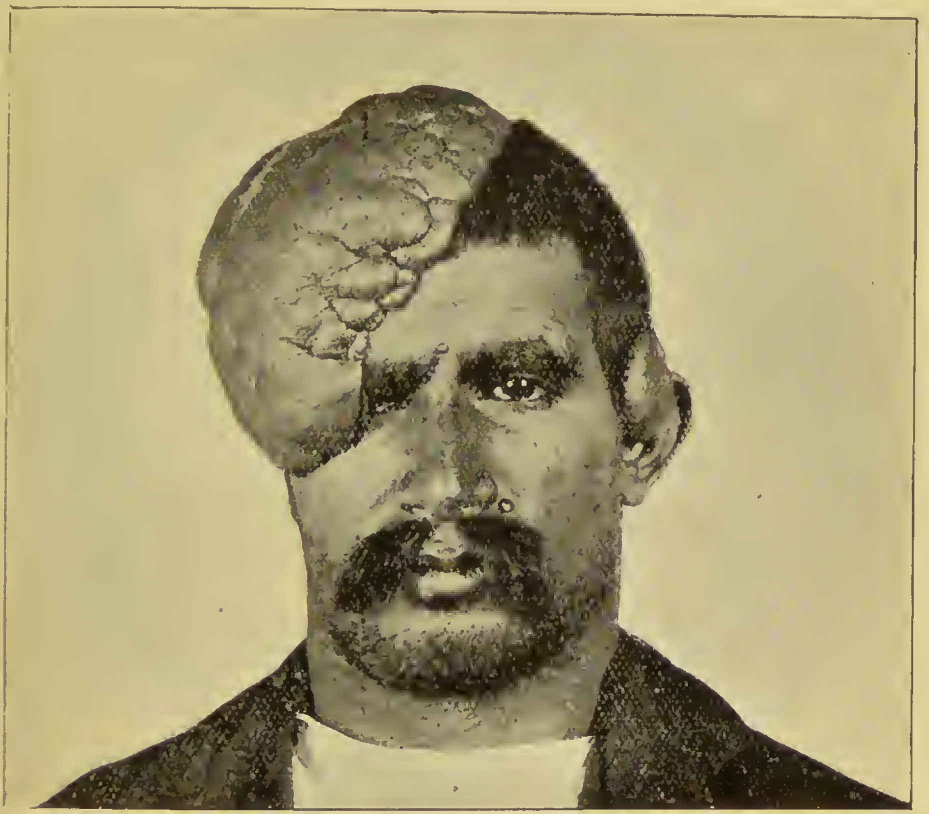


Fig I
a
b
c
d
e
f
g
h
Fig II
a
b
c
d.
Fig III
a
b
c
d
e
f.
Fig IV
a
b.
1
c
d
2
e
f
3
g
Fig V
a
b
c
d
Fig VI
a
b
c
d
e

sought relief. There was another growth, about the size of a walnut, over his left ear, and there were numerous small growths on his face, trunk, and limbs—in fact, all over him.

The large growth is dark—a blue-black—smooth, elastic, india-rubbery, freely lobulated, resembling the convolutions of the brain; devoid of hair, and, to the touch, seems warmer than the rest of the body. There is nothing tender or painful about it; and, beyond the visual disturbance, it gives rise to no inconvenience, for—after the manner of his countrymen—he wears a turban, not a hat.

He states that up to the age of 12 there was no growth, only the skin was black, smooth, and hairless. Growth then started and has continued, slowly increasing, since.



In every other respect the patient's condition is normal, and although the blood was examined on several occasions—early in the days and also late at nights—no filariæ were discovered. There have never been any febrile attack nor any conditions suggesting filariasis.

To relieve the right eye, a wedge-shaped piece was removed from the overhanging mass. This piece was about three inches long, corresponding to the line of the eyebrow, and the wedge base about one and a quarter inches broad. On section, the growth was found to be very tough and leathery; in itself it was but sparingly vascular, yet, when cut, the few vessels in it pumped very briskly, for there was no retraction; deeper down, beneath the growth proper, vessels were in plenty, and there was very free bleeding.

There was primary union, and the patient left the hospital, rejoiced that his right eye was permitted to see.

In about seven weeks he returned; the eye again covered. This was not due to any increase of growth, but to the mass being dragged, by its own weight, over the eye and on to the cheek. A second operation was performed. A larger wedge removed, similar conditions of vascularity met with, and healing by first intention obtained.

Pathological Notes.

Skin.—Stratum corneum, well marked, not desquamative. Rete Malpighii considerably narrower than usual, and not nearly so much broken up in outline by the papillæ of the dermis as is normal. Fibrous tissue more condensed at the surface, and becoming more swollen and œdematous towards the base. No sebaceous, or hair follicles, are visible. Rudimentary, or

atrophied, sweat-glands are seen. Cellular masses lie scattered through the more superficial layers of the fibrous tissue. Pigment exists in the lower layers of rete and in some of the deeper cellular masses. In the deeper layers the cells mostly disappear and the fibro-elastic tissue becomes more swollen. Some pigment is also seen.

Microscopically the above description corresponds with the structure of fibroma molluscum, though different clinically. The structure does not appear to be rich in blood or lymph vessels, though the succulent nature of the deeper fibres suggests Elephantiasis, which is, however, not borne out by the presence of isolated nodules on body.

Conclusion.—A fibro-cellular growth, allied to Fibroma Molluscum in structure, or possibly Elephantiasis of the scalp; the absence of filaria in the blood would tend to negative the latter view.

The only case that I know at all similar to this is the one reported and figured by Dr. Allardice Macdonald in *The Journal of Tropical Medicine*, April 1st, 1905, and described as "Elephantiasis of the scalp." Pictorially the two cases appear identical, and very probably they are in most other respects. Macdonald's patient suffered from the periodic febrile and other disarrangements peculiar to filariasis, and the nematodes existed in his blood in great numbers.

It is not actually so stated, but it is inferred that the tumor—the elephantiasis—of the scalp had a filarial origin.

In Macdonald's patient the growth is solitary; in mine, there are many growths—they are multiple.

My patient has no filariæ, nor has he, at any time, had any of the constitutional disturbances created by their presence; therefore there is no reason to suspect that his growths are due to blood worms.

For the clinical notes I am indebted to Dr. T. G. Ross, and for the pathological to Dr. Wilton Love.

A NOTE ON THE FREQUENCY OF STREPTOTHRIX INFECTION IN MAN.

By Professor D. A. Welsh, M.A., B.Sc., M.D., and J. E. V. Barling, M.B.

Within the last eighteen months four specimens of pus have been brought to the Pathological Department of the Sydney University, in which the presence of a streptothrix has been demonstrated. In addition, there have been one or two cases at least during the same period in which, though the clinical features clearly pointed to actinomycosis, yet the organism was not found in the pus; probably owing to the fact that in the earlier cases Gram's method of staining was not used. All the cases occurred in Dr. MacCormick's practice, the specimens having been obtained through him.

Three types of the disease were represented in the four cases, in which the presence of a streptothrix was definitely ascertained. In the first place, in two of the cases the infection had evidently taken place through the mouth, the resulting lesions taking the form of hard, brawny, inflammatory swellings. In one of these cases the swelling was relatively small, being situated below the left malar bone, and involved the tissues of the cheek, the bone apparently escaping: the other case was more extensive, the swelling reaching from the upper part of the temporal region down to the middle of the neck on the right side. Scattered throughout the mass were small shallow abscesses. Subsequently, at the operation, portion of the lower jaw was found to be involved in the inflammatory process. In both these cases the pus contained the characteristic sulphur grains.

Secondly.—In one case the infection was a pulmonary one. The trouble started with an attack of pleurisy over the left base, followed later by the development of a cough and the expectoration of a good deal of sputum, with small amounts of blood. Later, several small subcutaneous swellings appeared over the left base, which, on being opened, discharged a thick yellow pus, which contained sulphur grains.

Thirdly.—In one case the infection was an abdominal one. The process started as an inflammatory attack in the region of the appendix, associated with the appearance of a mass along the course of Poupart's ligament, on the right side. At the operation the appendix was found buried in a mass of recent inflammatory material, with a perforation about its centre. There

was no pus present. Later an inflammatory mass developed in Douglas' pouch, which, however, disappeared in the course of three weeks without suppurating. Subsequently a small pocket of pus pointed in the left nipple line, just below the costal margin, which was found to communicate with a large abscess cavity behind the spleen. Later an abscess developed between the diaphragm and upper surface of the liver, on the right side. It was in films of the pus taken from this situation that the organism was first detected, though no characteristic grains were present. During the course of the illness the appendiceal wound gave occasional issue to small amounts of a thick creamy pus, in which were present typical sulphur grains and in film preparations of which the presence of a streptothrix could be readily demonstrated. Just prior to the fatal termination of the disease the subcutaneous tissues of the abdominal wall above the umbilicus became thickened and infiltrated with inflammatory products.

As to the staining reactions and appearance of the organisms in film preparations :—As regards their staining the most noticeable feature was the difficulty experienced in getting them to take up the ordinary dyes—such as methylene blue, thiouin blue, or weak ziehl. It was owing to this fact that the organisms were overlooked on one or two occasions. They, however, stain well by Gram's method, and all suspected cases should therefore be treated in this way. In specimens stained thus the branching mycelial filaments present a characteristic appearance. The branches are usually short, and disposed laterally along the course of a filament. In some cases the ends of the branches were bulbous. In none of the cases was the " ray fungus," or actinomycotic arrangement, observed in the colonies, branching mycelial filaments being the only elements noticed.

The cultivation of the organisms in artificial media presents some difficulty. In three out of the four cases, however, pure cultures have been obtained, though their full cultural characteristics have not yet been worked out. The following points may, however, be noted :—

First.—Their comparatively slow growth in the ordinary culture media.

Secondly.—As pointed out by Fullerton and Jones, they grow best in media which contain maltose.

Thirdly.—As also noted by Fullerton and Jones, when cultivated in maltose broth they produce granular masses of growth, which, settling down to the bottom of the test tube, produce no turbidity in the medium.

Fourthly.— There are indications that they have a leaning towards anærobic conditions.

As regards the mode of infection, in two of the four cases there was a history of the patients being in the habit of chewing either wheat grains or the straw of wheat previous to the onset of the symptoms.

SECTION OF PUBLIC HEALTH.

SOME LESSONS FROM THE STATISTICS OF INFANTILE MORTALITY IN SYDNEY.

By W. G. Armstrong, M.B., Ch.M., Sydney; D.P.H., Cambridge.

Gentlemen—Before beginning my address I wish to gratefully acknowledge the distinguished honor which has been conferred upon me of election to the Presidency of this Section—an honor which, I am well aware, is due to the position I occupy in the Public Health Service of the State of New South Wales, and not to any personal merits of mine.

The subject I have chosen for my address is a departure from the custom often observed in presidential addresses, in that it deals with a special subject rather than with a general review of progress in the work and knowledge peculiar to this Section. I trust you will pardon the innovation.

It will not be out of place to begin this paper by drawing a comparison between the infant mortality of England and Wales and that of New South Wales. Table I. gives the materials for such a comparison. This table gives the mean of ten years' statistics, and has been extracted from the annual reports of the English Registrar-General and the New South Wales Government Statistician. Corresponding figures for the city and suburbs of Sydney have been introduced into the table, showing the effects of urban conditions upon infantile rates. The rates in the table, as throughout this paper, are calculated on the number of deaths under one year of age per 1,000 births, which is the most accurate method of stating such deaths.

There is a great contrast between the total infantile mortality of England and that of New South Wales. The former exceeds the latter by 44 per cent. and is 25 per cent. greater than the infant mortality of Sydney. This difference is one of relatively recent growth, as in former years the infantile mortality of New South Wales exceeded that of the mother-country. With so great a difference in the totals, it is not surprising that most of the component rates should be in favor of New South Wales. As a matter of fact, under the thirteen different headings which compose the totals in the table, there are only two under which the English infantile death rates do not exceed those of New South Wales. The two in question are diarrhœal diseases, with an English mortality of 29.1 per 1,000 births, as against 30.7 per 1,000 births in New South Wales; and congenital defects, in which the rate (3.9 per 1,000 births) is exactly the same in both England and New South Wales. Very striking differences are observed in the mortality from pneumonia and bronchitis, which is nearly 150 per cent. greater in England than in New South Wales, and in those from tubercular diseases and convulsions, both of which are in England more than double the New South Wales rates.

That the death rate from respiratory affections should be greater in a country with the climatic conditions prevalent in the British Isles than in New South Wales is not astonishing, but the great excess in deaths from convulsions is more difficult to account for, and, it appears to me, must be due

to divergences in the practice of certification or in classification. The relative superiority of the position of New South Wales as regards infantile deaths from tubercular diseases cannot, I think, be dissociated from a lesser prevalence of tuberculosis among Australian dairy cattle. But probably the most important factor is that the standard of living among the poorer classes in Australia is higher than among the corresponding population of Britain. Among the causes predisposing to tubercular disease a food supply insufficient in quantity, or defective in quality, ranks high. It is to be noted that thirty years ago the infantile death rate from tubercular diseases was higher in New South Wales than in England—a statement which is equally true of the general infantile mortality. During the past thirty years the English infantile mortality has actually shown a tendency to increase, while that of New South Wales has, in the same period, declined considerably. Table II. shows that in Sydney and suburbs the fall has been continuous and fairly regular since the year 1886, when it stood at 173 per 1,000 births, until 1904, when it reached the minimum of 98 per 1,000 births.

The influences which bring about infantile mortality may be classified generally as (1) hereditary, (2) intra-uterine, and (3) post-natal. To what extent each of these groups of causes is severally responsible for the infantile mortality of cities is extremely difficult to determine. As regards the first and second groups, very little effective control can be hoped for in the present state of our knowledge; and it is noteworthy that deaths classified under two headings which fall within one or other of these groups, namely, prematurity and congenital defects, have latterly shown a slight but undoubted tendency towards increase in Sydney (Table II.).

The most important, numerically, of the causes of death among children under twelve months of age in New South Wales, as in Great Britain, is diarrhœa. Diarrhœal diseases, which include epidemic or summer diarrhœa and enteritis, as well as the more sporadic forms of fatal diarrhœa which occur from week to week throughout the whole of the year, are annually responsible in England for 29.1 deaths in every 1,000 births, and in New South Wales for 30.7 deaths; while in Sydney the number of children who succumb during their first year to this cause is not less than 40.9 out of every 1,000 births.

Apart from rigid mathematical or experimental proof, there are few facts better established than the intimate connection between infantile diarrhœa and improper feeding. Ernest Holt, in his "Diseases of Infancy and Childhood," emphatically expresses the dictum, "This disease prevails to the extent to which other food than breast-milk is given to infants": and the weight of all authority, and of common experience, points unmistakably in the same direction. But the influence of the food factor is not only shown in the deaths from infantile diarrhœa.

The cause of infantile deaths which is, numerically, of second importance to "diarrhœa" is that which is classed by the statistician under the heading of "atrophy, debility, and inanition," all of which expressions are of course synonymous for the same pathological condition—or, rather, for the same clinical group of symptoms. Looked upon as a fatal disease, they were responsible for 17.6 infantile deaths in every 1,000 births in Sydney during the past ten years.

Atrophy, debility, and inanition are all very loose terms, and but little superior in accuracy to "dropsy," since they ignore the primary cause of the physical condition which they indicate. In so far as they convey any information, it is that of inability to assimilate food, a pathological condition which frequently—probably in the majority of cases—is a sequel to an attack

of acute diarrhœa. Even in those cases in which the condition has not been ushered in by an attack of acute diarrhœa, clinical evidence is strong on artificial feeding as a cause.

Let me quote Holt again. Speaking of marasmus—another synonym for infantile atrophy—he says, " In the vast majority of cases it depends upon two factors—the food and the surroundings . . . . The following is the story most frequently told at the hospitals. ' At birth the baby was plump and well-nourished, and continued to thrive for a month or six weeks, while the mother was nursing it ; at the end of that period circumstances made weaning necessary. From that time the child ceased to thrive. It began to lose weight and strength, first slowly, then rapidly.' "

Eustace Smith sets forth the four principal causes of infantile atrophy thus :—(1) unsuitable food, (2) chronic vomiting—gastric catarrh, (3) chronic diarrhœa—intestinal catarrh, (4) constitutional diseases. The same opinions are expressed by every author of repute.

Looked upon as causes of death, infantile convulsions and teething may also be regarded as due to artificial feeding. The latter—which, I am glad to point out, is rapidly disappearing from Sydney statistics as a cause of death— ought certainly never to be so assigned in any certificate. That it still is given occasionally on the certificate is a satirical reflection on the scientific medicine of the present day.

The diagram which I have prepared from the statistics published by the New South Wales Registrar-General and Statistician represents the mean monthly deaths from diarrhœal diseases (diarrhœa, cholera, dysentery, and enteritis), atrophy, convulsions, and dentition at all ages for the past thirty years in Sydney and suburbs. Each vertical square on the diagram represents a rise or fall of 10 per cent. above or below the mean. It should be premised that the curves ought to have been constructed on weekly figures, and for infantile deaths only ; but the data necessary for these are, I regret, unavailable. The curves are interesting. In the first place, they all closely resemble one another in their salient outlines. Practically, at their minimum level during the months of June, July, August, and September, they begin to rise in October. Diagram number 2, which is compiled for three years on the only weekly data which are available, shows that the rise in diarrhœa does not begin until after the middle of October, and continues to rise during November and December. The diarrhœa and atrophy curves attain their apex in this latter month, and show a beginning of a fall in January ; but the curves of dentition and convulsions are at their highest in January. The curve of convulsions shows another departure from the common type in a slight fall in December, which is an intensification of a tendency in the other three curves to fall away slightly in December, after their abrupt November rise. From January to February there is a marked fall in all the curves, and thereafter a more gradual decline to the minimum. Another point of difference between some of the curves is the lesser amplitude of departure from the mean observed in the curves of atrophy and convulsions, when compared with those of diarrhœa and dentition. But the close general similarity of the curves thus demonstrated is strong presumption in favor of identity of etiology of the diseases classed under these several heads, and clinical observation further supports this presumption, so that I think I am justified without further laboring of the subject in treating all these assigned causes of infantile deaths as essentially one. Let us now examine Table II., and see how this is borne out by the vital statistics of the infantile population of Sydney during the past thirty years. In the table causes of death which are intimately connected with the feeding of children have been placed in juxtaposition. Diarrhœa, as a cause of death,

actually shows an increasing mortality during the past thirty years; whereas the headings of convulsions, dentition, and the various synonyms for atrophy, all show a strongly diminishing tendency. Convulsions, as an assigned cause of death, has decreased from 18.6 deaths per 1,000 births in 1875 to 3.2 in 1904, or 83 per cent; dentition from 8.4 to 0.7, or 92 per cent; and atrophy, debility, and inanition from 43.6 to 11.4, or 74 per cent. These enormous falls would be quite incomprehensible except on the supposition that increased accuracy of diagnosis and certification has gradually transferred most of the deaths, which would formerly have been loosely attributed to them, to some other heading; and a brief examination of the table leads to the conclusion that the only headings to which such deaths could have been transferred are those of diarrhœa or enteritis. And this supposition probably exactly represents the actual facts, except that the sudden diminution in deaths from dentition between the years 1892 and 1894 suggests the introduction of a modification in the system of classification adopted by the Registrar.

We shall therefore get a clearer mental picture of the statistics of infantile mortality due to improper food as exemplified in Sydney if we combine the mortality from the several causes—diarrhœa, convulsions, dentition, and atrophy. This I have done in the last column of Table II., and the result is a column of figures which represents a mortality having a slight tendency to increase between the years 1875 and 1886. After the latter year the mortality from the combined causes began to fall, and has continued to diminish in a tolerably even curve until the present time: we shall inquire into the probable causes of these phenomena presently. It is advisable at this point to clear the ground by expressing the opinion that a review of the causes of infantile mortality, having any useful end in view, must practically resolve itself into an investigation into the circumstances connected with the mortality from diarrhœa and those other causes of death which depend upon the defective food supply of infants. Prematurity and congenital defects, as causes of death, we can hardly at present hope to modify. Tubercular diseases as a factor in infantile mortality have gradually, during recent years, lost most of their significance. Respiratory diseases, as represented by pneumonia and bronchitis, evidently depend almost entirely upon seasonal influences; and though the mortality from these diseases fluctuates widely from year to year, the oscillations depend chiefly upon the character of the seasons, and the means of a series of years show little movement either of advance or of retardation. Zymotic diseases, other than diarrhœa, constitute the other important heading. The infantile deaths under this column are due for the most part to measles and whooping cough. There has been a marked fall in infantile deaths under this heading, as shown in the table, and the present rate in Sydney is between 5 and 6 per 1,000 births.

The scheduled causes of infantile deaths which depend upon improper food, or dietary diseases, as they may be called, account for about one-half of the total infantile mortality. They have, therefore, a far more important influence in determining the amount of infantile mortality than any other group, or combination of groups, of diseases. The facts that they are admittedly dependent upon improper feeding, and that their fatality has largely diminished and is still on the down grade, argue that, as causes of death, they are controllable. Can they not be more thoroughly controlled?

Dr. Hope, the Medical Officer of Health of Liverpool (England), recently remarked that in cities an infantile mortality of 100 deaths per 1,000 births might be regarded as an ideal minimum of inevitable deaths during the first year of life. This dictum, of course, applied to England, and to English conditions. In Australia (the statement is based upon Sydney figures, but I have

no doubt they are, on the whole, applicable to the other large towns of Australia), where the infantile deaths from tubercular, respiratory, and zymotic diseases are very much less than the English experience, the figure for infantile deaths which must be regarded as unavoidable should be considerably lower than the standard set up by Dr. Hope.

It is not my intention, in this short address, to enter at all deeply into the question of the influence of meteorological conditions upon the mortality from dietary diseases. Ballard, in his classical report to the English Local Government Board, in 1888, went very fully into this subject, and his conclusions are well known. His "working hypothesis" connected the essential cause of diarrhœa with the life process of some unknown micro-organism resident in the more superficial layers of the soil, the vital manifestations of which are dependent, among other things—perhaps, principally—upon conditions of season, and on the presence of dead organic matter, and which finds, especially at certain seasons, nidus and pabulum in human food where it manufactures a substance which is a virulent chemical poison. This "working hypothesis" up till the present time sums up the knowledge we possess as to the essential cause of diarrhœa. We have made but little advance in this direction.

Other propositions of Ballard's are certainly not applicable to Sydney conditions, however true they may be in England. His conclusions on the relation between the summer increase of diarrhœal mortality, and the temperature registered by a 4-foot earth thermometer, do not hold good here. There are, it is true, no records of the temperatures registered by the 4-foot earth thermometer in Sydney, but there are complete records of the temperature of the earth 5 feet below the surface for the past thirty years, and these differ so slightly from the 4-foot observations that they may be regarded as practically identical. Ballard's observations referred to are as follows :—

(*a*) The summer rise of diarrhœal mortality does not commence until the mean temperature recorded by the 4-foot earth thermometer has attained somewhere about 56° F., no matter what may have been the temperature previously attained by the atmosphere :

(*b*) The maximum diarrhœal mortality of the year is usually attained in the week in which the temperature recorded by the 4-foot thermometer attains its mean weekly maximum :

(*c*) The decline of the diarrhœal mortality . . . . coincides with the decline of the temperature recorded by the 4-foot earth thermometer, which temperature declines much more slowly than the atmospheric temperature.

In Sydney a comparison of the records of 5-foot earth temperatures for thirty years with the weekly records of diarrhœal mortality shows (1) that in many years the 5-foot earth thermometer does not register below 56° F. at any time, consequently that temperature cannot be regarded as a critical one; (2) that the maximum diarrhœal mortality of the summer in Sydney is reached in December, whereas the mean weekly maximum of the 5-foot earth thermometer is generally not reached till March, and never before the end of January; (3) that the decline of the summer diarrhœal mortality in Sydney does not begin till March. All these points are shown in the attached diagram, which exhibits the rise and fall in the diarrhœal mortality in Sydney and suburbs, and the variations in the meteorological data, from week to week during the years 1902-1905.

While, however, this is the case as regards the earth temperature, the relation of shade temperature to diarrhœal appears to merit investigation.

The summer rise in diarrhœal mortality in Sydney usually begins about the last week in October or the first week in November, about a fortnight

after the mean temperature in the shade permanently attains a height of 60° F., and the diarrhœa rise continues to about the middle of December, when the apex is attained, and the descending curve begins, falling before the apex of the mean temperature curve is reached (during January and February). Having once begun, the fall in diarrhœal mortality continues evenly. apparently without reference to the temperature curve, until the minimum of the mortality curve is reached in September. Thereafter a slight rise occurs in October, but the real rise is deferred till November.

If one allows for the average duration of illness in fatal cases of diarrhœa, it will appear that the beginning of the summer epidemic manifestation of diarrhœa in Sydney is very nearly coincident with the time at which the mean temperature in the shade touches 60° F. Out of 279 cases of fatal diarrhœa in infants in the city of Sydney, which I investigated during the years 1902-1905, the average duration of illness was found to be twenty-eight days, and the duration of illness in individual cases was as follows :—

During the	first week	of illness	107	died, or	36.2	per cent.	of all cases
"	second	"	58	"	20.8	"	"
"	third	"	35	"	12.5	"	"
"	fourth	"	19	"	6.8	"	"
"	fifth	"	5	"	1.8	"	"
"	sixth	"	11	"	3.9	"	"
"	seventh	"	4	"	1.4	"	"
"	eighth	"	16	"	5.7	"	"
After the	eighth	"	30	"	10.7	"	"

Allowing for two days' delay in registration of the deaths, which appears to be about the average, and a very short incubation period, it would seem from the above figures that about a week after the critical mean temperature in the shade, if there be one, has been recorded, a response should appear in the rise of the curve in the registered mortality from diarrhœa, and this is seen to be the case after a mean temperature in the shade of 60° F. has been reached.

When the epidemic mortality has once received a start, it gathers force rapidly throughout a period from three to four weeks, after which it begins to decline without further relation to the shade temperature curve. A fairly good simile for the phenomenon is the pressing of the trigger of a gun, and the outburst diarrhœal mortality is the explosion which follows. The best expression of the facts which I have come across in medical literature is one by Ernest Holt, who says, " It appears that an average mean temperature . . . of about 60° F. is required to start the epidemic. Not many cases are seen until such a temperature has lasted for some days. The explanation of the high mortality appears to be . . . . that the majority of the susceptible infants are unable to withstand the first very hot month. Humidity and rainfall, according to the most careful investigations of Siebert in New York and Baginski in Berlin, do not influence either the prevalence of summer diarrhœa or its mortality."

The last statement of Holt's is striking, in view of the contrary opinions held by most English authorities, notably Hope, of Liverpool, and Newsholme, of Brighton, on the influence of rainfall upon summer diarrhœa. Personally I have always held the opinion that copiousness of rainfall during the hot summer months bears a very close inverse relationship to the prevalence of diarrhœa and cognate disorders ; and, without being able to produce any convincing statistical evidence to back this opinion, I must confess to retaining it still. But an investigation into the records of meteorological data in Sydney for twenty-eight years, and their coincidence with fluctuations in the summer mortality from diarrhœa is disappointing. All one can definitely gather

from them is that, on the whole, diarrhœal mortality has been higher in dry and hot summers than in cool and wet ones, and *vice versa*. It appears to be very difficult, if not impracticable, to separate the influences of drought and heat in this connection. Even when they are combined, and both excessive, there are exceptions to the general proposition that under such circumstances the mortality from diarrhœa is coincidently high.

For instance, the summer of 1904-5, with the highest mean temperature in the shade (with one exception, that of 1895-6), and the lowest rainfall (without exception) for twenty-eight years, also witnessed the lowest diarrhœal mortality experienced in Sydney for twenty-eight years. But whatever may be the effect of meteorological conditions upon the incidence and fatality of diarrhœa and its cognate causes of mortality, there must be some other cause or causes at work of almost equal or even greater importance. If not, there could be no progressive change in the mortality from this group of diseases such as has been experienced in Sydney during the past thirty years ; and this is a most comforting reflection, otherwise we should have a very hopeless task set us in the reduction of infantile mortality from these causes, since we have no control over meteorological conditions.

I have already drawn attention to the course of movement shown by the Sydney mortality curve from diarrhœa and its kindred diseases, and indicated that the level of the curve showed no particular tendency towards movement in either direction until after the year 1886, when a downward movement set in which has been progressive until the present time. In the eleven years prior to 1886 the infantile mortality rate from those diseases which I have discussed fluctuated between 84 and 111 per 1,000 births, with a slightly higher tendency towards the close of the period in 1886 than at its beginning, in 1875. Since 1886 the rate has fallen, till in 1904 it stood at 46 per 1,000 births. What have been the causes at work in Sydney which have so reduced the rate ?

Infantile diarrhœa is, no doubt, directly due to food infection. This much is proved by the relative immunity from the disease of breast-fed children. In a series of sixty infants under three months of age who died from diarrhœa in the city of Sydney during 1902-5, and whose cases I investigated, I found that only 6.6 per cent. had been entirely breast-fed, while 93.4 per cent. has been wholly or partially fed on other foods. During the year 1904 I made an inquiry into the methods of feeding of 621 living infants under the age of three months. The results showed that 72 per cent. were entirely breast-fed, and the remaining 28 per cent. were wholly or partly fed on other foods. If these sets of figures are comparable—as I have no doubt, in the main, they are, since my inquiry into the feeding of living infants was conducted in the districts and among the classes of population which chiefly furnish the diarrhœal mortality—the mortality among infants under three months old fed wholly or partially otherwise than on the breast is, in Sydney, thirty-seven times as great as among infants of the same age wholly breast-fed. This startling discrepancy is only to be accounted for on the theory that, by some means, the essential cause of diarrhœa reaches the infant through its food. Food, however, is only to be regarded as the vehicle of the infection. Ballard regarded the upper layers of soil as the *nidus* of the infective material, and a large mass of evidence which has been accumulated since his classical report was published has done nothing to weaken his hypothesis. Perhaps the weight of evidence is rather in favor of the surface of the soil instead of its upper layers being regarded as the *habitat* of the diarrhœa organism or organisms, and diarrhœa is now rightly considered as a filth disease, and every movement in domestic and municipal sanitation which makes for cleanliness is to be looked upon as one step towards the diminution of this most fatal disorder.

The sanitary history of Sydney during the past thirty years lends support to this view. Prior to the year 1888 the water supply of Sydney was obtained from the Botany swamps, situated about five miles south of the centre of the city, in a low-lying peaty and sandy morass, on the shore of Botany Bay. The catchment area and conservation beds were liable to pollution from many sourees, and the district of Botany itself has been for many years a hot bed of diarrhœa and typhoid fever. Since the year mentioned, the water supply of the city and suburbs has been obtained from a catchment area situated on mountainous sandstone country about fifty miles to the south-west of Sydney —a source which is almost unimpeachable on sanitary grounds.

The change in the system of sewerage in Sydney has been no less important than that in the water supply. In 1877 the system of sewers in the city was about as bad as it was possible to be. The sewers were of various sizes and shapes, eonstructed in brick and stone, and all discharged into the harbor. The tank stream sewer, running through the centre of the city, and entirely built over by dwellings and business places, was in a foul dilapidated state: the sewage stagnating along an uneven invert, while the sewer air found vent through innumerable unplanned crevices and openings, not to mention the unventilated house drains. The suburbs were entirely unsewered, and cesspits of the foulest description, and the most insanitary construction, were common both in city and suburbs. In that year the sanitary arrangements of the city were considered to be so defective that the Government of the day appointed a board to report on the subject, and the report brought to light many glaring evils. As a result of this report, the present excellent system of sewers began to be constructed in the year 1885, and was steadily pushed forward until the year 1897, when the system serving the city was completed. Even in the year 1890 there was still a eonsiderable number of filthy cesspits in the city; and in the suburbs, as late as 1899, there were known to be 1,559 of these abominations, and probably as many more existed unaccounted for. Almost all these insanitary conditions have now been removed, and the city and all the more populous suburbs of Sydney are as well off as regards sewerage and water supply probably as any large city in the world.

In the year 1886 the Dairies Supervision Act came into operation, and was enforced year by year with gradually inereasing stringency, improving vastly the conditions of the metropolitan dairies, and, as a consequence, raising the quality of the milk supply.

Finally, the advent of plague in 1900 caused the municipalities of the metropolitan district to exercise their powers to make by-laws for the suppression of filth on premises, and to enforce them when made, and generally to put their municipal houses in order with such vigor as was before undreamed of.

It might have been anticipated that all these advances in sanitary conditions would have had a salutary effeet on the infantile death rate from diseases caused by food pollution, and the coincidence of sanitary improvement with progressively falling death rates is so striking as to be convincing evidence of the casual relation of the one to the other.

But there is one field of municipal endeavor in the prevention of infantile mortality, the soil of which has been but lightly stirred hitherto, but which, with diligent tilth, should yield a goodly crop. It is that necessary to break the vicious chain of events which carries the infective material from the soil into the stomaeh of the infant by preventing the pollution of the food supply, *i.e.*, the milk. The Dairies Supervision Act has already proved a step in this direction in Sydney. By enforcing many improvements in the construction of the dairies and milk shops it has lessened greatly the opportunities which

existed for filth and micro-organisms to find their way into the milk. A vigorous enforcement of the adulteration of food clauses of the Public Health Act has also been of avail. The suppression of the practice of adulterating milk by the addition of water has diminished the chances of micro-organisms finding their way into the indispensable food of young children, and the prohibition of the use of preservatives has compelled the milk vendor to sell fresh milk only, and limited his opportunities of passing a stale and germ-laden fluid on an unsuspecting public.

The question, however, is constantly present in one's mind as to whether more pollution does not enter the infant's milk in the home of the infant himself than at the dairy and in the course of transit to the consumer's dwelling. Newsholme believes that the home is the most common site of infection; and there are many reasons in support of his contention. No one who has been accustomed to visit the homes of the poor in the crowded slums of great cities will need to be reminded of the innumerable chances of pollution of the food supply of the infant, once he has been permitted to receive nourishment otherwise than from his mother's breast. Imperfectly cleansed bottles with long rubber tubes, in which particles of sour milk curd adhere and decompose, setting up rapid changes in every fresh meal that is introduced into them; dirty methods of storage; the access to food of flies and other insects with feet swarming with micro-organisms, after visiting heaven knows what foul garbage; dust from streets and yards, carrying particles of manure, &c.—these are only some of the dangers to which the infant life of the poor neighborhoods of our cities is exposed. To minimise these dangers there are, it seems to me, two lines of procedure open. One is entirely new to us in Australia, but evidences of its value in the saving of the infant life are rapidly accumulating in France, in England, and in the United States: I refer to the establishment of infant milk depots, and model dairies established and conducted by private philanthropy, or, preferably, under municipal control; such, for instance, as that of Battersea in London, or Rochester in the State of New York. In these depots sterilised milk, produced at a model dairy, is modified, according to the requirements of the age of the child for whom it is intended; is put up in bottles of such a size that each contains one meal; and is sold to poor mothers in baskets, each of which contains enough meals for one child for twenty-four hours, at a price which barely pays expenses. At Battersea the charge for supplying an infant in such a manner is 1s. 6d. a week. The children supplied from the depot are registered, and their homes visited from time to time by a woman inspector, who notes the progress of each infant, and reports at the depot. She also instructs the mother in the feeding of the child. Such statistics as are available in connection with these depots, as well as those in France and other continental countries, are of the most gratifying character, and show an immense saving in infant life from their operations. Indirectly also they are of great value in educating the public in the best methods of feeding young children, and the model dairies connected with the depot are valuable as object lessons to the ordinary dairy farmer, whose ways and methods in the production of milk are never ideal, and often disgusting. To all who are interested in this subject, a perusal of Dr. McCleary's valuable little book on infantile mortality can be most heartily recommended. The other precedure to which I allude is purely educational in its method, and has been in operation in the city of Sydney for rather more than a year. A daily list of all births registered in the city is obtained from the registrars, and, within a few days after registration, the home of each child born in a poor neighborhood is visited by a woman inspector of the municipal council, who interviews the mother, talks to her confidentially on the management of the child, advises her as to the methods she ought to follow, and earnestly inculcates cleanliness, leaving,

also, at each house a copy of a brief and plainly worded leaflet setting forth the dangers of infantile diarrhœa, and giving instructions on the feeding of infants. The principal points impressed on the mother by the inspector are the great superiority of breast-nursing over every other form of feeding: should breast-feeding be impossible, the use of properly modified fresh cow's milk is advocated, and warnings against the long tube bottle and the use of starchy or patent foods are tendered. The conditions as to cleanliness and general sanitary state of the dwelling are noted by the inspector, and reported by her at headquarters, where any action which may appear to be indicated by the information in the report is taken by the Medical Officer of Health.

It is evident that the value of such a proceeding as this must largely depend upon the personal tact of the visiting inspector, and the municipality of Sydney has been fortunate in obtaining the services of a woman who is in all respects exceedingly well fitted for the work. It is worthy of note that in only two or three instances has there been the slightest objection, or resentment, on the part of mothers to these domiciliary visits.

Since this action was inaugurated in May, 1904, over 1,400 homes of newly born children have been visited in this manner. The experiment is yet too recent for me to be able to offer you any useful statistics on its effects; and, moreover, such statistics would be obscured, and their value diminished, by the unusually low infantile mortality which appears to have prevailed throughout eastern Australia during the summer of 1904-5, and which has evidently largely depended on causes beyond human control.

There are many other aspects of this most important and fascinating subject to which I might refer, but I feel that I have already taken up too much of your time and attention. I trust that our experiences in Sydney may be of some slight value to sanitarians in other States, and particularly to the members of this Section.

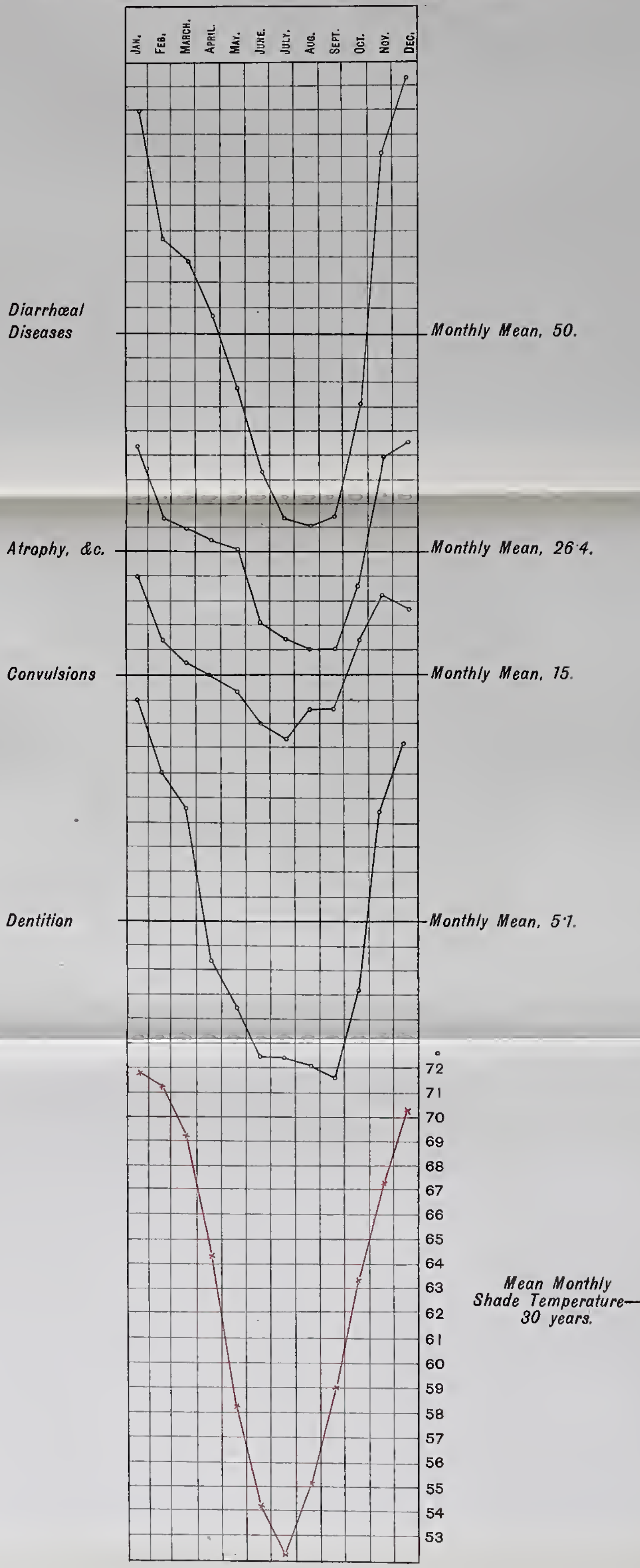
TABLE I.

Deaths of Infants under One Year per 1,000 Births Registered.—Mean of Ten Years' Observations—England and Wales, 1892–1901; New South Wales and Sydney, 1895–1904.

	England and Wales.	New South Wales.	Sydney and Suburbs.
Measles	3.0	.4	.6
Whooping cough	5.7	3.3	3.1
Pneumonia and bronchitis	25.2	10.3	11.2
Diarrhœal diseases	29.1	30.7	40.9
Convulsions	17.8	8.2	6.5
Dentition	2.4	1.1	.7
Suffocation	2.0	.6	.8
Atrophy, debility, and inanition	20.8	15.6	17.6
Premature birth	19.1	14.3	16.0
Congenital defects	3.9	3.9	4.6
Tubercular diseases	7.5	3.5	3.9
Syphilis	1.4	1.0	2.0
All other causes	15.9	14.5	15.
All causes	154	107	123

SYDNEY AND SUBURBS.

Monthly Deaths from Diarrhœa and certain other Diseases at all ages.
Excess or Defect per cent. compared with the Mean Average of 30 years—1875-1904.



SURVEYOR GENERALS OFFICE ADELAIDE A. Vaughan, Photolithographer

Diagram showing the Rainfall, Mean Temperature in the Shade, Mean Temperature Registered by 5-foot Earth Thermometer, and number of Deaths from Diarrhœal Diseases in Sydney and Suburbs for each week from July 1st, 1902, to June 30th, 1905.

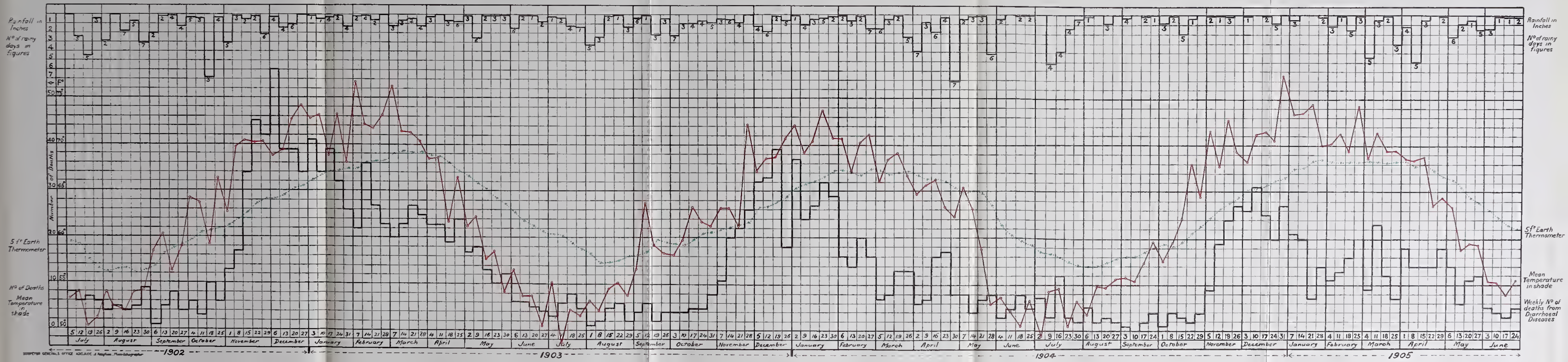


TABLE II.

SYDNEY AND SUBURBS.

Deaths of Infants Under One Year per 1,000 *Births for the Years* 1875-1904, *inclusive.*

1	2	3	4	5	6	7	8	9	10	11	12
Year.	All Zymotic Diseases (except Diarrhœa).	Pneumonia and Bronchitis.	Diarrhœal Diseases.	Convulsions.	Dentition.	Atrophy, Debility, and Inanition.	Prematurity.	All Congenital Defects.	Tubercular Diseases.	All Causes.	Combined Infantile Mortality from Causes in Columns 4, 5, 6, and 7.
1875....	22.0	14.1	24.7	18.6	8.4	43.6	15.0	2.6	10.9	176	95.3
1876....	19.0	12.2	26.1	20.1	5.2	40.7	13.0	3.1	13.2	169	92.1
1877....	15.9	10.8	32.5	16.6	7.4	28.4	11.9	3.6	10.5	156	84.9
1878....	25.8	13.1	29.6	19.8	5.0	32.5	11.9	3.6	9.5	173	86.9
1879....	9.5	11.5	36.3	20.6	5.1	30.8	10.4	1.8	9.8	153	92.8
1880....	18.4	21.1	32.2	24.2	5.6	43.2	13.6	3.2	7.5	192	105.2
1881....	12.1	14.8	32.1	22.8	4.3	35.5	8.9	2.5	9.2	162	94.7
1882....	12.7	17.1	42.0	20.6	5.9	36.9	12.1	3.6	9.3	183	105.4
1883....	10.3	13.8	31.9	19.0	4.6	38.8	12.9	2.7	9.2	163	94.3
1884....	12.3	15.7	36.1	21.3	3.4	40.5	10.3	4.1	9.7	172	101.3
1885....	10.9	14.5	39.9	22.3	6.8	42.2	13.3	2.5	11.1	186	111.2
1886....	11.3	14.5	37.3	17.4	5.9	37.2	12.4	4.6	11.3	173	97.8
1887....	6.3	10.5	34.6	14.2	6.3	22.1	12.6	3.4	9.5	140	77.2
1888....	6.9	12.3	39.0	14.5	5.2	30.9	14.3	2.9	9.3	152	89.6
1889....	14.8	16.2	44.4	14.0	6.6	25.5	13.6	4.3	8.7	172	90.5
1890....	6.2	14.6	31.2	13.3	3.9	20.0	15.9	3.1	6.2	135	68.4
1891....	8.7	17.7	34.5	13.6	4.0	23.9	14.8	3.8	5.1	148	76.0
1892....	7.8	11.0	30.5	10.4	5.0	23.7	14.4	2.5	5.5	130	69.6
1893....	12.6	11.7	37.9	13.5	1.6	25.7	13.9	4.5	5.8	147	78.7
1894....	10.0	10.6	35.4	10.9	.2	23.7	14.7	4.5	5.4	134	70.2
1895....	7.0	10.1	39.2	9.0	.7	21.7	15.1	3.8	5.4	130	70.6
1896....	3.7	9.7	45.4	9.5	.4	24.5	14.4	4.2	5.4	139	79.8
1897....	3.5	12.8	43.4	8.9	.4	21.2	17.1	3.7	3.9	129	73.9
1898....	14.7	17.9	46.1	9.5	.3	22.8	15.9	5.0	5.5	154	78.7
1899....	9.2	10.7	33.6	6.1	.2	16.8	17.1	4.6	4.7	120	56.7
1900....	2.8	8.3	36.4	7.6	.3	16.0	15.4	4.2	3.6	109	60.3
1901....	10.3	11.8	37.4	5.5	1.4	17.1	15.5	4.2	2.9	120	61.4
1902....	5.8	10.7	39.0	2.7	1.5	12.5	16.4	4.6	3.8	112	55.7
1903....	6.7	12.5	44.2	3.9	.8	12.5	14.5	5.4	2.3	116	61.4
1904....	5.1	8.4	31.0	3.2	.7	11.4	18.8	6.5	2.0	98	46.3

SEWAGE TREATMENT.

A REVIEW, BY E. S. STOKES, M.D., &c.

The necessity, in the interests of public health, for the appropriate treatment—as apart from the mere disposal—of sewage appears to have met with adequate recognition only within comparatively recent years.

The phenomenal overgrowth of cities and towns, especially during the latter half of the last century, and the concomitant advance of public opinion in hygiene, have been the principal factors in bringing into prominent notice the exigencies of the situation.

Matters first came to a head, so to speak, in England, after the passage of the Public Health Act of 1848, by virtue of which a large number of sewers were installed. There does not, however, seem to have been at this time any very serious or systematic endeavor, on the part of local authorities

concerned, to purify the sewage before its discharge—which was, in most cases, into rivers. In consequence the rivers became seriously polluted, and a general outcry arose. In 1857 the first Sewage Commission was appointed, and, after sitting assiduously for over eight years, they declared that "the right way to dispose of town sewage is to apply it continuously to land; and it is only by such application that the pollution of rivers can be avoided." Obviously this opinion did not meet the requirements of the day, because we find a second Commission appointed two years later, in 1868. This Commission endured only for six years, *i.e.*, until 1874, when they presented their fifth and last report. The opinion of this Commission on the comparative methods of the classes of processes for the treatment of sewage, viz., chemical precipitation, intermittent filtration, and broad irrigation, may be stated thus :—(1) All these processes are, to a great extent, successful in removing polluting organic matters in suspension, but intermittent filtration is the best; broad irrigation ranks next, and chemical precipitation processes are less efficient. (2) But for removing organic matters in solution, the processes of downward intermittent filtration and broad irrigation are greatly superior to upward filtration and chemical processes.

The Commission defined "broad irrigation" as "the distribution of sewage over a large surface of ordinary agricultural land, having in view a maximum growth of vegetation (consistent with due purification) for the amount of sewage supplied"; and "intermittent filtration" as "the concentration of sewage at short intervals, in an area of specially chosen porous ground, as small as will absorb and cleanse it; not excluding vegetation, but making the produce of secondary importance. The intermittency of application is a *sine qua non* even in suitably constituted soils, wherever complete success is aimed at."

Nothing further was done in the way of appointing Sewage Commissions for the next eight years, but during this time the condition of the Thames became so bad from the quantity of sewage flowing into it that a third Commission was created in 1882, to investigate and report upon this overwhelming nuisance. After two years they reported that evils did exist, imperatively demanding a prompt remedy, and that by chemical precipitation a certain part of the organic matter of sewage could be removed. They also reported, however, "that the liquid so separated would not be sufficiently free from noxious matter to allow of its being discharged at the present outfalls as a permanent measure. It would require further purification; and this, according to our present state of knowledge, can only be done effectually by its application to land."

Guided by these reports, since 1884 the Local Government Board of England have required that any scheme of sewage disposal for which money is to be borrowed with their sanction should provide for the application of the sewage, or effluent, to an adequate area of suitable land before its discharge into a stream. This requirement has been a fruitful source of dispute between local authorities and the Central Board, especially in cases where the former satisfied themselves, by extended experiment, that they were able to produce an effluent better in many cases than the water, in the stream into which it was to be discharged, and, naturally, they considered it a gross injustice that they should be compelled to outlay large sums of money in the purchase of land which they did not consider they required, and which, if purchased, they could not use. Furthermore, the recent evolution of biological methods of sewage purification, and the undoubted degree of success attained by several of these processes, added great weight to the contention of local authorities that land treatment as demanded by the Local Government Board was not absolutely necessary.

Mainly to settle this point, we find the fourth Sewage Commission appointed in 1898. To date they have issued four reports, with numerous appendices, containing valuable information; but so far their conclusions have been very few and very guarded. In an interim report issued in 1902, they state in reply to the question, "Are some sorts of land unsuitable for the purification of sewage?" "We doubt if any land is entirely useless; but, in the case of stiff clay and peat lands, the power to purify sewage seems to depend on the depth of the top soil. There are of course numerous gradations in the depths of top soils which are met with in nature, and it is not easy to draw the line between lands which contain a sufficient depth to justify their use and lands which do not. We are, however, forced to conclude that peat and stiff clay lands are generally unsuitable for the purification of sewage; that their use for this purpose is always attended with difficulty; and that, where the depth of soil is very small, say 6 inches or less, the area of such lands which would be required for efficient purification would in certain cases be so great as to render land treatment impracticable."

In the same report they answer the question—"Is it practicable uniformly to produce, by artificial processes alone, an effluent which shall not putrify and so create a nuisance in the stream in which it is discharged?" thus—"We are satisfied that it is practicable to produce, by artificial processes alone, either from sewage or from certain mixtures of sewage and trades refuse—such, for example, as are met with at Leeds and Manchester—effluents which will not putrify, which would be classed as good according to ordinary chemical standards, and which might be discharged into a stream without fear of creating a nuisance. We think, therefore, that there are cases in which the Local Government Board would be justified in modifying, under proper safeguards, the present rule as regards the application of sewage to land. No general rule as to what these safeguards should be can be laid down at present; and, indeed, it will probably always be necessary that each case should be considered on its own merits."

It is of interest to note, then, that in England, during the past forty-eight years, *i.e.*, since 1857, Commissions on Sewage have been sitting for fully twenty-three years, and that the last Commission is still at work endeavoring to solve the sewage problem. Also, during this time several Rivers Pollution Commissions have existed, which dealt largely with the pollution of rivers with sewage. So that, practically, during the past half century there have been very few years in which a Commission of one sort or the other was not engaged in making inquiries into some aspect of the subject.

The results attained do not seem altogether commensurate with the time and energy expended; but perhaps these seemingly barren results are due to the complexity of the problem. The first Commission went bald-headed for land treatment; the second was very strong on this point, but recognised chemical processes; the third appeared more kindly disposed towards the latter, but still demanded final land treatment before the discharge of effluents into streams; whilst the present Commission has gone so far as to express the cautious opinion that land treatment is not always necessary, and may, indeed, under suitable safeguards, be at times omitted.

But whilst Commissions have calmly pursued their way, and given birth to ponderous tomes and appendices, their efforts have not enabled them to give voice to an *ex cathedra* pronouncement as to which of the methods in use or advanced can be relied upon to effectually purify sewage under any conditions. Certainly the earlier Commissions were somewhat positive in their expressions, but, as subsequent development has shown that their conclusions were based upon what we now know to be insufficient evidence, the

present Commission hesitates—no doubt, wisely—to say much on this point until they are able to speak in the light of more extended experience. I think it is rather too much to expect at any time a dogmatic answer to this question, but the least we might hope for would be a clear enunciation of the principles involved in the purification of sewage, together with a generalised account of their application under the variety of circumstances met with in practice.

Beyond all the work done by and on behalf of the above Commission, we have records of prolonged and expensive experimental investigation undertaken by interested corporations, both in Europe and America, and exhaustive reports on the subject by engineers, chemists, and biologists. Out of this class of information it is possible to glean, here and there, some facts which seem to indicate the direction in which we must turn our eyes to discover the real solution of the problem.

I propose shortly to review the several methods of treatment which are now in use, or which have been suggested or used at various times; and, in doing so, shall endeavor to trace as far as possible, in proper sequence, the principal developments that have occurred during the past fifty years.

In his evidence before the present Commission, Mr. Adrian, Assistant Secretary to the Local Government Board, subdivided the years from 1842 into three periods, viz.:—(1) 1842-1857, when the primary aim was to secure prompt removal of sewage from the neighborhood of dwellings; (2) 1858-1870, which was characterised by the abandonment of the notion of possible injury to health as a consequence of irrigation of land by sewage, and by the growth of the belief in the ideal system of sewage disposal as being that which should aim at its profitable utilisation by direct application to land, and should thus facilitate the protection of rivers from contamination; (3) the period from 1870 to the present time, with the dominant idea that the prevention of the pollution of streams is an indispensable requisite of every system of sewage disposal which can lay claim to efficiency.

In view of the already mentioned requirement of the Local Government Board as to the whole or partial land treatment of sewage before discharge into a stream, it is only natural that this form of treatment should have been a prominent feature in the past; and there is no doubt that, under suitable conditions and efficient management, it, either as intermittent downward filtration or broad irrigation, has produced most satisfactory results; and it is also certain that under contrary conditions it has caused grave nuisances.

The most recent statement on this subject will be found in the report of Drs. McGowan and Houston and Mr. Kershaw, on land treatment, to the present Commission. Their conclusions, generally, are favorable to land treatment. In summarising their opinion with regard to suitability of various soils, they say, with regard to filtration, that excellent results can be obtained from a light loamy soil overlying a porous subsoil; that a sandy soil and subsoil, and also a partially peaty soil overlying gravelly sand, are capable of yielding good results; that peat, pure and simple, is not well adapted for sewage purification, and that, with regard to chalk, they were unable to speak with confidence, owing to insufficient data. They further state that, with regard to broad irrigation, "heavy loam and clay soil, although not so well suited for sewage purification purposes, may yield fairly good effluents if the volume of sewage per acre is relatively small."

Thus almost any kind of soil can be used for the purpose of sewage purification, provided of course that the volume is proportionate to the purifying capacity of the soil in question. We are far from advocating the treatment of sewage on land which is, practically speaking, not well suited for the purpose.

but this does not invalidate the truth of the proposition that the matter is nearly always one of degree of suitability, and seldom one of intrinsic disability.

Stress is laid upon the necessity for efficient screening and settling before applying sewage to land, unless it is delivered in a thoroughly disintegrated condition—such as may at times be seen in cases where the outfall works are situated some considerable distance from the gathering ground, and where, consequently, the sewage becomes altered during its long passage, or where the sewage is broken up by pumping.

The advantage of cropping is pointed out, and the best crops are stated to be those which can be more or less continuously sewaged without detriment, *e.g.*, quickly growing plants like rye grass, mangold wurzel, &c.

Some of their conclusions on other points may be quoted :—

" The effluents from land processes of sewage treatment are not, from the bacteriological point of view, in a proper condition for discharge into drinking water streams."

" The effluents from land possess a bacterial flora characteristic of sewage, and the microbes characteristic of soil (in the sense of being peculiarly abundant in soil) are relatively absent from land effluents.

" As a result of its treatment on land, and judged by the bacteriological tests employed in this investigation, sewage does not seemingly become modified in its biological characters to any material extent. The bacteria, however, were reduced in number to a marked extent.

" The few samples of subsoil water collected in the neighborhood of some of the sewage farms were usually found to be pure, both chemically and bacteriologically ; but this, of course, must not be regarded as proving that wells sunk in such situations are safe for domestic use, or free from serious objections.

[A footnote in reference to this says :—" Two years' work by one of us in the chemical and biological qualities of the Chichester well waters showed that shallow wells, sunk in polluted soil and subsoil, may show, on searching bacteriological examination, unequivocal evidence of excremental, and therefore potentially dangerous, pollution."

" Generally speaking the discharge of effluents into streams did not exercise any marked prejudicial effect in the water of the stream ; indeed, the reverse was sometimes observed. There seems to be no reason to doubt that the effluents from properly managed sewage farms would, when discharged into non-polluted streams of relatively large volume, neither give rise to nuisance, nor, as far as may be judged by rate of absorption of oxygen, prove injurious to fish.

" There seems to be no reason to doubt that, with proper management, land can purify sewage for a practically indefinite period.

" As regards the likelihood of sewage farms being dangerous to health, we can do no more than tentatively express the opinion that no convincing proof has yet been furnished of direct or widespread injury to health in the case of well-managed farms. It may be possible that the foul emanations from a badly-managed or over-sewaged farm constitute an indirect source of danger to health by lowering the vitality of weakly and susceptible individuals."

To my mind, these quotations, in the main, fairly sum up the situation as regards land treatment at the present day ; but they can only be considered as confirmatory of previous views, and not as expository of new discoveries of any essential character. Whilst, therefore, we cannot accept that land treatment under certain conditions is an efficient method of purifying sewage, we must not ignore the fact that these certain conditions—nature and area of land available are purely local matters, and that many towns are so situated that they cannot obtain a sufficient area of suitable land. The recognition of these difficulties, and the general struggle for improvement in all directions, aided by

increased knowledge—chemical and biological—so noticeable since the middle of the last century, have co-operated in the evolution of other methods of sewage treatment, the earliest of which may be classified under the heading of "Chemical processes."

Chemical processes for the treatment of sewage have been instituted with several objects in view beyond mere purification. The earliest chemical process actually used (1846) was the lime process, and this was employed apparently only for clarifying. Very many modifications of the lime process have since been introduced—such as combination of lime and tar, lime and tar and calcined chloride of magnesia, lime and carbolic acid, lime and clay, lime and prepared alkali waste, and lime and herring brine. The addition of some of these chemicals was with the idea of effecting disinfection. The clay was used with lime in connection with the manufacture of Portland cement. In addition, lime has been used to neutralise acidity of sewage, due to the entrance of trades wastes.

Alumina processes have also been extensively used. Salts of aluminium, either alone or in connection with lime, zinc salts, carbonate of soda, iron salts, permanganates, clay, &c., have been tried or recommended at different times. The well-known A. B. C. process (alum, blood, and clay) appears to have been one of the most successful of these, although at present time the blood is omitted.

Certain salts of iron form useful precipitants. Both ferrous and ferric salts have been employed—most generally the ferrous sulphate. The perchloride has been tried in combination with a similar salt of magnesia. Holden's process consists of the use of sulphate of iron, clay, lignite, charcoal, coal dust, and lime. It is stated that sewage treated with this shows considerable amounts of nitrates and nitrites, which is, of course, a sign that a certain degree of oxidation of nitrogenous matter in the sewage had taken place—a rather unusual event to occur after the action of a chemical precipitant. Ferrozone is a proprietary substance made up of a combination of salts of iron and alumina, and is recommended as an efficient precipitating agent.

At first exaggerated ideas prevailed as to the manurial value of the sludge obtained by the above chemical process. But experience and analysis both go to disprove this. The sludge contains very little nitrogen and potash, and perhaps a slightly greater amount of phosphorus. With the object of trying to arrest some of the more valuable constituents of sewage, a number of processes have been tried. The originators of the magnesia process hoped to precipitate ammonia and phosphates as triple phosphates, but failed to carry it out. Several phosphate processes have been experimented with. The process consists in the employment of soluble or superphosphate of magnesia, which was added to and mixed with sewage, and then precipitating it with lime or other precipitating agent; the idea being that, when superphosphate of magnesia was added to the sewage, it neutralised the ammonia, forming an insoluble triple phosphate of magnesia and ammonia, which is precipitated out of the liquid. For a variety of reasons all these processes failed.

Other chemicals have been used for the purpose of deodorising or sterilising sewage. Dibdin considers that it is quite possible to deodorise large volumes of sewage with manganates or permanganates and sulphuric acid. Manganese oxides are deposited in the sludge, and there possibly effect further oxidising effects. Chlorine and chlorine compounds act as conveyers of oxygen, and in that capacity have been applied to sewage, most commonly directly in the form of chloride of lime or bleaching powder, but also indirectly as hypochlorous acid formed by the action of an electric current passed through either the sewage itself or a saline solution—such as salt water. There is no doubt that chlorine in this form exerts a powerful destructive

effect in the lower forms of life, but it is by no means satisfactorily shown that this electrolytic process is efficient in the purification of sewage. This process may be adopted for the sterilisation of an otherwise purified effluent which is discharged into a stream used for drinking purposes.

Although largely used and experimented with, chemical processes have failed to secure of themselves any very appreciable measure of success as agencies for the complete purification of sewage. The effluent produced may be clear, owing to the removal of suspended matter, but the organic matter in solution is left unaffected, and may even be added to in some cases by the solvent effect of the precipitant; consequently these effluents undergo secondary decomposition, and require further treatment before being allowed to flow into streams. This treatment usually consists of passage through filter-beds, or of methods mentioned under the heading of land treatment.

With the aid, then, of other processes, chemicals may be said to have attained the object aimed at, but at the cost of a serious disability—I refer to the production of sludge from the action of the precipitant in the sewage. This sludge consists of the matters in suspension plus the precipitants, and, of course, is always being produced and constantly calling for removal. At first it was believed that it would possess considerable manurial value, but this, as already stated, was shown to be incorrect; in fact, in this capacity it would not pay for cartage away. The perennial sludge difficulty with chemical processes undoubtedly stimulated experiment in other directions, the results of which now appear before us in the various methods usually classed under the term "bacterial" or "biological purification."

The explanation that formerly held with regard to the purification of sewage by land treatment, or the purification of effluents from chemical processes by filters, was that, apart of course from the mechanical straining, the organic matter was directly oxidised by the oxygen of the air by a simple chemical combination. The most important oxidation products being nitrates, it is only natural that attention should have been specially centred in these compounds. It is stated that, in 1862, Pasteur regarded it as probable that nitrification was the direct result of bacterial activity. In 1872 the Berlin Sewage Commission reported that sewage matters were converted into nitrates, not by a simple molceular process, but by organisms present in natural sewage and soil. In 1876, at the Paris Sewage Farm, Schloessing and Muntz demonstrated this by sterilising the soil with vapor of chloroform. These experiments were subsequently amply confirmed by other observers, but none of them were successful in isolating an organism able to build up nitrate from ammonia. In 1886 Heraeus claimed to have isolated pure cultures capable of transforming ammonia into nitrous acid. Other investigators, notably Frankland and Warrington, closely examined Heraeus' work, and came to the conclusion that the nitrous acid formed was due to the reduction of nitrate in the medium, or, at times, to absorption of the same from the air. The search after the nitrifying organisms, however, though at first negative, was not fruitless. Almost about the same time Jordan in America, the Franklands and Warrington in England, and Winogradsky in Germany, appear to have arrived at the conclusion that these organisms would not grow on the ordinary media, and they therefore tried weak ammoniacal solutions in combination with small quantities of salts of the alkalis, *i.e.*, a medium containing nothing but inorganic matter. Frankland and Winogradsky succeeded in isolating an organism that would build up nitrites, and Jordan one that completely nitrified (but this latter was probably a mixture). Since that time (1890) a vast amount of experimental investigation has been put in hand, so that we are now in possession of the main facts concerning both the building up and breaking down of nitric acid by bacteria. The first complete investigation into the treatment of sewage

by filtration through specially prepared artificial beds was undertaken by the Massachusetts State Board of Health, from 1887 to 1900. They clearly established the fact that sewage could be completely purified by intermittent filtration through appropriate filters, and that the purification was due to bacterial agencies. All these inquiries, however, had main reference to oxidation, but, as it has been shown the nitrifying organism will not act upon nitrogen in organic combination, it must be apparent that there exists a preparatory stage in which organic nitrogen is converted into ammonia. The fact that solid excrementitious matter undergoes apparently spontaneous disappearance by liquefaction in cesspools had been noted undoubtedly before this, but no great importance appears to have been attached to such phenomenon until 1891, when Scott-Montcrieff began his experiments on a practical scale at Ashtead. His upward filtration tank is so well known that I need not attempt to describe it; it is sufficient for me to say here that his demonstration of the disappearance of sludge by natural agencies in the tank attracted general attention, and opened up a new era for sewage treatment.

In 1895, Cameron, of Exeter, introduced his "septic tank," and added further proof that the solid matter of sewage undergoes spontaneous liquefaction. Both Cameron's and Montcrieff's processes appeared to obviate the sludge difficulty of the precipitation processes, and the sewage problem seemed solved. But whilst it cannot be now claimed that these processes achieved everything that enthusiasts believed them capable of at first, still it must be admitted that Cameron's "septic tanks" especially have had a tremendous influence in shaping subsequent events in sewage treatment. Chemical processes have been dropped, and no one now thinks of aught but bacteriological installations. A host of inventors have arisen with many and wonderful valves, syphons, distributors, *et hoc genus omne.* We have few discussions on the relative values of the various systems as such, but on all sides we have advocates of closed septic tanks and open septic tanks, contact beds and continuous filtration, Montcrieff's tanks, Dibden's beds, ærating filters (such as Ducat's, Lowcock's, and Waring's, &c.), each being able to show that his system will effect a purification of over 90 per cent.

To attempt to describe fully the complex changes in dead organic matter that go on quietly in nature's laboratory would take us to realms beyond the scope of this paper; but a brief account may be acceptable. The final products of the process are fully oxidised compounds; nitrogen becomes converted into nitric acid, carbon into carbonic acid, and hydrogen to water. Organic matter, however, seldom oxidises directly to these ultimate compounds, except, perhaps, under such energetic influences as fire or electric discharges, but undergoes a preliminary disintegrating transformation to simpler compounds capable of easy oxidation by bacterial agency. In the first stage—which is found to proceed most rapidly under anærobic conditions—we see proteid matter converted into peptones, and peptones again split up into amido-compounds; amido acids are further decomposed into fatty acids and ammonia, and basic amines into hydroxy-compounds with evolution of of free nitrogen. Urea is decomposed into carbonate of ammonium generally before the sewage is received for treatment. The organic acids are reduced with the production of acid carbonates, free carbonic acid, hydrogen, and methane. Acetic acid is generally the penultimate product, but this eventually passes into the compounds and gases just mentioned. In formation of $C\ O_2$ here, the oxygen is derived from the original substance, and the process cannot be considered a parallel to what we shall see occurs in the later stage. Cellulose and fibrous matter may be hydrolysed with the formation of carbonic acid and marsh gas, and probably, at times, of fatty acids. Carbohydrates and fats, similarly, are decomposed into fatty acids and gases.

The bacteria at work in this stage are anærobes—obligatory and facultative—and the absence of free oxygen probably causes the latter to break up a good deal more organic matter in their search for oxygen than they would do if it were readily available from the air. We observe here the gradual formation and splitting off of ammonia from organic nitrogen, with the concomitant elaboration of nitrogen free compounds. We also note the evolution of gases, hydrogen, nitrogen, and marsh gas, which escape into the air, and ammonia and carbonic acid, which remain in solution. We further see that no oxidation products are formed, with the exception of carbonic acid, as already explained.

The next stage, which does not seem to be generally recognised as distinct from the others, is one of partial oxidation. Here we find the free ammonia acted upon by organisms which produce nitrous acid, and, in addition, important chemical reactions have been recorded between the nitrous acid so formed and transitional products carried over in solution from the first stage—such as ammonia, amido-acids, and amines—resulting in the evolution of considerable volumes of free nitrogen. Nitrosification is said to proceed most rapidly in the presence of diffuse light and a moderate supply of oxygen.

In the final stage nitrites are converted into nitrates, and the carbonaceous residues oxidised as completely as possible.

The conditions for nitrification by bacteria are as follows :—1. The antecedent formation of first ammonia and then nitrous acid from the organic nitrogen ; (2) the presence of some fixed base ; (3) darkness, and free admission of air ; (4) absence of excess of alkalinity. Warington found that nitrification could not proceed if the alkalinity as ammonia exceeded 44.6 parts per 1,000,000.

Carbonic acid gas is also formed in abundance in this stage, and, unless provision is made for its rapid and continuous removal, nitrification will be delayed—if not altogether inhibited.

An interesting feature in connection with nitrification is the rapidity with which it takes place. Scott-Montcrieff found that in passing a tank effluent over his differential trays, in eight to ten minutes nine parts of nitric-nitrogen per 100,000 were formed—a very high figure. Probably in contact beds most of the nitrate production occurs as the beds are slowly emptying ; and whilst standing full the changes described as belonging to the second stage are happening.

A consideration of the above account will throw some light on the failure of such systems as Ducat's, Waring's, and Lowcock's, which sought to treat raw sewage in filters by direct oxidation. We see how nature does not provide for the direct oxidation of nitrogen, and therefore the systems are based upon incorrect principles. Any measure of success they may have attained can be accounted for by the fact that the sewage they were treating had undergone, before reaching the works, the changes pertaining to the first stage. The reason that nature does not permit of the direct combination of nitrogen with oxygen is obvious when we think of the composition of the atmosphere. If we could oxidise the nitrogen in the proteid molecule, by causing any extra blast of air to be directed on to it, we should soon see the whole world converted into an alkali desert, and we ourselves would be as Lot's wife.

Although bacterial purification of sewage has been sprung upon us of late as if it were a new invention—like wireless telegraphy—we must not fail to recognise that it is no new process, but that the identical changes so fully elucidated by numerous recent investigations have been going on in the world as long as living matter existed thereon. In short, it is an integral part of the economy of nature ; and, without the intervention of agencies to effect the changes necessary for the transformation of effete animal matter into substances suitable for absorption by the vegetable kingdom, life, as we know it, would soon disappear from the face of the earth. What we really have been

doing is, so to speak, to harness the forces of nature, so that we can use and regulate them to work—not in any way contrary to their disposition, but under conditions most favorable for efficiency.

Glancing back over the last fifty years we can now recognise the effect of these unseen agencies in the evolution of sewage treatment. First we have land treatment, which has been generally successful, only failing when applied under inappropriate conditions: this is bacterial treatment pure and simple. Next we see the introduction of chemical processes, all of which ended in failure and necessitated subsequent treatment of the effluents, either on the land or on specially prepared filter beds. Once again bacteria are called in to the rescue. And, finally, we come to the present time, when, with a full appreciation both of the changes involved in the disintegration of organic matter and the building up therefrom of innocuous and important inorganic compound, and of the agencies whereby this metamorphosis is effected, we find ourselves surrounded by a multitude of systems of bacterial treatment—as numerous almost as the roads to heaven—each of which has its army of adherents ready to pledge themselves in all sincerity that theirs is the only system worth a second thought.

Out of all the apparent confusion can we gather any hope for the future? I think we can. It may be risky to attempt to prophesy, but in this instance I believe I shall be on safe ground in expressing the opinion that in the application of bacterial treatment we shall find the true solution of the sewage problem. It will not be solved by somebody's patent filter, or valve, or such-like, but the correct answer will be arrived at only by prolonged and careful study of each individual sewage we propose to treat, and the application thereto of a line of treatment—the details of which must be determined by actual experiment—based upon the broad principles already enunciated.

Finally, I am well aware that I have said nothing new nor original, and that I have not referred to many points of interest in connection with the subject; my object being to submit this review in the hope that it may be of interest, and possibly of some use, to the members of this Section. A wise physician makes full use of the *vis medicatrix naturæ.* We should never fail to remember that there is also a *vis purificans naturæ.*

TYPHOID FEVER IN NEW SOUTH WALES, 1898-1904.

By R. J. Millard, M.B., Ch.M., Sydney, D.P.H., Cambridge.

In 1898 notification of certain infectious diseases, including Typhoid Fever, became compulsory in New South Wales under the Public Health Act of the State. The notifications, sent in the first instance to the local authorities, are by them forwarded to the Board of Health of the State and recorded in the Notification Bureau of the Department of Public Health. Statistics for seven years have thus become available, and the object of the present paper is to review these statistics concerning Typhoid Fever, and the results of some investigations into outbreaks of the disease during this period.

For permission to use these official data I am indebted to the Chief Medical Officer of the Government and President of the Board of Health (Dr. J. Ashburton Thompson), under whose instructions I made the investigations into country towns typhoid referred to later on.

The importance of the subject is very great. Every year some hundreds of persons die of this disease in New South Wales, and some thousands are

incapacitated from wage-earning or enjoyment of life by an illness which has an average duration of many weeks. Moreover, the most susceptible persons are adolescents and young adults, and the disease not infrequently permanently impairs the constitution and development. A similar state of affairs exists throughout the civilised world, and for many years the attention of hygienists and of sanitary authorities everywhere has been largely occupied with measures to elucidate and reduce typhoid incidence and mortality.

I have tabulated the figures under two main heads, Metropolitan and Country. It is necessary to separate metropolis from country because the conditions of life are largely dissimilar. The metropolis possesses an extremely good water supply, is extensively served by a very excellent system of sewers, has the advantage of energetic sanitary administration, offers facilities for hospital isolation, and last—but not least—being on the sea coast, has very different climatic conditions from the inland regions which form the greater part of the country districts.

For details concerning Typhoid Fever in the metropolis I am largely indebted to the excellent annual reports of the Medical Officer of Health for the Metropolitan Combined Districts (Dr. W. G. Armstrong).

Fatality.—The fatality has been :—

Metropolis....................	5,790 cases	568 deaths	— 9.8 per cent.
Country Districts.............	16,217 "	1,809 "	— 11.2 "
Whole of New South Wales	22,007 "	2,377 "	— 10.8 "

These percentages are very low. In England the fatality per cent. has generally been about sixteen.

Age and Sex Distribution has not differed, generally speaking, from that observed in other countries. The incidence has been heaviest on adolescents and young adults. The sexes have been affected fairly equally.

Incidence Rate.—Reckoned on the population figures obtained at the Census of 1901, the incidence of typhoid in the seven years has been as follows :—

	Population.	Typhoid Cases.	Annual Rate per 1,000.
Metropolis	487,900	5,790	1.69
Country Municipalities	369,476	11,682	4.52
Unincorporated portions of the State	497,470	4,535	1.30
All New South Wales	1,344,846	22,007	2.31

For comparison with these figures, it may be mentioned that the incidence rate of London, for the ten years 1893-1902, was .8. The incidence rate of the Sydney metropolis is thus more than twice, and of the combined country municipalities more than five times, the London rate.

The combined unincorporated districts have a rate lower than that of the metropolis, and less than one-third that of the country municipalities. This comparatively low rate is probably due mainly to the fact that these districts are all rural, and include few centres of population sufficiently large to suffer from their defective sanitary arrangements : most of the population is scattered on farms, stations, mining settlements, and small townships. Another contributory cause of the low rate is probably that many cases of typhoid in these country districts are never seen by a medical man, and consequently are never notified.

In the combined country municipalities the rate is 4.52. The marked excess of this over the metropolitan rate is mainly due to the climatic differences referred to more fully later on ; but is also largely attributable to the fact

that these country municipalities possess many of the defects, and lack most of the advantages, usually attaching to urban conditions. Defective sanitation, lack of sewers, faulty methods of disposal of excreta and house refuse, especially in towns where considerable numbers of people are aggregated—these are, no doubt, important factors in producing the higher incidence on the country municipalities. Another point which deserves mention is the relative densities of population per acre and per house. In the country municipalities, containing, as most of them do, large areas of which only the central part is populated, the density of population per acre for the municipality is very much less than in the metropolis; but the number of persons per dwelling was found at the Census of 1901 to be the same, namely, 5.4, both for Sydney and suburbs and for the total country municipalities, while the sizes of the dwellings were smaller in the country municipalities. Of the total dwellings in either division, 37 per cent. in the country and only 32 per cent. in Sydney and suburbs had four rooms, or fewer, so that the country people were definitely more crowded in their dwellings. In connection with infantile diarrhœa, Newsholme considers density of population per house to be of more importance than density per acre, and it is at least interesting to find this theory supported by our New South Wales statistics concerning Typhoid Fever. Increase of numbers per house means diminution of power to isolate infectious cases, and this must necessarily influence the spread of the disease.

In the Metropolis the incidence rate has been heaviest on those municipalities in which the sanitary conditions were worst. Botany has had a bad pre-eminence in this respect throughout the period under review. From time to time different localities have been the subject of special mention in the annual reports of the Medical Officer of Health, on account of epidemic or increased endemic incidence. No outbreaks due to water-borne infection occurred. This was to be expected, as the metropolitan water supply is exceedingly good. Small localised outbreaks due to milk infection occurred in 1898, at Burwood, twenty-three cases, and in 1904, at Surrey Hills, again twenty-three cases. In 1900, in the Ultimo portion of the city, seventy-two cases occurred in a small area of which the Medical Officer of Health reported that "the only satisfactory thing, from a hygienic point of view, was the water supply. The dwellings were ill-constructed . . . . and ruinous . . . . yards badly drained, unpaved, and often filthy . . . . old-fashioned sewers . . . . house connections very defective . . . . a few of the houses served with filthy cesspits . . . . much of the area liable to flooding in heavy rain storms."

The municipality of Balmain (population 30,256) in 1901 had 183 cases, and in 1903 had 150 cases, the mean number for the other five years of the period 1898-1903 being 66.2. The increased incidence was in each instance carefully investigated, and it was found that the ordinary epidemic causes could be excluded. In 1901 the Medical Officer of Health suggested that the extensive disturbance of the upper layers of the soil, consequent on the active construction of sewers in Balmain, might have played a part in disseminating the disease. In 1903 it was concluded that "the diffusion of the disease was essentially due to the many grossly insanitary and unwholesome premises, and to the inadequate methods of disinfection adopted."

In the Country there have been marked differences in the incidence rate in different districts. The most striking general difference has been between the comparatively low rates in the coastal and the comparatively high rates in the inland districts. This is illustrated by the maps which I have had prepared, and, in more detail, by Table A, in which I have set out (1) the population at last Census, (2) aggregate typhoid cases for seven years, (3)

incidence rate per 1,000 of population per annum, and (4) mean annual rainfall, for all municipalities throughout the State. The district divisions, viz. :—A, Coastal ; B, Tableland ; C, Western Slopes and Riverina ; and D, Western Division, are those adopted in the Statistical Register of New South Wales for 1903 ; and the rainfall figures are those published by the Government Astronomer. I have inserted the figures for the metropolis at the head of the table, but have not included these in calculating the aggregate coastal incidence rate, for the reason stated above, that metropolitan sanitary conditions are so much superior to those of country towns. A measure of this superiority is in fact afforded by these figures. The metropolitan typhoid incidence rate for seven years is 1.69 per 1,000, while the other coastal towns, generally similar to the metropolis in climate and rainfall, and containing in the aggregate a population of 183,122, have for the same period a mean rate of 2.34 per 1,000. For the population figures I have drawn on the Census returns of March 31st, 1901. This date is very nearly midway between January 1st, 1898, and December 31st, 1904, the period for which the notifications are here tabulated.

It will be seen that the aggregate incidence rates and mean rainfalls for these four Divisions form a regular gradation, the incidence rate increasing with the diminution of the rainfall as follows :—

A. Coastal Division (excluding Metropolis)	Rainfall	40in.	Incidence	rate	2.34
B. Tablelands	"	29in.	"	"	4.74
C. Western Slopes and Riverina	"	21in.	"	"	7.46
D. Western Division	"	13in.	"	"	10.03

Corresponding variations have been recorded for India by Major A. R. Aldridge, in a paper on " Enteric Fever and Sewage Disposal in Tropical Countries " (*Journal of Hygiene*, July, 1902). He says—" Of these twenty-four stations, the thirteen having the highest admission rates are geographically very distinctly separated from those having low rates, all being situated in or closely bordering on the dry dusty alluvial plain of Upper India . . . On the other hand, of the eleven stations having a relatively low rate, four are situated on or near the coast with a damp climate throughout the year, while the remainder are situated on rocky soils of volcanic origin, and are relatively free from dust."

The divisions of the State which I have tabulated differ in other respects than in rainfall. Thus the Coastal Division is, generally speaking, free from the extreme heat which characterises the interior. As regards dust, too, this depends not only on the rainfall but also on the winds. In the inland divisions dry west winds blow in the summer time and dust storms are common. The difference in rainfall may be supposed to exert a twofold influence —on the soil and so on the saprophytic existence of the typhoid bacillus, and on its dissemination in dust and in the water supply. Dwellers in the Coastal Division have less difficulty in getting a satisfactory source of supply than in the waterless west, where the choice is limited. And this suggests another difference, namely, in mode of living and diet. It is probable that, on the whole, the dwellers in the Coastal Divisions have a higher standard of living than the others ; though, as only municipalities are under consideration, the difference is not, I think, very great.

As regards general sanitation, the divisions are in many respects similar. Sewerage is practically non-existent in all. The individual municipalities vary in the energy with which they avail themselves of their statutory powers for preservation of the public health ; but, generally speaking, more attention is paid to these matters inland than in the coastal division—partly because the coastal towns lack the annual stimulus of a heavy typhoid list, and

partly because they are generally older towns and slower to avail themselves of the comparatively recent legislation on the subject. Thus cesspits and inefficient scavenging are at least as common in the coastal as in the other divisions.

The cleanliness of any town depends largely on the contour of its site. Other things being equal, a hilly well-drained town will be cleaner and more healthy than a town on the plains. In this respect most of the coastal towns are superior to the western towns, which are, many of them, on very flat country. But the tableland towns generally are quite as well situated as the coastal; yet the tableland incidence rate is twice as heavy as the coastal.

The relative abundance of flies in the coastal and other divisions during the typhoid season is a matter on which I have no certain data, but it appears to be a general belief that flies are very much more numerous inland than on the coast.

It will have been noticed that while the divisional incidence rates form a regular gradation from coastal westwards, yet there are in each division municipalities which have rates differing widely from the mean. This is only to be expected in dealing with numerous comparatively small centres, where an inconsiderable outbreak in any one year will suffice to greatly exalt the incidence rate for the seven years period. These individual centres necessarily vary greatly in their local sanitary conditions, and in most cases a cause for the exaltation of the rate is to be found in some gross neglect of sanitation, such as continuance of the primitive cesspit, formerly so common in all our towns.

Such departures from the mean do not, however, invalidate the general observation that, in the four great divisions of the State, Typhoid Fever and rainfall have been inversely proportional. They may be taken as indicating that, though rainfall and the conditions accompanying its abundance or scarcity are apparently the factors having most influence on the incidence of the disease, yet that local circumstances may override this influence: which may be used to point the moral that even in the favorably situated coastal towns it is not safe to neglect sanitary principles and practice.

Annual Variation.—For the seven years the mean number of notifications per annum has been—New South Wales, 3,144; metropolis, 827; country, 2,317. Notifications above the mean were received in 1898 for country and metropolis, in 1900 for both metropolis and country, 1901 for metropolis only, 1903 for both metropolis and country. The highest figures for the two divisions respectively were, in 1900, metropolis, 1,045; and in 1903 country, 4,022. The year 1903 was remarkable for a very large amount of typhoid throughout the country districts, the figures for this year totalling more than the sum of the figures for 1902 and 1904. The metropolis shared in this rise, but only to a moderate degree. This year was marked, meteorologically, by the break-up of a long period of drought. In 1902 the rainfall throughout the State had been abnormally low, everywhere below the average, and in many places more than 50 per cent. below. The drought continued into the early part of 1903, but about the latter half of April there were general heavy rains. It is noteworthy that the greatest typhoid incidence was toward the end and after the break-up of the drought. And this necessarily suggests that cases may have been in part due to contamination of water supplies by the washing of long accumulation of surface dirt into wells and underground tanks. In the several outbreaks which were specially investigated by me there was, however, no evidence to support such a theory, wells and underground tanks being comparatively uncommon, and it is more probable that this was merely the autumnal incidence somewhat prolonged in consequence of the large amount of infectious material throughout the country, and of the mild weather which accompanied the rains.

Seasonal Variation.—The notifications received during each fortnight of each year are tabulated in Table B, and in Tables C to I are shown curves obtained with these figures. These tables do not accurately indicate the actual seasonal incidence, as they are based on the dates of receipt of notifications at the Notification Bureau. The actual date of infection of any case is probably, as a rule, three or four weeks earlier. The tables for successive years are, however, comparable with one another, and show a striking similarity. Thus, in all, the curve begins to rise about the end of October and continues to rise, with perhaps a few breaks, until April, after which a descent begins, which is not complete until July. The figures then remain low until the October rise recommences.

No exact interdependence of these curves with meteorological conditions can be demonstrated, and in some respects they seem to be mutually independent. Thus the curves are similar for droughty and other years alike. In this connection I have collated and examined the rainfall and typhoid incidence for each month of the seven years in the metropolis and in certain representative country towns—Broken Hill, Tamworth, Bathurst, Wagga, and Albury—but without being able to satisfy myself of any direct connection between the two. The most obvious connection appears to be with the temperature, the summer rise and winter fall of temperature being the only meteorological conditions which recur with absolutely regular periodicity each year. The averages of the mean shade temperatures obtained at the Sydney Observatory during the past forty-four years are as follows:—

January...	71°.5	maximum	July........	52°.3	minimum
February..	71°.0	"	August	54°.8	"
March	69°.3	"	September ..	58°·9	"
April	64°.5	"	October.....	63°.5	"
May	58°.4	"	November...	66°.9	"
June......	54°.4	"	December ...	69°.9	"

This may be taken as indicating, approximately, the seasons in other parts of the State also; but the extremes of heat and cold are much more marked inland, where it is common to get summer maximum over 100° and winter minimum below 32°. The typhoid rise begins in the early summer, and coincides with the general rise of temperatures. The ascent continues, however, for some time after the shade-temperature has reached its maximum, and the decline does not begin until autumn. An important point, which should not be lost sight of, is that in the winter months, though cases continue to occur, anything in the nature of an epidemic is extremely rare. Why should this be so? Infectious material is being produced, and food and water supplies are exposed to risks of contamination, yet the infection is not disseminated. Obviously some necessary conditions are lacking in winter which are supplied in summer.

In the multiplication of cases of typhoid two factors are of main importance: (1) the persistence and propagation of the bacillus in its saprophytic existence; (2) the distribution of the virulent bacillus to susceptible individuals. As regards (1), repeated observations have shown that the typhoid bacillus is extremely resistant to cold. Curschmann ("Typhoid Fever," p. 35) states "The bacilli persist at temperatures as low as 10°C., and even repeated freezing and thawing of fluid containing the bacilli do not appear to destroy them (Chantemesse and Widal, Janowsky) . . . . . Prudden observed that typhoid bacilli kept in ice at a temperature between 1°C. and 11°C. retained their vitality for three months." But although it retains its vitality, the bacillus is probably in a state of hibernation at these low temperatures. In the laboratory it is found that the optimum temperature for culture is 37°C., and the limits 9°C. and 42°C. Thus it may be supposed that the lowered temperature of winter, while not killing the bacillus, tends to inhibit its growth. How is the infection handed on from one summer to another?

The notification figures show that throughout the country the occurrence of new cases ceases almost entirely in the winter, and examination of records of individual isolated country towns usually shows a complete cessation for many months. Yet in these same towns when the appropriate season comes round typhoid breaks out again, and frequently shows a special affinity for certain areas or premises which have in previous seasons been connected with other cases of the disease. It is impossible to avoid the conclusion that the case-free interval is bridged over by the saprophytic persistence of the bacillus. It is true that the researches of Horton Smith and others have shown that the bacillus may persist in the patient's urine for months and even years after convalescence, but the seasonal curves recurring year after year with absolute regularity show that such dissemination of bacilli is insufficient by itself to cause any considerable spread of the disease. For, just when there are the greatest number of convalescents at large, namely at the end of the autumn, the case incidence falls abruptly. The saprophytic theory is obscured by the conflicting results obtained in various experiments on the vitality of the typhoid bacillus in soil. Thus, on the one hand, Dr. Sidney Martin, in his report on the Antagonism of the Soil to the Typhoid Bacillus (L.G.B. Report, 1900-01), concludes as a general result of his investigations " that the typhoid bacillus has commonly only a short existence in the soil; that it is destroyed by the products of the putrefactive bacteria which exists in most cultivated soils." On the other hand, Professor Firth and Major W. H. Horrocks, in their " Inquiry into the Influence of Soil, Fabrics, and Flies in the dissemination of Enteric Infection " (*B.M. Journal*, September 7th, 1902), came to the following conclusions (*inter alia*) :—

1. That there is no evidence to show that the enteric bacillus, when placed in soil, displays any disposition or ability to either increase in numbers or grow upwards, downwards, or laterally.
3. That the enteric bacillus is able to assume a vegetative existence in ordinary and sewage-polluted soil, and survive therein for varying periods, amounting in some cases to as much as *seventy-four days.*
4. That the presence or absence of organic nutritive material in the soil appears to be a largely negligible factor . . . .
5. That an excess or great deficiency of moisture in soils appears to be the dominant factor affecting the chances of survival of the enteric bacillus in, or at least the possibility of recovering it from, the soil.
6. From fine sand allowed to become dry the bacillus can be recovered on the twenty-fifth day after inoculation.
7. From fine sand kept moist with either rain or dilute sewage bacillus cannot be recovered later than *twelfth day*, probably because it has been washed into the deeper sand layers.
9. From ordinary soil kept damp by occasional addition of rain water the bacillus can be recovered up to and on the *sixty-seventh day.*
10. From same soil kept damp with dilute raw sewage, recovery up to *fifty-third day.*
11. From same soil, kept damp with dilute sterile sewage, recovery up to *seventy-fourth day.*
12. That in similar soil, after heavy rainfall, the bacillus at once disappears from the surface layers.
13. That from same soil allowed after inoculation to become so dry as to be readily blown about as dust, the bacillus can be recovered up to the *twenty-fifth day.*
20. That the bacillus is able to survive in surface soil an exposure to 122 hours direct sunshine extending over twenty-one consecutive days. (Experiments conducted at Netley, in the months of June and July, 1902.)

And in a later communication (*B.M.J.*, October 4th, 1902), Professor Firth draws attention to a paper by Professor E. Pfuhl, of Berlin, who found that after inoculation of various soils he could recover the enteric bacillus for various periods, as follows :—Moist garden earth, eighty-eight days ; dry sand, twenty-eight days ; and moist peat, twenty-one days.

As regards (2) —distribution—it is probable that flies and other flying insects play an important part in the distribution of infectious material. It is, I believe, a fact that flies are almost extinct in winter, and that they gradually increase through the summer until the first nip of returning cold weather kills them off in thousands. Thus the increase and decrease of flies are respectively coincident with increase and decrease of typhoid. How far they are casually related is probably not susceptible of demonstration ; but observations are on record which show that the same individual flies may frequent latrines and mess tents, and that it is possible for them to carry infectious material on their feet.

In an article on Typhoid Fever in South Africa (*B.M.J.*, March 8th, 1902) A. B. Dunne, M.B., B.C., Cantab. (late Civil Surgeon, South African Field Force), speaks of the "plague of flies" at Bloemfontein during the epidemic of enteric in 1900, and of the noticeable diminution in cases admitted to hospital coincident with the killing off of the flies by the cold nights of May and June.

So far I have dealt with typhoid mainly as occurring endemically in New South Wales. It may not be out of place to add something concerning some epidemic and exaggerated endemic manifestations which it has been my duty to investigate.

The year 1903 was remarkable for an unusually heavy incidence of typhoid throughout the State generally, and for epidemic outbreaks at several country towns.

1. Of these, Coonamble was the most striking example. Here in a town of 1,680 inhabitants there occurred 256 cases of typhoid fever in the eight weeks January 30th to March 26th. The disease manifested itself in typically epidemic form. For the first four weeks of the year only two cases were notified, and then in the next four successive weeks there occurred sixty-one, forty-six, seventy-one, and forty-six cases respectively. After this, as a direct result apparently of the measures adopted, the numbers dropped abruptly, and in the next four weeks were eleven, nine, five, and seven. The water supply of the town was above suspicion, being derived from an artesian bore 1,320 feet deep, and supplied to the town mains under continuous pressure from an overhead tank 70 feet above ground level. Samples taken from different points in the town were examined in the biological and chemical laboratories of the Department of Public Health, and found free from any indication of pollution. As regards household milk supply, the majority of patients had either used condensed milk or none at all. The cause of the epidemic outbreak appeared to be the consumption of summer drinks, and of a new and popular concoction known as "milk-shake," at a particular shop in the town. In this shop there were employed (1) a youth who, while mixing drinks, &c., was "out of sorts," and who a fortnight later was found to have advanced typhoid fever, and (2) a man, who at the same time was the sole attendant on his wife then ill with typhoid. Of the sixty-one cases occurring in the first fortnight (January 30th to February 12th) forty-one had consumed "milk-shakes" from this shop, and sixteen were secondary cases in already infected households. In the succeeding weeks of the epidemic, multiple cases in households became a prominent feature. Altogether sixty-one households had more than one case, and of these households four had six cases, five had five cases, eight had four cases, seventeen had three cases, and twenty-eight had two cases. These cases occurred, generally speaking, at such intervals after the primary case as to

leave it scarcely open to doubt that they had been infected directly or indirectly from the primary case, and not from the one common cause. Opportunities of such infection were many. Isolation at home was impracticable, and the members of the household generally shared the duties of nursing, with its manifold risks. Besides this, the method of disposal of infectious excreta was calculated to promote infection. Instead of using the special closet pails provided by the council, most householders attempted to "burn" the stools and urine. The *modus operandi* was to make a fire, and pour the stools, &c., over it. Perhaps the fire was made in a hole. In any case the result was merely that the fire was extinguished and the liquid material soaked into the ground practically unaffected. In this way the back yards became infected. Agencies for carrying infective material into the houses and into food were supplied by flies, and by the small whirlwinds of dust prevalent at that time of the year. The measures adopted to check the epidemic comprised rapid extension of the local hospital from twenty to 150 beds, removal thither of every case as it occurred, and disinfection of premises, especially of back yards. With these measures the outbreak ceased rapidly, as above stated; and it is interesting to note that only nine cases occurred in Coonamble in the following year, 1904.

In the same year, 1903, examples of increased incidence almost amounting to epidemics were afforded by Tamworth, Bathurst, Cobar, and other country towns.

2. Tamworth, population 5,799, had 126 cases in the period January 14th to May 20th. The heaviest incidence was in the fortnights ending April 8th and 22nd, and May 5th, when twenty-four, twenty-nine, and thirty-one cases respectively were reported. No epidemic cause could be ascertained. Water supply was not obtained from a common source by any considerable number of cases. Almost every household used rainwater caught on the roof and stored in iron tanks. Milk was very scarce in consequence of the drought, and was not used at all by the majority of the patients. Employment or school attendance could not be incriminated. Association with previously infected premises was a most striking feature of the outbreak. A map of the town on which were "spotted" residences of all cases which had occurred in successive years, demonstrated this association very clearly. With few exceptions, the cases reported in 1903 occurred on premises or in blocks which had been invaded at least once, and in some instances every year, since 1898. This was especially noticeable in West Tamworth, where the majority of the cases occurred. Here again, as at Coonamble, the disposal of the infectious excreta was almost universally faulty, the practice having been to bury it in the back yards. This practice had prevailed for many years, and it was easy to believe the soil had been in this way repeatedly infected. But it was less easy to explain why the outbreak should have been so much more extensive in 1903 than in previous years, unless the explanation was to be found in the climatic peculiarities of this season. The summer had been unusually dry. In January and February respectively only .47 and .38 inches of rain fell at Tamworth. This was considerably less than for the same period in any of the five preceding years. In March 2 inches, and in April 2.5 inches fell, and the rapid increase of typhoid cases coincided with this rainfall. It should, however, be noted that at the same period there was a markedly exaggerated incidence of typhoid fever in other country towns where the same relationship to rainfall could not be demonstrated. The only condition in common was that the country had suffered from a long severe drought, but how this stimulated the typhoid bacillus to such vigorous vitality was not clear.

3. Bathurst, population 9,327, had 135 cases in the twenty-four weeks ending June 17th. The cases were fairly evenly distributed over this period.

the heaviest incidence being in the middle eight weeks ending April 22nd, in which seventy-three cases were reported. For the previous five years the notifications had been—1898, 35; 1899, 14; 1900, 115; 1901, 36; and 1902, 22 cases. In 1903 the gradual increase of cases made it improbable that there was any epidemic cause such as infection of water supply, and investigation of individual cases confirmed supposition. The town water supply was as a matter of fact a good one, derived from a shaft and a tunnel in the drift of the Macquarie River, well above the town, whence the water is pumped to a high-level reservoir and distributed under continuous pressure through the mains. Of 1,691 premises supplied by the town water mains only 105 were invaded by the disease, and there was no indication of any special incidence along the course of any particular main or mains. The spread of the disease appeared due to defective sanitary conditions combined with imperfect isolation of patients and disinfection of excreta. Bathurst is an unsewered town, and in consequence the disposal of household slops is generally unsatisfactory. At best the slops are but led to the nearest street gutter, and as a rule the house drains are not watertight. The disposal of household garbage and refuse was in 1903 equally unsatisfactory. Systematic regular removal was not enforced, and consequently on most premises there was an ashpit or ash heap, where the accumulated garbage of months was thrown, and which was probably never thoroughly cleared away. Moreover, on this heap bedroom slops were commonly cast, and in some instances even typhoid stools were buried there. Instructive examples of the spread of the disease under these circumstances were observed, of which two may be quoted here:—

(1) In a small terrace of six houses in Piper Street, four cases occurred, three houses being invaded. The first case, Veronica G., aged 12 years, was notified on December 24th; three weeks later, on January 13th, her sister Ruby, aged 16 years, was notified; four weeks later Linden H., living three doors away, and three days after him Marie J., aged 45 years, living next door to the first case. A defective open gutter ran down immediately at the back of the terrace receiving the slop waters from all the houses, and close by was an ancient "ash heap" common to all the houses.

(2) In another household four cases occurred, as follows:—

D.R., female, 17 years—Notified February 14th, removed to hospital February 12th, discharged March 18th.

A.R., male, 12 years—Notified March 26th, removed to hospital March 24th, discharged March 16th.

L.R., female, 6 years—Notified April 20th.

M.R., female, 15 years—Notified April 30th.

On these premises the usual drainage and garbage defects existed.

4. Cobar, population 3,371, had 118 cases in the twenty weeks ending May 20th, 1903, and of these, ninety-seven cases occurred in the latter half of this period. For the previous five years the cases notified had been successively 12, 23, 68, 38, and 14. As regards causation, it was not possible at Cobar to exculpate the water so satisfactorily as at the other towns already mentioned. The town supply was indubitably not of good quality, and it was being newly laid on at the time when the cases were becoming numerous. But many of the cases occurred on premises not yet connected with the mains, where rainwater only was used for drinking purposes, and throughout the town there were instances of recurrence of the disease on premises infected in previous years, and of secondary cases in an already infected household. These circumstances brought this outbreak very closely into line with the outbreaks at the other towns. Here also there were no sewers, and

the yards were fouled with slop waters and house refuse. The "spot" map showed that in certain blocks typhoid had recurred year after year, suggesting very strongly that the source of infection was persistent and localised on these premises.

As examples of the handing on of infection from case to case there may be instanced three hotels, of which one had seven and the others each six cases :—

1. Hotel X.—Seven cases—
   - i. H. G., 40 years, lorry-driver, lodged at hotel. Onset about 25–12–03. To hospital, 2–1–03 ; discharged, 6–2–03.
   - ii. J. E. McL., 23 years, miner, lodger. Onset, 10–3–03 ; hospital admitted, 17–3–03 ; discharged, 2–4–03.
   - iii. J. K., 45 years, blacksmith, lodger. Onset, 3–4–03 ; hospital admitted, 10–4–03 ; discharged, 12–5–03.
   - iv. J. O'D., 34 years, tailor, boarder only. Lodged at his shop. Onset, 6–4–03 ; hospital admitted, 8–4–03 ; discharged, 7–5–03.
   - v. J. 40 years, miner, arrived from Canbellego ill on 23–4–03. Hospital on 26–4–03. N.B.—This patient evidently did not receive his infection at this hotel, but he contributed to infect the premises, and is included on that account.
   - vi. W. R., 28 years, engine-fitter at mine, lodger. Onset, 3–5–03 ; hospital, 10–5–03.
   - vii. D. McP., 33 years, miner, lodger. Onset about 13–5–03. Under treatment at hotel, 20–5–03.
2. Hotel Y.—Six cases—
   - i. M. H., 11 years, schoolgirl. Onset about 10–2–03 ; hospital admitted, 13–2–03 ; discharged, 22–2–03 ; re-admitted, 3–3–03 ; discharged, 25–3–03.
   - ii. N. M., 26 years, servant, came from Dubbo, and after staying for about a week at the hotel, fell ill about 10–3–03 ; hospital admitted, 17–3–03 ; discharged, 7–4–03.
   - iii. A. H., 33 years, cook, mother of H. M. (No. 1). Onset, 14–3–03 ; hospital admitted, 21–3–03 ; discharged, 2–4–03.
   - iv. C. R., 22 years, miner, lodger. Onset about 1–4–03 ; hospital admitted, 5–4–03.
   - v. J. K., 30 years, miner, lodger. Onset about 15–4–03 ; hospital admitted, 22–4–03.
   - vi. W. H., 38 years, miner, lodger. Onset, 25–4–03 ; hospital admitted, 2–5–03.
3. Hotel Z.—Six cases—
   - i. W. B. G., 22 years, assistant C. P. S., boarded only, lodged elsewhere. Onset about 4–3–03 ; hospital admitted, 8–3–03 ; discharged, 4–4–03.
   - ii. B. D., 22 years, female, cook's assistant. Onset about 5–3–03 ; hospital admitted, 9–3–03 ; discharged, 22–3–03.
   - iii. P. F., 31 years, part proprietor of hotel. Onset about 15–3–03 ; hospital admitted, 22–3–03 ; discharged, 18–4–03.
   - iv. A. R., 25 years, barmaid. Onset about 15–3–03. Nursed at hotel in room adjoining bar.
   - v. C. W., 37 years, female cook, shared room with B. D., and helped nurse her. Onset, 22–3–03 ; hospital admitted, 29–3–03 ; discharged, 28–4–03.
   - vi. C. F., 34 years, part proprietor. Onset about 23–4–03. Being nursed in hotel.

In considering the data and experience relating to Typhoid in New South Wales in the seven years to 1904, four points appear to me to especially merit emphasis :

1. That typhoid has very regularly recurring seasonal variations, which correspond closely to those observed in other parts of the world. Moreover, there is a close resemblance in this respect between typhoid and infantile diarrhœa, which also is believed to be due to a micro-organism having naturally a partially saprophytic existence.

2. That for its dissemination the typhoid bacillus is very dependent in some way on external conditions, as exemplified in the difference between the incidence rates of the coastal and inland districts. How these conditions act must be largely a matter of speculation, but in the present example the rainfall would appear to be the most important constituent of the climatic differences.

3. That infection of water supplies has not, in New South Wales, been an important factor in spreading typhoid fever. It is necessary to insist upon this, because the medical and lay mind has become strongly imbued with the theory that in water, and in water alone, is to be found the *causa causans*. As the Frenchman in difficulties of another kind promptly suggests, *cherchez la femme*, so the average would-be sanitarian, confronted with a typhoid outbreak, at once turns to the water supply, and is satisfied if he can ingeniously prove that it has been exposed to the danger of contamination by sewage. I do not for a moment suggest that the water supply has not been the vehicle of infection in many epidemics. The circumstantial evidence is very strong, and is vouched for by witnesses of the highest standing. But other means of spread have, as a matter of fact, proved to be more common, and therefore of more importance, in New South Wales ; and it is to be feared that the too earnest attempt to sheet home the crime to the water supply often diverts attention from simpler and more obvious causes, as, for instance, the fouling of the back yard by infectious excreta.

4. That infection from a previous case, either directly in the sick-room, or indirectly from excreta imperfectly disinfected or improperly disposed of, has been a factor of great importance. It is not only in New South Wales that the casual importance of association with infected persons has been noted. Recently the trend of skilled opinion has been strongly in this direction, and many observers reporting on outbreaks have emphasized the influence of this, rather than the more conventional epidemic cause—water-borne infection. As an example may be quoted the very complete official report on typhoid in in the volunteer camps of the United States Army during the war with Spain in 1898 (summarised in the *B.M. Journal* of July 26th, 1902) ; and additional evidence in abundance is furnished by the many articles and discussions reported in the medical journals, more especially in connection with Typhoid Fever in South Africa and India. A notable apostle of this doctrine is Professor Koch, who, in an address at the Kaiser Wilhelm's Academy, on November 27th, 1902, emphasized the importance of personal, as distinguished from water-borne, infection (*B.M.J.*, February 28th, 1903).

From this it results that the two essentials in dealing with any outbreak are (1) to isolate the sick, (2) to disinfect their excreta. Proper isolation and treatment can rarely be secured except in hospital, and therefore every case should, as early as possible, be removed thither. The necessity for this is not sufficiently appreciated. and too often the medical attendant consents to or even advises the home treatment of the patient, though it must be obvious that it will be impossible to secure effective disinfection of soiled clothing and of excreta. And this brings me to the all-important matter of the disposal of excreta. It is now well known that the bacilli may occur in both urine and stools, and consequently both these matters are to be disposed of. Where sewers are available, disposal is simplified ; but it is otherwise in the

unsewered country towns. Here various expedients are adopted. Burning is frequently attempted, on the alleged advice of the medical attendant, but is manifestly impracticable unless there is available sufficient absorbent material, such as sawdust, to soak up the liquid, and a furnace into which the mass may be cast. Otherwise the net result is a mild degree of desiccation, with, presumably, little injury to the vitality of the bacillus. Burying on the premises is wonderfully common. Throughout the country I have found this practice adopted; and again the medical attendant is generally credited with advising it. Even when the local council is willing and anxious to supply a special pail for the removal of typhoid excreta, I have found the householder diligently digging little potholes about his back yard, and sowing his soil with infectious material. Sometimes he attempts a preliminary "disinfection" with chemicals, but it may be taken as a general rule that the average householder cannot be trusted to use disinfectants effectually. The most effectual and simplest method of disinfection is *boiling*. This may be easily done in the back yard in a kerosine tin over an improvised hearth of a few bricks. If the infectious slops as produced are poured into the tin and kept covered, it will probably be sufficient to boil the contents night and morning. After boiling, the excreta, now sterilised as regards typhoid bacilli, may be emptied into a closet-pail for removal in the usual way. Little, if any, nuisance is caused by the boiling if care be taken not to allow the contents of the vessel to get burnt. At the temporary hospital at Coonamble, all excreta were thus disposed of in several large washing-coppers, which were placed at a convenient distance from the wards. The contents of bed-pans and urinals, and any soiled waters, were cast direct into the coppers, under which fires were kept burning. At suitable times the council's contractor removed the contents to the ordinary nightsoil depot. As there were at one time over 140 typhoid patients in the hospital, this method was fairly thoroughly tested. The result was eminently satisfactory, both as regards absence of nuisance and non-infection of the nursing staff.

TABLE A.

TYPHOID FEVER IN NEW SOUTH WALES, 1898–1904.

Typhoid Incidence and Rainfall in the Municipalities of the Several Divisions of the State.

Municipality.	Population, Census 1901.	Typhoid Cases Notified 1898–1904.	Rate per 1,000 per Annum.	Rainfall, Mean Annual, in Inches.
A.—COASTAL DIVISION.				
Metropolis	487,900	5,790	1.69	50
I. North Coast—				
Ballina	1,819	11	.86	75
Casino	1,926	111	8.23	44
Coraki	770	23	4.26	50
Grafton (North and South)	5.147	93	2.58	39
Kempsey	2,329	4	.24	50
Lismore	4.378	78	2.54	55
Maclean	1,333	7	.75	51
Murwillumbah (incorporated, 1902)	772	8	—	—
Port Macquarie	1.160	2	.24	63
Taree	871	1	.16	47
Ulmarra	1.722	4	.33	40
Wingham	556	2	.51	46
Aggregate North Coast (deducting Murwillumbah)	22,011	336	2.18	51

TABLE A—*continued.*

Municipality.	Population, Census 1901.	Typhoid Cases Notified 1898-1904.	Rate per 1,000 per Annum.	Rainfall, Mean Annual, in Inches.
A—COASTAL DIVISION—*continued.*				
II. Lower Hunter—				
Dungog	1,082	56	7.39	35
Gosford	751	27	5.13	50
Greta	861	56	9.29	34
Maitland (East and West)	10,073	388	5.50	34
Morpeth	1,288	25	2.77	41
Muswellbrook	1,710	65	5.43	24
Newcastle and Suburbs	54,991	1,142	2.96	48
Raymond Terrace	823	13	2.25	38
Singleton (North and South)	2,872	134	6.66	30
Aggregate Lower Hunter	74,451	1,906	3.66	37
III. County of Cumberland—				
Auburn	2,948	16	.77	31
Bankstown	1,246	6	.68	31
Cabramatta and Canley Vale	980	6	.87	31
Campbelltown	2,152	9	.59	29
Castlereagh	609	7	1.64	29
Dundas	1,087	6	.78	36
Ermington and Rydalmere	1,231	20	2.32	36
Granville	5,094	46	1.29	31
Ingleburn	362	—	—	30
Liverpool	3,901	34	1.24	30
Mulgoa	476	1	.30	29
Parramatta	12,560	143	1.63	36
Penrith	3,539	55	1.41	29
Prospect and Sherwood	3,259	13	.57	36
Richmond	1,202	7	.83	32
Rookwood	4,496	16	.50	31
Smithfield and Fairfield	1,643	7	.60	31
St. Mary's	1,840	5	.38	29
Windsor	2,039	46	3.22	28
Aggregate County of Cumberland (omitting Metropolis)	50,664	448	1.26	31
IV. South Coast—				
Bega	1,898	62	4.66	34
Berry	1,990	13	.94	64
Bowral	1,752	26	2.12	39
Braidwood	1,551	75	6.90	26
Broughton Vale	322	—	—	64
Camden	1,719	11	.91	29
Gerringong	1,051	4	.54	51
Illawarra, Central	4,664	16	.49	43
Illawarra, North	3,190	25	1.11	43
Jamberoo	1,291	—	—	53
Kiama	1,769	16	1.29	55
Mittagong	1,210	20	2.36	39
Moruya	1,099	4	.52	38
Moss Vale	1,385	6	.61	40
Nowra	1,904	9	.67	40
Picton	1,053	4	.54	30
Shellharbor	1,929	4	.29	53
Shoalhaven South	909	—	—	40
Ulladulla	1,765	4	.32	48
Wollongong	3,545	14	.56	43
Aggregate South Coast	35,996	313	1.24	44
Grand Aggregate Coastal Division (omitting Metropolis and Murwillumbah)	183,122	3,003	2.34	40

TABLE A—*continued.*

Municipality.	Population, Census 1901.	Typhoid Cases Notified 1898-1904.	Rate per 1,000 per Annum.	Rainfall, Mean Annual, in Inches.
B.—TABLELAND DIVISION.—				
I. Northern Tableland—				
Armidale	4,249	76	2.55	32
Bingara	879	44	7.15	30
Glen Innes	2,918	90	4.40	33
Hillgrove	2,274	37	3.25	36
Inverell	3,293	185	8.02	30
Manilla (incorporated 1901)	888	17	—	—
Quirindi	1,676	86	7.33	28
Tamworth	5,799	379	9.33	28
Tenterfield	2,604	5	.27	34
Uralla	681	4	.83	34
Walcha	980	16	2.33	30
Aggregate Northern Tableland (deducting Manilla)	25,353	922	5.19	31
II. Central Tableland—				
Aberdeen	749	43	8.20	20
Bathurst	9,223	393	6.03	24
Blayney	1,529	151	14.10	30
Carcoar	578	52	12.85	30
Cowra	1,811	71	5.60	26
Cudgegong	2,985	18	.86	26
Gulgong	1,579	23	2.03	27
Hill End	643	3	.66	28
Katoomba	2,270	22	1.38	60
Lithgow	5,268	127	3.44	35
Molong	1,254	58	6.60	30
Mudgee	2,789	97	4.96	26
Orange (West and East)	6,331	238	5.37	34
Wellington	2,984	144	6.89	23
Scone	1,145	53	6.61	24
Murrurundi	1,235	5	.57	32
Aggregate Central Tableland	42,373	1,498	5.05	30
III. Southern Tableland—				
Bombala	986	6	.86	24
Burrowa	839	14	2.38	19
Cooma	1,938	38	2.80	20
Goulburn	10,612	218	2.93	26
Grenfell	869	57	9.37	26
Queanbeyan	1,219	20	2.34	24
Tumut	1,391	142	14.58	32
Yass	2,220	45	2.89	24
Young	2,755	42	2.17	25
Aggregate Southern Tableland	22,829	582	3.64	23
Grand Aggregate Tableland Division (deducting Manilla)	90,555	3,002	4.74	29
C.—WESTERN SLOPES AND RIVERINA DIVISION.				
I. North-Western Slope—				
Coonamble	1,680	419	35.63	21
Gunnedah	1,910	76	5.68	25
Moree	2,298	83	5.16	23
Narrabri (West and East)	2,963	336	16.20	25
Warialda	875	5	.82	28
Aggregate North-Western Slope	9,726	919	13.49	24

TABLE A—*continued.*

Municipality.	Population, Census 1901.	Typhoid Cases Notified 1898-1904.	Rate per 1,000 per Annum.	Rainfall, Mean Annual, in Inches.
C—WESTERN SLOPES AND RIVERINA DIVISION—*contd.*				
II. Central Western Slope—				
Condobolin	1,078	84	11.13	18
Cudal	599	9	2.14	25
Dubbo	3,409	221	9.26	25
Forbes	4,294	358	11.91	19
Narromine	931	53	8.13	18
Parkes	3,181	174	7.81	20
Peak Hill	1,107	10	1.29	22
Warren	1,175	81	9.84	18
Aggregate Central Western Slope	15,774	990	8.96	21
III. South-Western Slope—				
Albury	5,823	111	2.72	28
Cootamundra	2,424	159	9.37	24
Corowa (incorporated 1903)	2,046	7	—	—
Gundagai	1,487	45	4.32	24
Junee	2,190	77	5.02	21
Murrumburrah	1,448	18	1.77	24
Temora	1,603	77	6.86	20
Wagga Wagga	5,108	260	7.27	22
Wallendbeen	736	9	1.74	22
Wyalong	1,575	63	8.31	14
Aggregate South-Western Slope (minus Corowa)	22,334	819	5.24	22
IV. Riverina—				
Deniliquin	2,644	59	3.19	16
Hay	3,012	212	10.05	14
Hillston	843	5	.84	14
Moama	928	5	.76	17
Narrandera	2,255	32	2.02	18
Jerilderie	744	1	.38	15
Aggregate Riverina	10,426	314	4.30	16
Grand Aggregate Western Slopes and Riverina Division (omitting Corowa)	58,260	3,042	7.46	21
D.—WESTERN DIVISION.				
I. East of the Darling—				
Balranald	741	41	7.90	12
Bourke	2,609	245	13.41	16
Brewarrina (incorporated 1901)	683	8	—	—
Cobar	3,371	367	15.55	15
Gladstone (incorporated 1899)	1,171	36	—	15
Nyngan	1,455	50	4.90	16
Wentworth	642	21	4.67	12
Aggregate East of the Darling (omitting Brewarrina and Gladstone)	8,818	724	11.84	14
II. West of the Darling—				
Broken Hill	27,500	1,872	9.72	9
Silverton	286	—	—	9
Wileannia	935	39	5.95	13
Aggregate West of the Darling	28,721	1,911	9.50	10
Grand Aggregate Western Division (omitting Brewarrina and Gladstone)	37,539	2,635	10.03	13

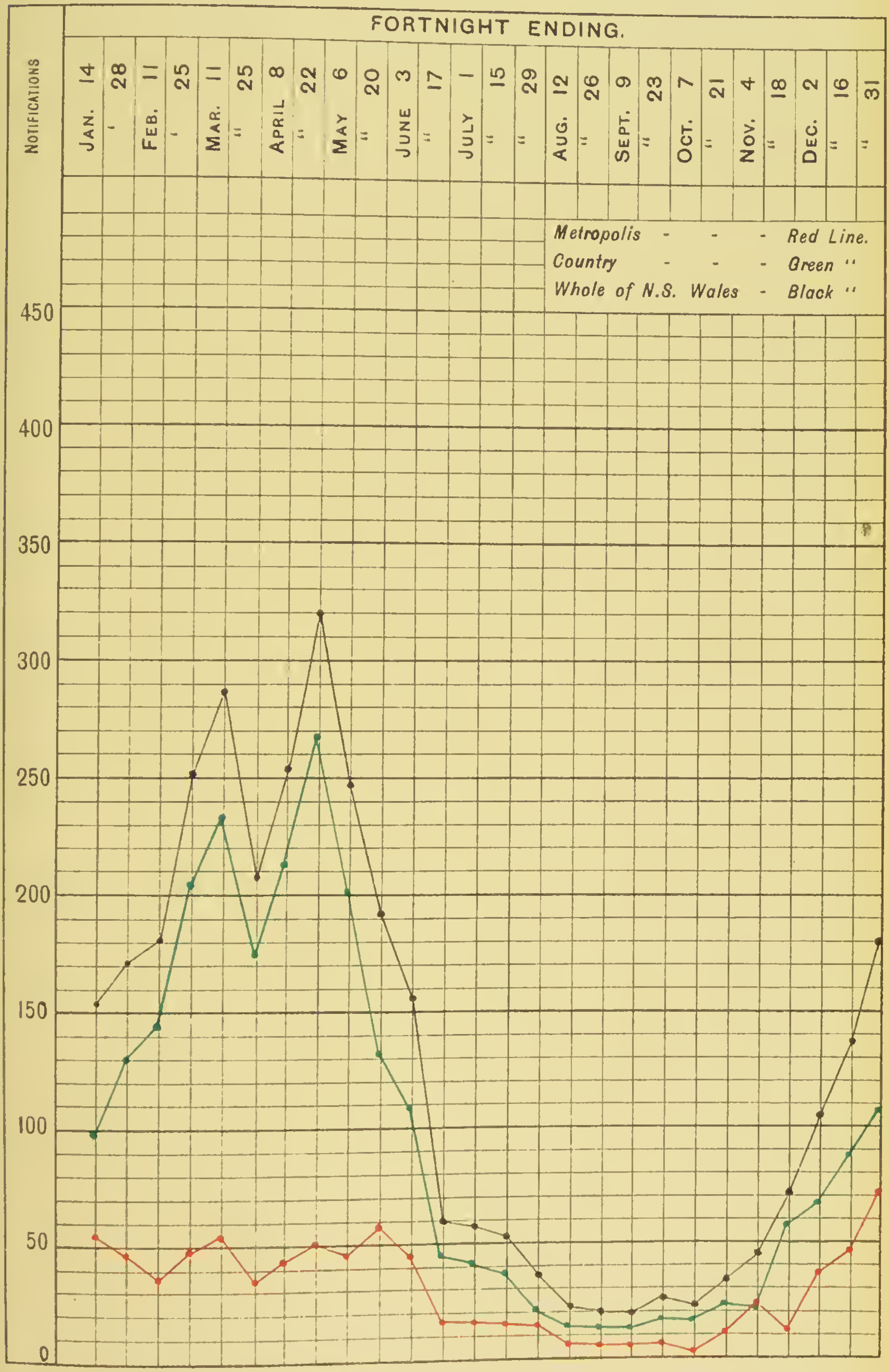
TABLE B.

TYPHOID FEVER—FORTNIGHTLY SUMMARIES OF NOTIFICATIONS, 1898–1904, INCLUSIVE, IN METROPOLIS, COUNTRY, AND NEW SOUTH WALES, AS A WHOLE.

	1898.			1899.			1900.			1901.			1902.			1903.			1904.		
	Metropolis.	Country.	N.S.W.	Metropolis.	Country.	N.S.W.	Metropolis.	Country.	N.S.W.	Metropolis.	Country.	N.S.W.	Metropolis.	Country.	N.S.W.	Metropolis.	Country.	N.S.W.	Metropolis.	Country.	N.S.W.
January 14	53	99	152	36	49	85	42	51	93	26	67	93	31	84	115	32	145	177	48	112	160
" 28	42	130	172	30	75	105	92	98	190	29	99	128	41	94	135	42	178	220	44	125	169
February 11	37	144	181	54	92	146	105	137	242	45	136	181	40	119	159	30	228	258	72	160	232
" 25	49	203	252	38	142	180	94	179	273	67	115	182	45	113	158	60	427	487	57	171	228
March 11	55	232	287	42	153	195	84	194	278	39	114	153	46	117	163	47	339	386	38	121	159
" 25	36	173	209	77	174	251	73	239	312	49	148	197	47	125	172	35	344	379	55	92	147
April 8	43	211	254	57	181	238	78	237	315	61	113	174	46	122	168	45	308	353	49	77	126
" 22	51	269	320	37	161	198	52	192	244	73	133	206	39	120	159	44	277	321	36	80	116
May 6	47	200	247	50	142	192	52	186	238	40	138	178	25	83	108	55	364	419	25	52	77
" 20	59	132	191	41	155	196	32	120	152	56	112	168	16	82	98	51	270	321	32	59	91
June 3	46	109	155	39	120	159	25	109	134	28	82	110	20	102	122	51	277	328	26	52	78
" 17	17	43	60	27	81	108	14	63	77	21	56	77	21	74	95	26	115	141	21	54	75
July 1	18	40	58	32	50	82	16	41	57	20	43	63	15	46	61	26	79	105	13	24	37
" 15	16	36	52	30	31	61	18	41	59	18	27	45	12	25	37	31	57	88	13	14	27
" 29	15	20	33	14	24	38	18	18	36	12	15	27	9	23	32	19	27	46	12	8	20
August 12	8	13	21	14	18	32	7	12	19	9	16	25	6	21	27	13	20	33	7	12	19
" 26	7	13	21	16	19	35	10	14	24	4	12	16	5	17	22	8	25	33	10	5	15
September 9	7	12	19	11	18	29	5	9	14	7	4	11	11	13	24	15	16	31	14	8	22
" 23	8	17	25	10	22	32	5	8	13	15	6	21	11	20	31	8	15	23	9	8	17
October 7	4	17	21	12	14	26	—	12	12	8	6	14	16	20	36	4	17	21	9	26	35
" 21	11	23	34	4	23	27	10	13	23	8	16	24	6	20	26	13	19	32	10	20	30
November 4	23	22	45	6	32	38	14	29	43	14	32	46	10	28	38	29	23	52	9	23	32
" 18	12	59	71	12	42	54	14	27	41	18	49	67	17	31	48	30	34	64	31	46	77
December 2	39	66	105	21	36	57	41	68	109	29	64	93	20	68	88	42	62	104	30	36	66
" 16	49	88	137	15	43	58	67	89	156	55	90	145	26	107	133	27	112	139	44	78	122
" 31	72	107	179	61	100	161	77	183	260	82	176	258	105	227	332	50	244	294	69	118	187
Total for year	824	2,478	3,302	786	1,997	2,783	1,045	2,369	3,414	833	1,869	2,702	686	1,901	2,587	833	4,022	4,855	783	1,581	2,364

1898 [Table C.

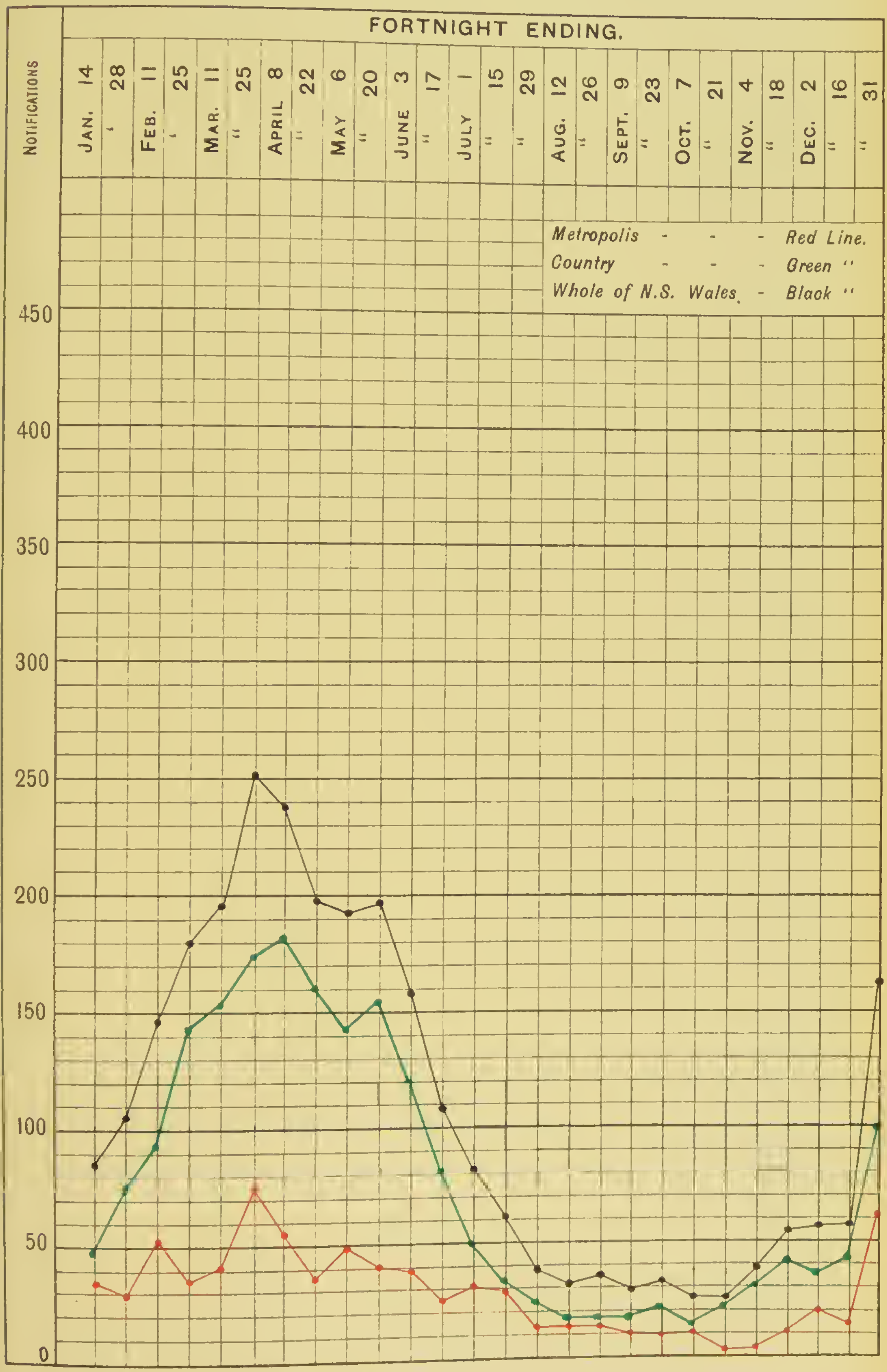
Typhoid Fever Notifications received at Notification Bureau during each fortnight, for Metropolis, Country, and whole of N. S. Wales.



SURVEYOR GENERAL'S OFFICE. ADELAIDE A Vaughan, Photo-lithographer

R. J. MILLARD.
30/8/05.

Typhoid Fever Notifications received at Notification Bureau during each fortnight, for Metropolis, Country, and whole of N. S. Wales.



SURVEYOR GENERAL'S OFFICE. ADELAIDE A Vaughan, Photo-lithographer

R. J. MILLARD.
30/8/05.

1900 [Table E.

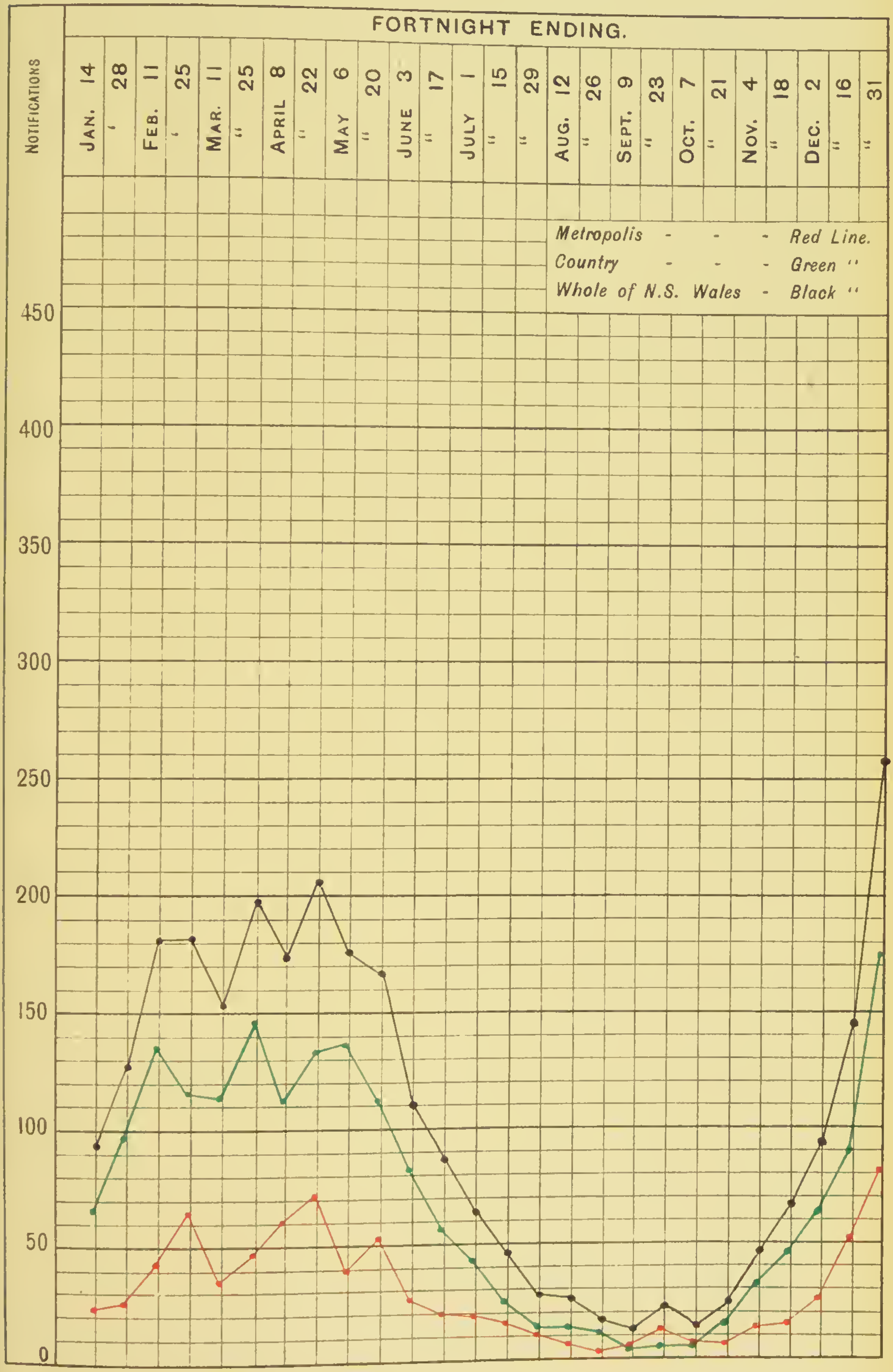
Typhoid Fever Notifications received at Notification Bureau during each fortnight, for Metropolis, Country, and whole of N. S. Wales.

SURVEYOR GENERALS OFFICE, ADELAIDE. A. Vaughan, Photo lithographer

R. J. MILLARD.
30/8/05.

1901

Typhoid Fever Notifications received at Notification Bureau during each fortnight, for Metropolis, Country, and whole of N. S. Wales.



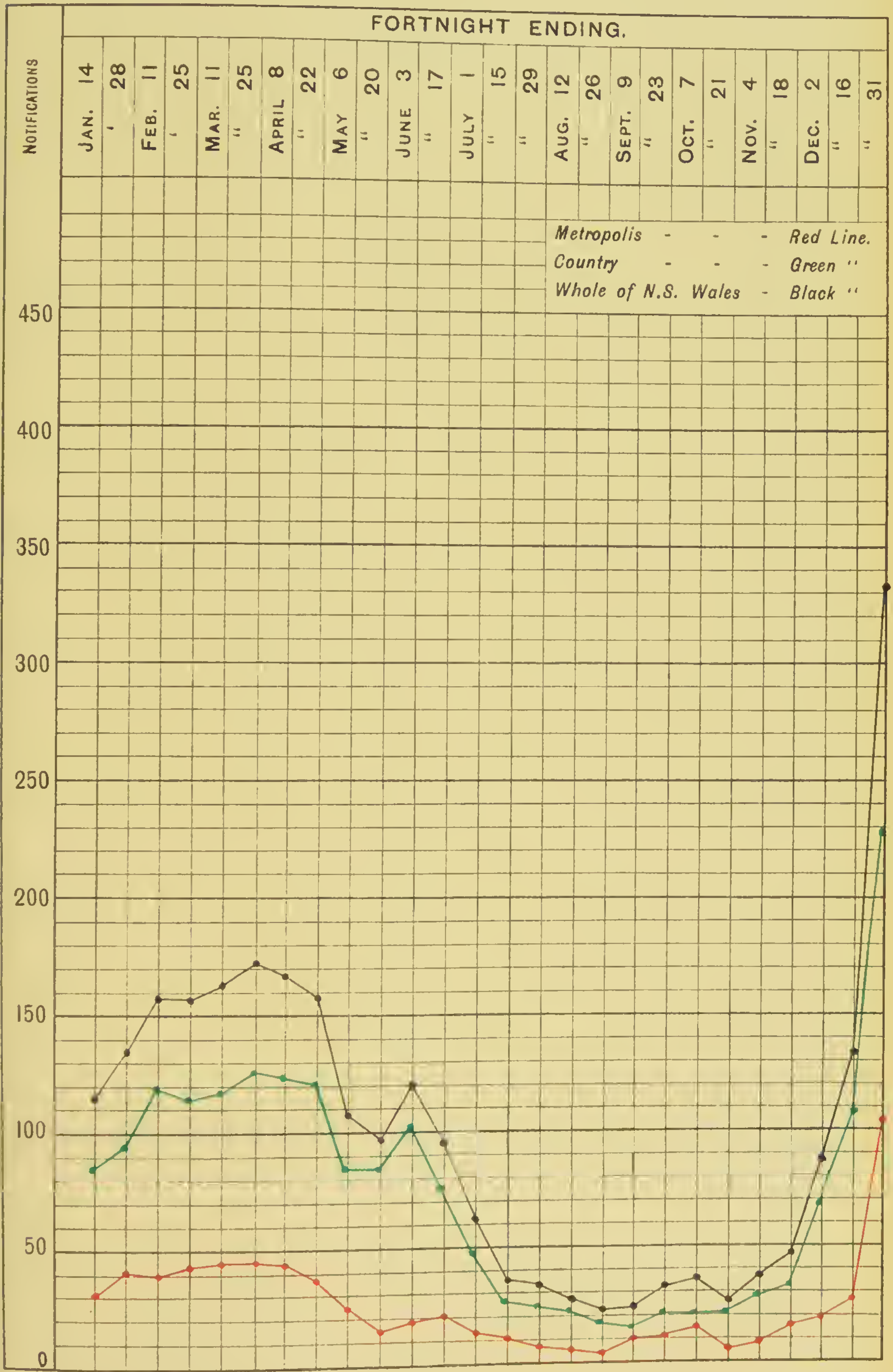
SURVEYOR GENERAL'S OFFICE, ADELAIDE A. Vaughan, Photo-lithographer

R. J. MILLARD.
30/8/05.

[Table G.

1902

Typhoid Fever Notifications received at Notification Bureau during each fortnight, for Metropolis, Country, and whole of N. S. Wales.



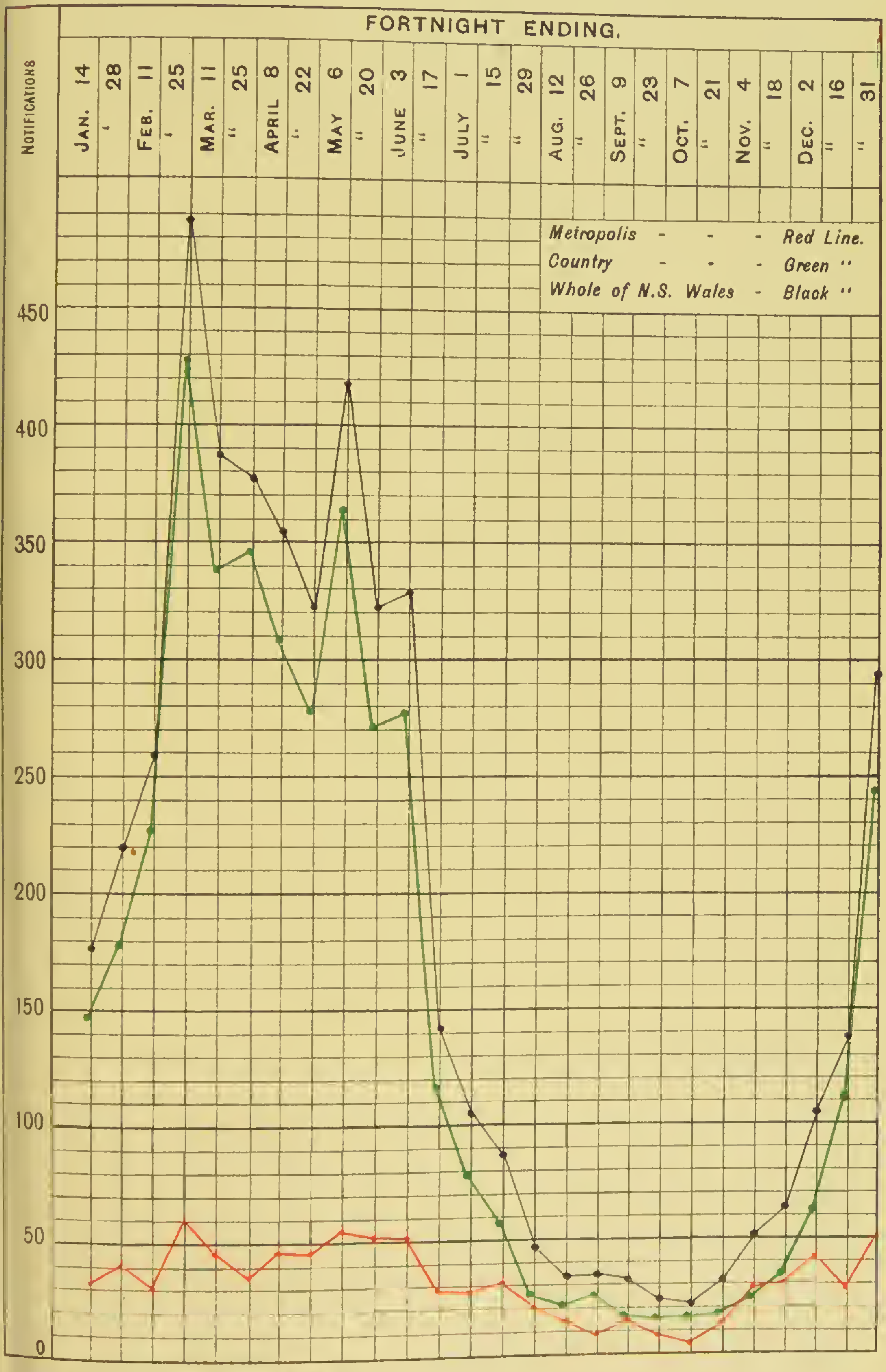
SURVEYOR GENERALS OFFICE, ADELAIDE. *A Vaughan, Photo-lithographer*

R. J. MILLARD.
30/8/05.

1903

[Table H.

Typhoid Fever Notifications received at Notification Bureau during each fortnight, for Metropolis, Country, and whole of N. S. Wales.

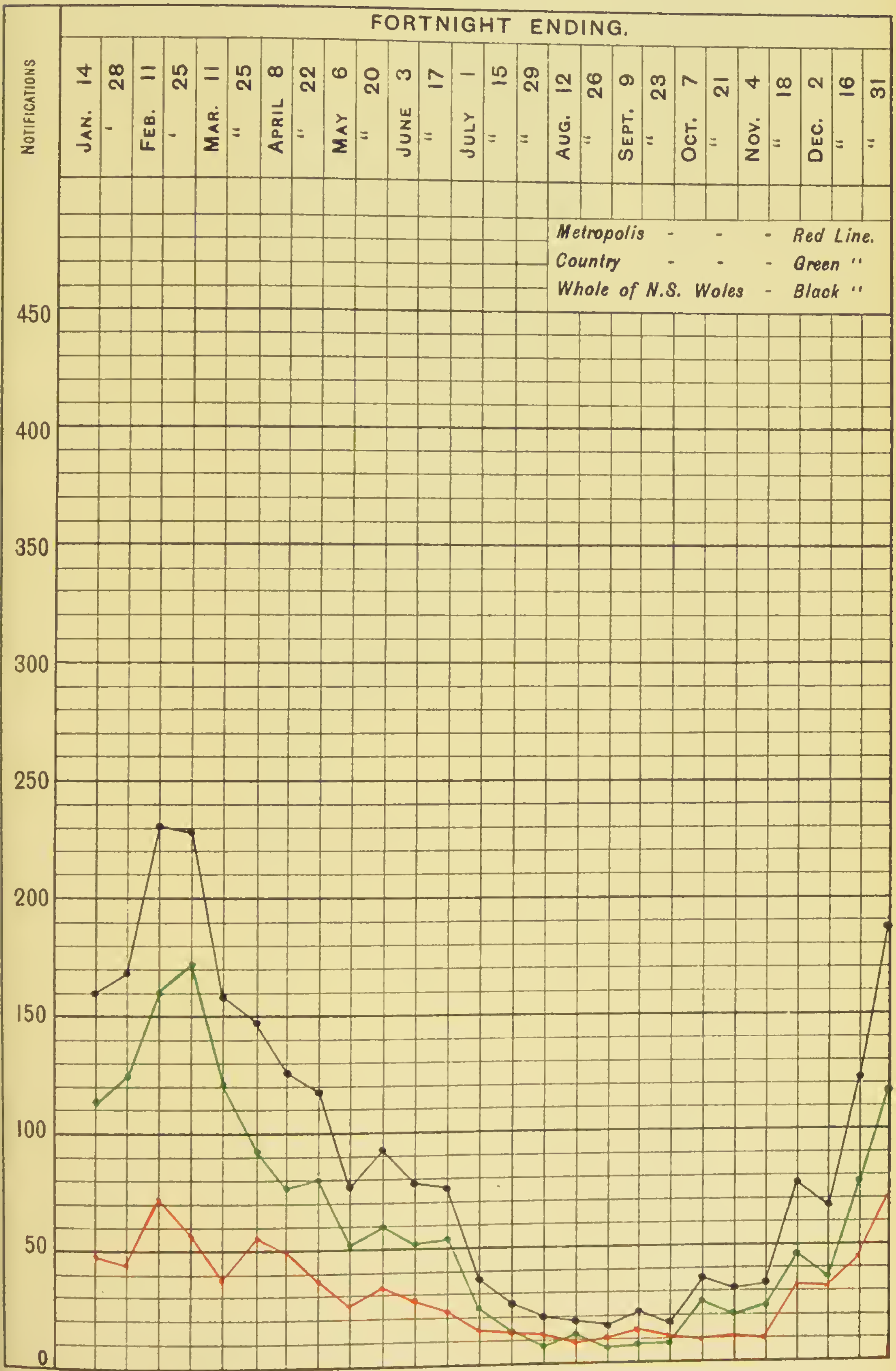


SURVEYOR GENERAL'S OFFICE, ADELAIDE. A Vaughan, Photo-lithographer

R. J. MILLARD.
30/8/05.

1904 [Table I.

Typhoid Fever Notifications received at Notification Bureau during each fortnight, for Metropolis, Country, and whole of N. S. Wales.

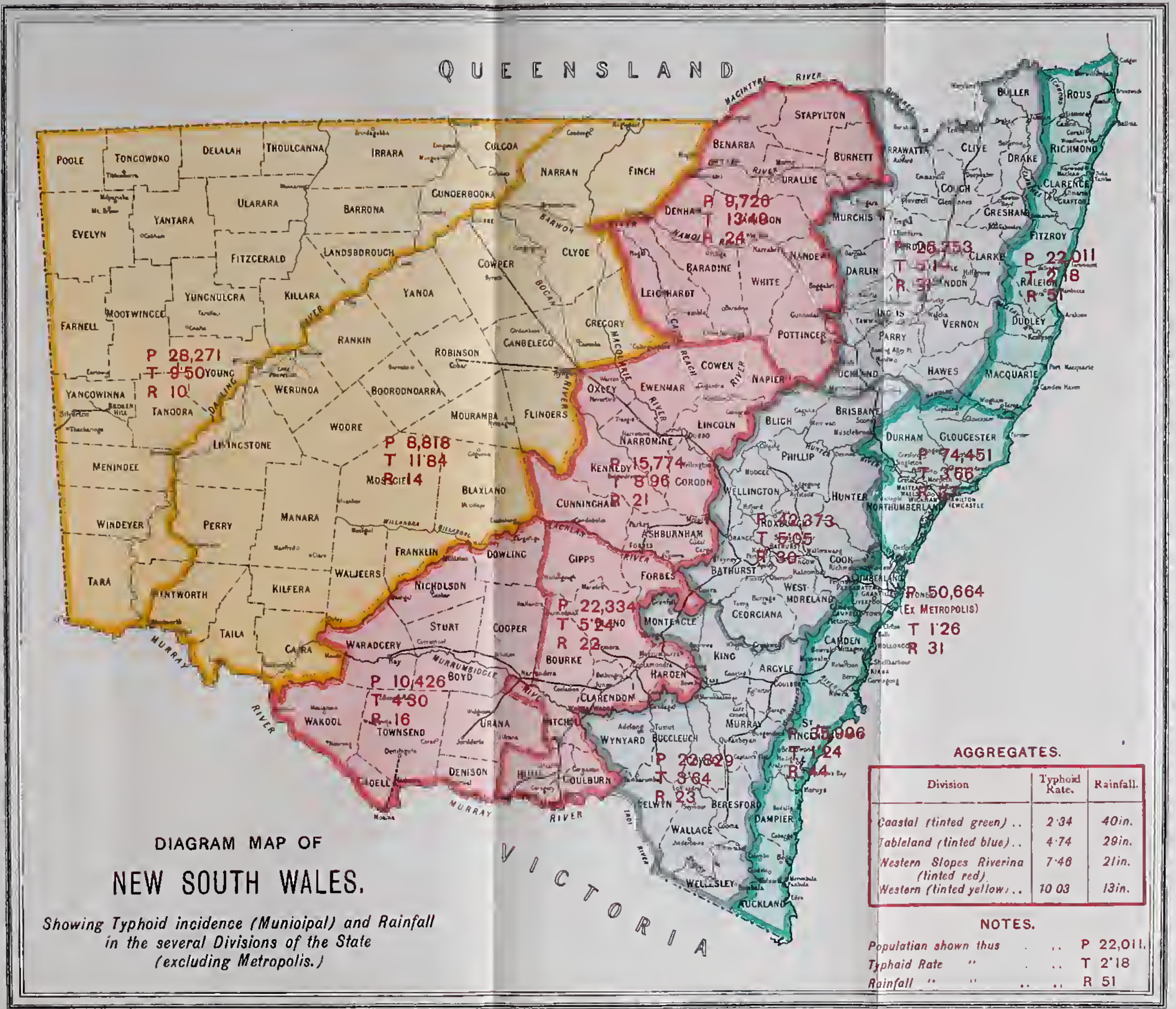


SURVEYOR GENERAL'S OFFICE, ADELAIDE A Vaughan, Photo-lithographer

R. J. MILLARD.
30/8/05.

TYPHOID FEVER IN NEW SOUTH WALES, 1898–1904.

By R. J. Millard, M.B., Ch. M. (Syd.), D.P.H. (Cam.)



SURVEYOR GENERAL'S OFFICE ADELAIDE A. Vaughan Photo-lithographer

SUMMER DIARRHŒA IN INFANTS: FROM THE PUBLIC HEALTH POINT OF VIEW.

By W. F. Litchfield, M.B.

To get a proper idea of the subject of Summer Diarrhœa in infants, it is necessary to take a glance at infantile mortality. This is calculated as the number of deaths in children under 1 year of age to every 100, or 1,000, births during the same period. The mortality of infants varies greatly in different countries. I may mention the following :—

New Zealand (10 years ending 1900)	8.3
Norway and Sweden	10.0
New South Wales and Victoria (10 years ending 1900) ..	11.2
Sydney " " ..	12.3
England and Wales (10 years)	15.0
London "	15.0
Manchester "	20.0
Berlin "	30.0

Thus it will be seen that Australia stands in a favorable position in this respect. Norway and Sweden is quoted by European authorities as having the lowest infantile mortality of any country. That of New Zealand, however, is lower; and, in 1904, Sydney, with its half million inhabitants, showed an infant mortality of under 10 per cent., to wit, 9.8 per cent. I notice that certain European authorities* speak of 10 per cent. as being a normal standard for infantile mortality. What they mean is that under present social conditions it is not likely to be less, but that an attempt should be made to bring it down to that level.

My own personal experience in Sydney led me, some time ago, to believe that acute diarrhœa directly or indirectly played by far the most important part in infantile mortality. Of 173 consecutive deaths under 1 year amongst my out-patients, 62 per cent. were due to enteritis, 16 per cent. to congenital syphilis, and 20 per cent. to other diseases, including congenital defect. The same was noticeable with regard to the case incidence : nearly all the bad cases could be traced to an accute attack of enteritis. A study of the general statistics confirms that view. I find that 50 per cent. of all deaths under 1 year in Sydney may fairly be attributed to diarrhœal disease. About 30 per cent. may be grouped under the heading of congenital defect and congenital disease, including congenital syphilis, while the remaining 20 per cent. is made up of a host of diseases, of which the respiratory take the chief place. Under diarrhœal disease I have included diarrhœa, enteritis, cholera infantum, dentition, tabes mesenterica, and atrophy : my own clinical experience is that they should be so included. Moreover, the seasonal incidence of these deaths corresponds in the main with deaths from diarrhœa. When you subtract the deaths due to congenital defect and congenital disease, which may fairly be called unpreventable, you have five-sevenths of all other deaths directly due to diarrhœa.

I find that during 1904 (a very favorable year as regards diarrhœal mortality), in Sydney alone, 626 children under 1 year died from this disease; and when you remember that there are a large number of children over 1 year, and a goodly number of adults, who succumb annually to the same disorder, you will perceive at once that it is by far the most deadly of Australian diseases. It causes many more deaths each year than does consumption, which has always been supposed to hold the pride of place as a death-dealer.

* M. Budin, of Paris, and the Ed. *B.M.J.*

I have been able to calculate corresponding figures for England and Wales* to those just given. They are :—

Diarrhœa	56 per 1,000
Congenital diseases	26 " "
Respiratory diseases	30 " "
Other diseases	39 " "

(See diagram.)

It will be noticed that, in relation to the total deaths, diarrhœa is more important in this country, while both absolutely and relatively respiratory disease is less fatal here than in England.

The disease is closely associated with summer weather conditions. Ballard made the important discovery that the mortality corresponded with a rise in the temperature, not of the air but of the ground, as tested by the four-foot thermometer. In England the disease is almost solely confined to the third quarter of the year. In this country the mortality begins to rise in October; it is most severe in November, December, and January, and then lessens month by month till September is reached, when it is lowest (see diagram).

Next, the evidence when carefully considered points to the disease being an acute specific infection. The seasonal incidence of the disease, its epidemicity, sudden onset, definite course, its pathological anatomy, and its behaviour in institutions, make that clear to my mind. Lately there has accumulated a good deal of evidence to show that the dysenteric bacillus of Shiga is an important, if not the sole, immediate cause of its production. This is a very important addition to our knowledge on the subject, and we owe it to American bacteriologists. Duval and Basset, Martha Walstein, and others have recorded series of cases of Summer Diarrhœa, from which the dysentery bacillus has been isolated. The most complete series was recorded by La Fetra and Howland. They found the bacillus in sixty-two out of sixty-four consecutive cases of Summer Diarrhœa, some of which were very mild. In conjunction with Dr. Hepsley, I found this bacillus in the stools of several cases of children suffering from diarrhœa in Sydney.† In a further number we established a specific agglutination re-action with the bacillus: moreover, we were unable to make any clinical distinction between those cases from which we obtained the bacillus and re-actions from the general run of other cases, and in every *post-mortem* examination made by us an inflammation of the large bowel was present. However, if it be thoroughly grasped that Summer Diarrhœa is an infection, then much of the confusion that exists with regard to it, and infantile mortality in general, will disappear.

The idea that it is caused by ptomaines is not supported by facts. The characteristic of ptomaine poisoning is the grouping of a number of cases around a certain food supply. Nothing of this is seen in Summer Diarrhœa. Moreover, the chief anatomical lesions are in the lower parts of the bowel, whereas they should be in the upper part of the bowel if ptomaines were the cause of the disease.

The part played by so called mal-feeding will be considered presently.

The next important thing to notice is that the mortality occurs chiefly in hand-fed infants. This looks at first sight as though the disease were a food infection. A careful consideration of the facts, however, seems to me to make it clear that that is not so. In the first place, infants fed exclusively on the breast do not altogether escape the disease. Richards,‡ in two series found that 14

* Culled from a table in article by G F. McCleary, in *B.M.J.* of August 13th, 1904.

† *A.M.G.*, January, 1899.

‡ H. M. Richards, *Journal of Hygiene*, July, 1903. This is an extremely important article (the factors which determine the local incidence of fatal diarrhœa).

per cent. and 11 per cent., respectively, of all deaths from diarrhœa were in infants fed solely by the breast. Holt, in a large number of deaths from diarrhœa, mentions 3 per cent. as being exclusively breast-fed. The case incidence, moreover, is much higher than the mortality incidence, because the prognosis in breast-fed children is better than in hand-fed infants, when the disease is contracted. In my own series there were 5 per cent. of purely breast-fed infants among the deaths; and I can say that a considerable number of infants who get no food but the breast contract enteritis every summer. Out of 224 cases recorded by me 44 were purely breast-fed.

Secondly, fatal diarrhœa is not uncommon in infants fed on foods that may be considered unimpeachable. I have seen this over and over again, in institutions and in private practice.

Thirdly, about one-half of the deaths occur in children who are being fed on one or other of the patent foods of which condensed milk is the chief. These foods are sterile. Richards gives the following figures :—In one series of 183 deaths, 33 per cent. were in infants on condensed milk. In another series of 191 deaths, 44 per cent. were in those fed on the same food. In my series of 242 cases (not deaths), only 59 were on cows' milk, 44 were on the breast, 47 on the breast and foods, 73 on condensed milk, and 17 on other foods.

Fourthly, sterilisation, in the shape of boiling the milk, or by the use of sterile foods, has been more or less universally adopted of late years, and yet the infantile mortality from diarrhœa has not correspondingly decreased. It is not possible, of course, to say that it is never communicated by means of food, any more than it is to say that scarlet fever, diphtheria, or typhoid fever is not communicated at times by that means; but I am convinced that food is not the vehicle of the germ in the majority of the cases.

The relation of mal-feeding in general to Summer Diarrhœa needs mention. It is customary for a general charge to be levelled against mal-feeding that it is the cause of the diarrhœal mortality. But if what has been said above about diarrhœa being an infection, and that that infection does not come through the food, is correct, it is obvious that mal-feeding *per se* is not the cause of that mortality. I must say that within my own experience it is very rare to see a death that could be put down to mal-feeding pure and simple. There is always some disease, such as syphilis or an acute enteritis, at the back of the trouble. One certainly sees a number of weakly infants of the illegitimate class that have not had diarrhœa; but here a general neglect is as much or more to blame than the food, and, as I have said, they do not die unless something more serious happens. Moreover, one does not find in the mortality tables a place for mal-feeding. If mal-feeding were a serious factor in itself for diarrhœa mortality, one would not expect to find an absence of the latter in two such warm months as September and October; yet such is the fact. What is the mortality among hand-fed infants? I doubt whether it is higher than the mortality among illegitimate infants; that is, 28 per cent. for Sydney. The mortality of infants boarded-out under the Infants Protection Act, and under the State Children's Relief Act, in Sydney, is put by Mr. Coghlan at 40 per cent. These infants constitute the least favorable class in the community, and the death-rate for hand-fed infants generally must be lower. My point is that it is not the mal-feeding that kills, but an acute specific infection; this will be clearer directly.

The fact remains, however, that breast children are largely immune to diarrhœa, and maternal nursing is the only sure prophylactic against that disease. My explanation of this fact is that the child receives from the mother protective substances, technically known as immune bodies, which prevent the disease from developing. There is no direct proof of this; and

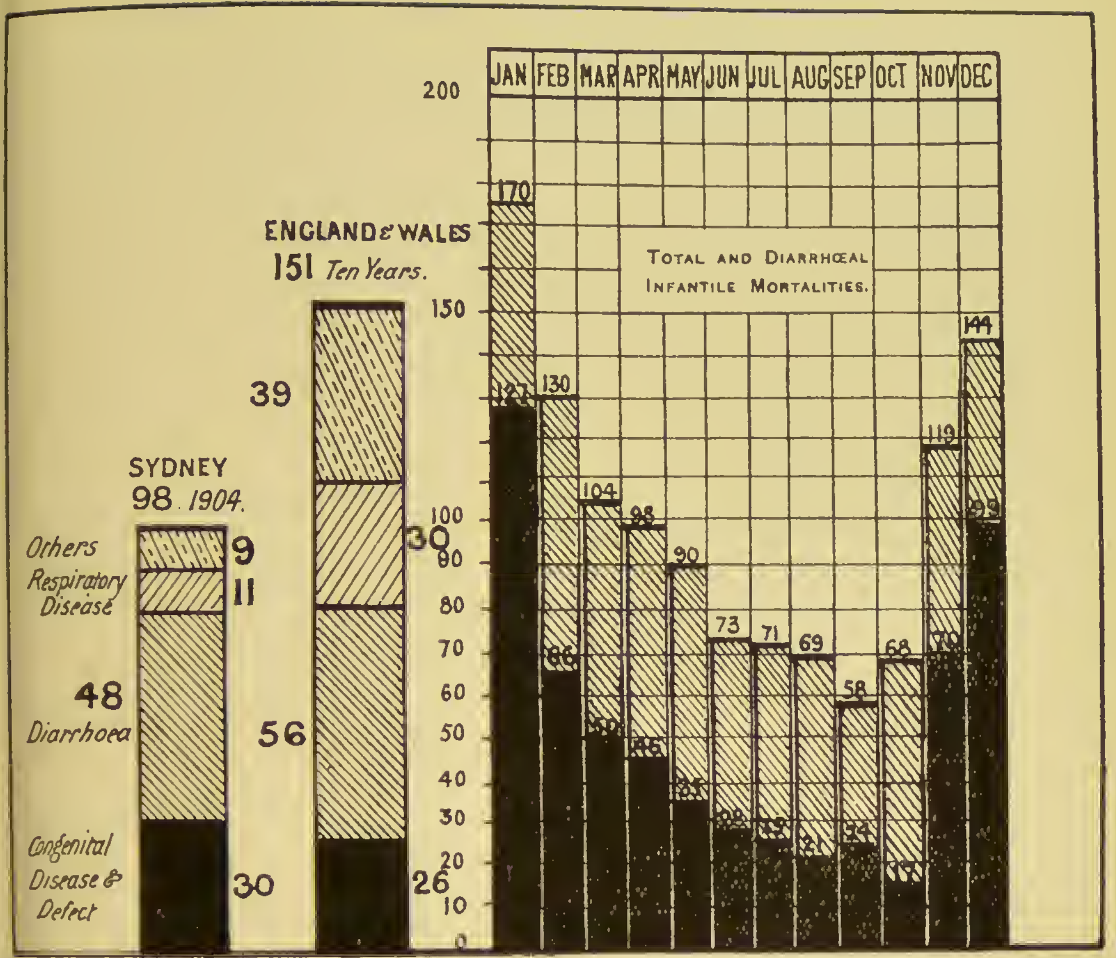
I arrive at it partly by a process of exclusion. It is known, however, that such substances in the case of other diseases are transmitted through the milk, and we have the circumstance that adults are resistant to the enteritis of infants. It may be argued that hand-feeding predisposes to a diarrhœal infection, by lowering the bodily resistance of the child. This may be so; but there is no evidence to show that it is an important factor. Richards, in his article, decides against it; and it is a common experience that healthy children, as well as poorly nourished ones, go down every summer to the disease in question. How, then, is the infection in Summer Diarrhœa conveyed? Well, there are lots of ways that an infection may be spread; but, in the case of the enteritis of young children, there is a good deal of evidence to show that it is spread through the air by means of dust. M. Richards concludes that the infection is closely associated with organic matter in the soil, and is spread by means of the dust. He looks to road sanitation as the most important element in prophylaxis.

The following facts support the dust theory:—(1) In wet seasons the mortality from diarrhœa is low. (2) In the suburbs of Manly and Hunter's Hill, which are almost surrounded by water, and which are therefore comparatively free of dust, the mortality from infant diarrhœa is almost nil. (3) At Glebe Point, Sydney, the general wards of the Children's Hospital are on one side of the main street, and the diphtheria cottage is on the other. Every year, in the general wards, sporadic cases of severe diarrhœa occur, while in the diphtheria cottage—during about ten years—no case has been observed. The only difference in the situation of the two places is that the diphtheria cottage stands well back from the road, and is screened by a number of trees and a well-grassed paddock; while the general hospital stands almost flush with the footpath.

Newsholm made an important observation with regard to infant mortality. He found that in overcrowded areas the mortality was not much increased unless there was overcrowding in individual houses; in other words, the number of houses did not matter greatly, but the number of individuals in each house did. This accords with opinions expressed by myself in an article on "Foundling Hospitals" (*A.M.G.*).* In that article I showed that infantile mortality was always enormous in institutions where a number of hand-fed babies were housed together. Some years ago I had an experience of a babies' home in a thickly populated position of Sydney. During the winter we got on fairly well, but during the summer bowel disorder was constant and death very frequent. In spite of careful sterilisation of foods and general cleanliness, wave after wave of diarrhœa used to go through the wards. Even the infants fed at the breast did not altogether escape. It was at that time that the importance of an ærial infection in diarrhœa was first impressed upon me.

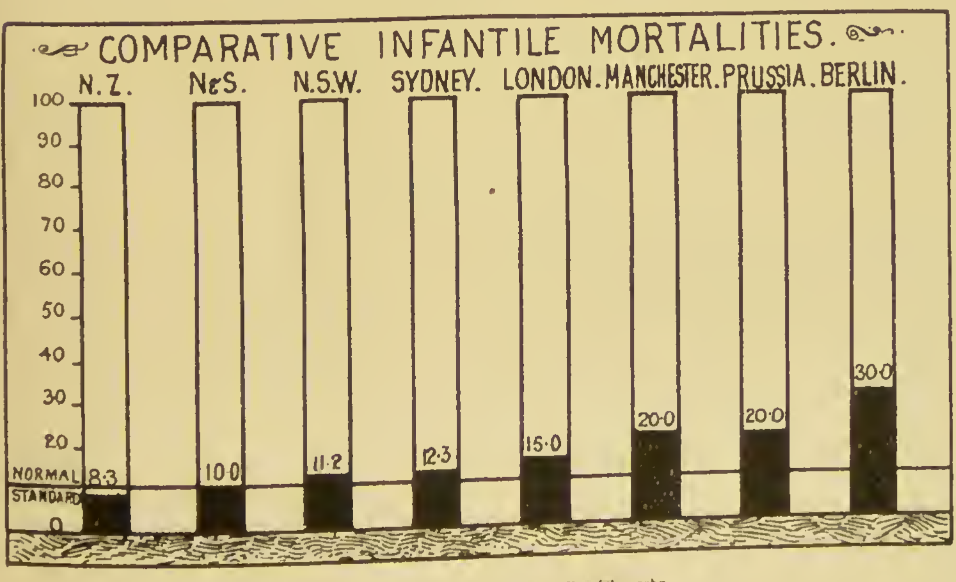
We come now to the consideration of preventive measures. Breast-feeding stands easily first in prophylactic measures. The mortality from diarrhœa in breast-fed infants must be very low. Richards found it to be, for Croydon, England, five per 1,000 infants so fed. Partial breast-feeding is better than whole hand-feeding. Unfortunately, however, under our present social conditions a considerable number of mothers cannot suckle their infants. Still I think, if it is thoroughly understood that when an infant is taken off the breast its chances of living are considerably lessened, and that it is one particular disease that is therefore likely to kill it, then some greater effort will be made in individual cases to persevere with maternal nursing. If my theory that human milk contains immune bodies is correct,

* *A.M.G.*, January, 1899.



Showing Deaths per 1,000 Births in Disease Groups. Infantile.

Showing Total and Diarrhœal Mortalities per Month. The Black Columns represent Deaths from Diarrhœa. This is for Sydney – during 1904.



SURVEYOR GENERAL'S OFFICE ADELAIDE A. Vaughan, Photo-lithographer

it affords a powerful argument with which to impress mothers. In this country the State has a large number of illegitimate children placed in its hands. The authorities should be well seized of the importance of keeping mother and child together during the first year of life.

Next, where hand-feeding is resorted to not more than one child should be reared under the one roof. One fault of the New South Wales Infants' Protection Act is that it permits a foster-mother to take up to three infants to look after. This is a serious blot, for reasons given above.

Next, in all cities a serious effort should be made to keep down the dust. I understand that there is a science in road-making. Well, roads should be so constructed as to yield a minimum of dust. The streets should be well watered. This especially applies to the summer months, for it is then that the disease is most prevalent. I think that tree-planting in the streets would have some effect in keeping down the dust. Houses well back from the road, and screened by trees and shrubs, might well be encouraged. Infants, too, should have a better chance in places away from the main thoroughfares. Then surely some discretion should be used as to the time of day when, and the place whereto, children are taken out for their daily walk. If a choice of residence is possible, some foliaceous waterside suburb, away form the main thoroughfares, ought to be favorable.

There can be little doubt that babies born at the end of the summer, say about February, are more favorably situated than those born at later periods. It would be interesting to have statistics showing the mortality of infants born in the different months. However, I do not suppose that we are ever likely to have a "baby season."

The advisability of cleanliness of person, food, and surroundings I need scarcely emphasize. The extension and perfection of the sewerage system, by keeping down the organic content of the soil, and by washing away diarrhœal discharges, must be of service as a preventive measure.

Such, then, are my views on the important question of Summer Diarrhœa in infants. The main point that I have tried to bring out is that it is an acute specific infection, and the present general tendency to put all the blame on the food is wrong. I know that in this I have run counter to those who have been blaming the milk supply for the excessive mortality in infants, but I think that a calm view of all the facts will convince the majority that milk has been unjustly blamed in the past.

TWENTY YEARS OF SANITARY PROGRESS IN MELBOURNE.

By James Jamieson, M.D.

The completion of twenty years of service as a health officer in a large Australian city may serve as my excuse for bringing before this Congress a short record of what has been attempted, and, in part, accomplished.

A comparison of different parts of the period is the easier, because the administration, with certain improvements, has been the same and the statistical methods similar. On the whole, indeed, the conditions have been very uniform, unless in one very important particular. The population of the city of Melbourne has not varied greatly during the period, and there has been only a trifling increase in area. In the year 1891 there was an improvement in the water supply, in respect both of quantity and quality, new sources being tapped; but no filtration method has at any time been introduced, otherwise than privately. The great change has been in the adoption of a system of

underground sewerage, bringing with it not only a drying and cleansing both of surface and subsoil, but also the almost complete abolition, in sewered areas, of the old and abominable pan system of nightsoil collection. Work was actually begun in 1897, and since that time has been steadily continued, so that it is now approaching completion, not only in the city but in most of the more central municipalities by which it is surrounded.

The city of Melbourne, with its population of rather less than 70,000, has the disadvantage of being thus surrounded, on all sides, by other independent municipalities, though it has compensation in possessing, in and close about it, a large area of public parks and gardens.

The staff most directly concerned in sanitary administration has been increased of late years, and may be described as fairly adequate. It consists of an Inspector of Dairies, Factories, and Lodging-houses; a Senior Inspector of Nuisances, with four assistants; and a female inspector (whose time is chiefly taken up with exercising supervision over cases of infectious diseases).

The meat supply is now carefully regulated; all carcasses, whether slaughtered in or brought into the district, must be branded, and special officers not only inspect the stock at the markets, but visit the butchers' shops at short regular intervals.

The milk supply is guarded in a double way. A qualified veterinary surgeon makes quarterly inspections of all cows kept in the city, or depastured in the parks adjacent; and the quality of the milk sold is frequently tested chemically by the City Analyst. On two occasions, also, a large number of samples were examined bacteriologically at the Melbourne University Laboratory, to which the City Council makes an annual grant. On the whole, it may be said, I think, that the food supply in all its kinds is fairly well guarded.

The important thing, however, is to discover what have been the results of the work, so far as shown by mortality returns. Dividing the twenty-years period, 1885–1904, into four divisions, it appears that the general rate of mortality, per 1,000 of population, fell successively in the following way on the average of the quinquennial periods:—1885-89, 19.48 per 1,000; 1890-94, 17.09 per 1,000; 1895-99, 14.65 per 1,000; 1900-04, 14.06 per 1,000.

The latest returns are the most favorable:—1902, 15 per 1,000; 1903, 13.8 per 1,000; 1904, 12.82 per 1,000.

While it may rightly be claimed that these results are satisfactory, it must also be admitted that most cities in Europe and America, as well as in Australia, show an improvement in their death rates, more or less closely resembling that here shown. It is only by examining certain details that the gain can be shown to have been greatly in the prevention of the diseases usually spoken of as preventable.

The infective diseases more strictly epidemic, as measles, scarlet fever, and influenza, vary so greatly at different periods, from causes so little known, that they cannot supply materials for a useful comparison. The prevalence of typhoid, on the other hand, is everywhere taken as providing a fair test of the sanitary condition of a town or district.

The number of deaths in quinquennial periods from typhoid has been:—1885-89, 223; 1890-94, 140; 1895-99, 103; 1900-04, 44.

These figures are sufficiently eloquent, if mere comparison of periods is made, though the population has fallen a little between the first and the last. The rate per 100,000 allows of comparison with other places. Things were at their worst in 1889, when the rate was 93.5, and at their best in 1903-4, when it was only 8.1. Taking London as the standard, a comparison shows that while, in 1889, typhoid was about five and a half times more fatal in Melbourne, in 1903-4 the rates were very nearly equal; that of the other great English towns averaging about 50 per cent. higher.

A variety of causes have, doubtless, contributed to the bringing about of this very remarkable improvement; but the main credit must be given to the introduction of the drainage system. In some German cities, notably Munich and Berlin, where a drainage system was also of late introduction, a similar result was got. Confirmation of this opinion is also found in the circumstances that, in the suburban municipalities, as a whole—in many of which the old methods of drainage and nightsoil removal are still obligatory, while the works are incomplete—the typhoid rate in 1903-4 was almost twice as great as in the city. In view of that fact we may hope that the limit of benefit has not even yet been fully got.

Though diphtheria is very distinctly a disease spreading mainly by personal contact, it is often taken as also supplying some test of the sanitary condition of a district. Taking the four quinquennial periods, as before, the deaths from diphtheria and croup numbered, in 1885-89, 131; 1890-94, 86; 1895-99, 53; 1900-04, 29.

It is true, of course, that diphtheria is a disease which has always shown great periodical fluctuations of prevalence, but the reduction of fatality has been so great and continuous that some other than meteorological and other general conditions must be assumed to have been operative. The benefit got has probably resulted largely from the system of notification, with consequent exclusion from schools, not only of children actually affected, but of other children from the house. Supervision of cases, with cleansing and disinfection of houses after recovery, has doubtless also had a share in the result.

The subject of infant mortality is one which, for many reasons, is at present receiving great attention. In a leading article in the *Lancet* of May 20th, 1905, a very strong statement on the subject is made :—" It is a strange satire upon preventive medicine to have to admit that, although our general death rate has undergone, and is still undergoing, a steady decline, our infantile mortality should show not only no substantial fall, but even in some instances a substantial rise." The following figures show that, however true this opinion may be as respects the English towns and cities, it does not correctly represent the condition in Melbourne. Taking again the periods of five years, and the average rate of mortality per 1,000 births under 1 year of age, this is the result :—1885-89, 194; 1890-94, 152; 1895-99, 156; 1900-04, 110.

In the two years, 1903 and 1904, the rates were respectively 105 and 83.4. In ordinary years the infant mortality rate is, in the main, co-incident with the mortality from the diarrhœal diseases, and, in fact, is essentially a question of feeding. It varies with season and atmospheric temperature, and the influence of these is chiefly by way of causing easy contamination or deterioration of food, and, above all, of milk. High temperature may be taken as acting chiefly in two ways: by increasing the amount of dust, carrying germs of various kinds, and by favoring germ-growth in milk, however contaminated. The deleteriousness of dust will depend on the amount of organic or living material which it carries, and that again on the comparative cleanliness or foulness of soil and surface adjacent to houses. It is also apparent that the keeping quality of milk will depend on the cleanliness and care with which it is collected, conveyed, and distributed. And there is the rather incalculable and unmanageable element of ignorance and carelessness on the part of mothers and nurses. The problem of prevention is clearly very difficult and complicated, and one which may be approached in many ways. Education on matters of general hygiene may do much, but, unfortunately, is slow in operation; but municipal action can do a great deal, though even that may be slow of effect.

Melbourne, in some respects, is not in a favorable position for attacking the problem. It is a dusty town, with a high summer temperature. It seems

hardly possible to allay the dust nuisance; but the dust may be rendered less harmful, as a food contaminant, by good drainage and careful street and lane cleansing. The improvement of our infant death rate is probably, therefore, due in part to the steady improvement in these respects, which certainly has been going on.

Safe-guarding the milk supply may be taken as the most important measure for the lessening of infant mortality; and for several years I have been in the habit of ascribing to measures of this kind the reduction in our diarrhœal and infant mortality rates. Quite a large proportion of the work performed by the City Analyst has been the examination of samples of milk. Whenever, in his opinion, there is clear evidence of adulteration, prosecution is instituted, and most of the benches of magistrates recognise the importance of the charge by fining severely. The result has been seen in the returns, which show a steady increase in the percentage of those milks which pass the "limit," and even pass the "standard." In 1904, further, out of eighty-three samples examined, only two were reported as containing boracic acid, or other preservative.

Bacteriological examination has confirmed what was shown by chemical examination. In the three years, 1900-02, ninety samples of milk were tested, by inoculation, for the detection of the tubercle bacillus, and five gave a positive result, though, in two of the guinea pigs used, only local lesions were produced. Of the ninety, fifty-four were tested in 1902, with a positive result in two only. In 1904 samples to the number of thirty were again tested, with a negative result in all. In the report for 1904 the following further statement was made:—"Incidentally, another important fact, bearing on the purity of the milk supply, has been brought out as a result of these examinations, *i.e.*, on previous occasions numbers of guinea pigs have invariably succumbed to peritonitis (septicæmia) within a few days of the injections. No such deaths occurred in the present instance; an observation which more or less justifies the following deductions:—(*a*) Absence of excessive numbers of bacteria, indicating general cleanliness in milking, &c.; (*b*) Absence of inflammatory condition of the udders."

I do not feel, therefore, that I can echo the pessimistic utterance of the *Lancet* (June 17th, 1905):—"We should be wrong probably in saying that there was not a pint of milk sold in London exactly as it comes from the cow, but we should not be far out if we said that at least—and probably more than—nine-tenths of the supply is not in its perfectly pure natural condition."

When it is seen that the infant mortality of London in 1904 was 146, and of the chief English towns 166, there is evident reason for statements of the kind quoted. There is, perhaps, reason also for the establishment of municipal milk depots, a step of not indisputed wisdom which has been taken in some English towns and cities. I have not felt called on to propose the adoption of any such measure, which does not seem urgently necessary, in view of the facts and figures adduced. It may happen that the low rate of mortality of 1904 will not be sustained, as the summer of that year was exceptionally cool and wet—though certainly not so cool and not wetter than that of England. But, the standard having been set, it will be incumbent on us to strive to keep up to it.

No field of sanitary activity has excited more attention, of late years, than that of the prevention of tuberculosis. Everywhere over the civilised world the "crusade against consumption" has been taken up; and, so far as evidence is yet obtainable, with a fair measure of success. If it has not been greater, the explanation may be found in the fact that the disease is, on the average, of rather long duration, and that the time is short since strict or systematic measures began to be taken.

It may be proper to show, first, whether there has been actual evidence of diminished prevalence and fatality in recent years; and, as figures are comparatively small, I have thought it better to compare the totals of successive quinquennial periods with the mortality rate per 10,000 of population: —

Mortality from Tuberculosis.

	Total Deaths.	Annual Rate per 10,000.
1890-94	1,021	28.2
1895-99	784	22.4
1900-04	711	21.4

There is a marked difference between the periods, and the contrast is even more pronounced when the first three and the last three years are compared:—

Mortality from Tuberculosis.

	Total Deaths.	Annual Rate per 10,000.
1890-92	665	30.2
1902-04	404	20.0

On either method of comparison a very manifest improvement is shown, the mortality rate having been reduced by quite one-third, in the fairly remote periods shown in the second table. The disappointing feature is the comparatively slight gain in the last of the three periods of the first table as compared with the second; the most satisfactory is that the last year, 1904, gives the lowest number of deaths, 116, and the lowest rate, 17.1.

That there has been substantial gain cannot be doubted; the question remains about the causes by which it has been produced. It must be confessed that no large share can be ascribed to the special precautionary measures which have been coming into use since the institution of the so-called "crusade against consumption." Rather more than five years ago serious endeavors to carry out these measures were begun in the city. In every instance of death reported as due to phthisis the house was visited, instructions given for cleansing and disinfection, and wherever necessary the room or rooms chiefly occupied were cleansed by washing down and stripping roof and walls, and renovating afresh. This is ordered to be done by the owner; but if fumigation is regarded as sufficient, the walls are rubbed down and fumigation by formalin done by the corporation officers. When articles of clothing or bedding could not be easily washed, an endeavor is made to have them burned. It was soon evident that such action, only or chiefly after death, did not properly meet the needs of the case; but, in the absence of any provision for notification, nothing seemed possible but to appeal to the hospital authorities for reports of cases admitted to the wards or attending as out-patients. There was no great response for a time, and it has only been since notification has been in force, even rather ineffectively, that cases in considerable number have become known. It is clear, therefore, that the great reduction between 1890 and 1899 could not have been due to the measures just described. I believe they were due mainly to increased attention to general sanitary precautions; better house construction; more regard to reasonable ventilation; and great improvement in the making and cleansing of streets and lanes, and the regular removal of household waste. A portion

of credit must also be given to the effect of legislation, in regard to shops and factories, by the prevention of over-crowding and the insuring of proper lighting and ventilation.

But there was another form of action which, I believe, had considerable influence in many ways, and not least perhaps in the way of phthisis prevention. Even before I entered on duty there had been instituted a system of house inspection, taken up chiefly with the internal condition. When, from structural defect or bad repair, a house has fallen into a condition making it unfit for human occupation as a dwelling, it is so reported in a certificate signed by the Health Officer and Building Surveyor. The owner has about six weeks allowed to make repairs and improvements, which must again be certified as satisfactory by the same officers. If he fails to do so, or to get extension of time for reason given, the house is condemned, and must be vacated. This compels action of some definite kind, one direct result in many cases being that the house is pulled down. Special attention has always been given to signs of dampness on walls or under floors, the repairs demanded including means for its correction. In the nineteen years 1886-1904, no fewer than 2,006 houses were thus certified, and no fewer than 546 were pulled down without much delay. Nearly all the others were repaired, to the satisfaction of the officers, within about the time allowed. It thus happened that, all through the period, without special thought of phthisis prevention, action was taken on a large scale which must have been beneficial in that way.

The lesson is that anything of the right kind, in the direction of sanitary improvement, brings its reward, and leads to benefit in ways not only foreseen and intentional, but in others that were not expected—perhaps not intended.

THE ADMINISTRATION OF THE HEALTH ACT, 1898, IN ADELAIDE.

By T. Geo. Ellery, Esq.

I would like, in the first place, to compliment Dr. Jameson on his paper and the good results that have followed during his long tenure of office. I am conscious of being placed at a disadvantage in following so eminent a hygienist, and I cannot be expected to deal with a similar subject in the same high scientific manner; but, as I appear before you as an administrative unit who has only a plain tale to unfold, I feel assured of your attention.

In dealing briefly with my subject, I hope to initiate a discussion on the equipment necessary to ensure effective sanitation in Australasian cities.

I have used the term "Australasian cities," not so much on account of any inherent difference between these and the cities of other countries as to show that we have considered this question at least from the Australasian standpoint, and in the light of local experience. The fact that the cities of Australasia itself differ so much in many ways will complicate the arrival at general conclusions, but the co-operation in the discussion of representatives from these cities should lead to something definite being laid down in the direction indicated.

By sanitary efficiency I mean reasonable practical results in all the branches of sanitary science embraced within the scope of the Health Acts in force in the various cities.

The Health Act of South Australia deals with the sanitation of premises and their surroundings, food supplies (chiefly meat and milk), and infectious diseases; and I propose to show what is being done, and the results, to some extent, of the work in Adelaide.

1. *Sanitation.*—Adelaide has a population of 39,240 (Census, 1901), with 10,498 tenements (houses, offices, shops, and hotels), within an area of 1,042 acres (exclusive of park lands, gardens, and squares). There is a staff of four inspectors, or one for every 10,000 of population or every 2,600 tenements. These inspectors, although not in possession of the Royal Sanitary Institute's diploma, on account of there being no facility for obtaining it in this city, are fully trained and competent men. Each has special duties assigned to him, and also takes part in the systematic house-to-house inspection of the city, in addition to doing a fair share of the clerical work attached to the service of notices. The special duties are allotted by the Medical Officer of Health as follows:—Butchers' premises and slaughter-houses, hide and skin marts, offensive trades premises, produce stores and salerooms, cowyards and dairies public w.c. and urinal accommodation, livery and bait stables, marine stores, restaurants and bakehouses, are inspected and reported on monthly; public and private schools, common lodging-houses, and banana-ripening rooms, quarterly; boarding-houses, hotels, private hospitals, and maternity homes, yearly; markets, spitting on footpaths, and a general observation of smoke emissions also receive attention. In addition, one of the inspectors acts as disinfection officer at the final disinfection of houses, and another carries out duties chiefly in regard to obtaining samples of milk for analysis under the Food and Drugs Act. The results of these arrangements have been found to be satisfactory. As regards house-to-house inspection, it required three years to complete the first sanitary survey of Adelaide. This was partly owing to the survey being the first systematic inspection made, and, as a result, necessitated more work than otherwise would have been the case; but, with the staff at its present strength, it is not anticipated that at any time the ground will be covered more than once in two years. With so long an interval between systematic inspection of premises, minor nuisances recur and may pass unobserved for a time, but this is met to some extent by noting the places likely to give trouble and visiting them at frequent intervals. I may mention that the card system of recording the sanitary history of the houses is in operation.

It is difficult to state the result in terms, but I think the members of this Section will find, if they apply a personal test, that the general sanitary condition of the city is good.

During the past five years 417 houses have been condemned as unfit for human habitation, and 243 of these were pulled down, while the others were made habitable.

In all minor matters the sanitary inspectors sign and serve their own notices, and copies of them are registered in the Secretary's office. If any are not complied with in the specified time, the Health Officer returns the docketed copy to me with a minute as to what action he desires taken. If legal proceedings are required, the notice must be adopted by the Board of Health, and the necessary authority to prosecute be given.

Major matters—such as declaring premises unfit for human habitation, ordering old insanitary houses to be demolished, requiring premises to be partly rebuilt, requiring extensive paving and drainage works, &c.—are

remitted direct to the Board of Health, who make the necessary orders, under seal, and the notices under the orders are signed by myself as Secretary; the same book records being kept as in the case of inspectors' notices.

Under section 76 of the Health Act, legal action may be taken by the inspectors, with the Secretary's authority, in regard to nuisances without serving any notices whatever.

The removal and disposal of house refuse are under the City Surveyor's department, and he is no more satisfied with the methods adopted than the Health Officer or myself. The refuse is conveyed in open carts to a "tip" on the park lands, where the evils inherent in all tips are minimised by covering up the refuse as soon as it is tipped, and by preventing rag and bone picking at the tip. I may add that the substitution of properly covered vans for the present carts is under consideration, and that a refuse destructor would have been in course of erection now but for influential local opposition to the site selected. As I have strong views on these matters, I would like to hear Dr. Jameson's opinion of the operation of refuse destructors in Melbourne.

The deep drainage system is under the control of a Government department, and, so far as can be ascertained, no outbreak of disease has been associated with it. The drainage system has been established for over twenty years.

Factories are also under Government inspection.

2. *Food Supplies.*—Small cattle are slaughtered on butchers' premises in the city, large cattle at the city slaughter-house. This latter is an antiquated structure, and would have been replaced by a modern abattoir but for the reason which has delayed the erection of a refuse destructor. Inspection is consequently superficial.

Milk supplies are protected by combined action on the part of the city and suburban local boards, which ensures inspection of premises and examination of cows within the metropolitan area. A small combination, comprising four local boards, employ a veterinary inspector. A larger combination comprises the city and fifteen other local boards, who have appointed a veterinary surgeon with a special training in dairy science as Inspector of Cattle, who is under my control with Dr. Borthwick as Consultant Medical Officer. The periodic visitations of these inspectors is supplemented by the more frequent visits of the local sanitary inspectors. There are altogether 728 registered dairies, over 4,000 cows, and 25 milkshops within the area. Premises have been rendered much more sanitary, cows are better tended, and adulteration with water is less frequent than formerly; so that a better and purer milk has been the result of the combined action. The Inspector of Cattle has a fully equipped chemical and bacteriological laboratory on the Town Hall premises for the analysis of milk.

Water supplies are under the control of a Government department as far as the city is concerned, and I may say that no outbreak of disease has been traced to the water supply.

3. *Infectious Diseases.*—The diseases are specified by the Health Act. The usual restrictive sections of health Acts are operative, and compulsory removal to a hospital is permitted under certain conditions. This latter power it has so far been unnecessary to exercise, as no difficulty in this direction has been experienced. Power is given to erect isolation hospitals, but this has not been taken advantage of. At present the Adelaide and Children's Hospitals, as well as private hospitals, take in cases of typhoid fever; the Children's Hospital has made admirable provision for diphtheria in children; some of the private hospitals have limited isolation accommodation for

infectious diseases generally; the Government finds provision on the Quarantine Station for exotic diseases, like plague and small-pox. The staff engaged in the administration of this part of the Act are a trained nurse with a special knowledge of the theory and practice of disinfection and a sanitary inspector who is told off for special duty when required. The apparatus at their disposal are formalin lamps (Alformant) and Equifex sprayers; the latter having almost completely displaced the former. A metal box is provided for the removal of infected articles, which cannot be otherwise disinfected, to be burned at an open grid. As soon as a case is reported the trained nurse visits the house, advises as to the best mode of isolating the patient in the house, and shows how to prepare and use disinfectant solutions. She also leaves (after explaining them to the person in charge) printed forms for guidance in maintaining effective isolation and disinfection during the progress of the case. Particulars, specified on a card, in regard to employment, milk supply, school attended, &c., are obtained by the nurse for information and guidance of the Health Officer. If the patient's family is poor, disinfectants are supplied gratis, and in that case bacteriological examinations can be also obtained free by the medical attendant. The trained nurse keeps in touch with the case throughout, and at its termination directs and oversees the disinfection of the contents of the room, leaving the latter ready for final disinfection by the inspector: this he accomplishes by spraying it thoroughly with formalin solution, and if anything requires to be destroyed by burning he also sees it done personally. Unfortunately, Adelaide does not possess a steam disinfector; but as this formed part of the plans for a destructor, its absence is similarly accounted for.

As to results, I shall place a few figures before you, supplied by the Health Officer. During the last five years, *i.e.*, since the Health Act came into operation, there have been notified 230 cases of typhoid fever, of apparently local origin. Of these 161, or 70 per cent., were removed to hospital (public or private), leaving 69 to be treated in their own homes. Secondary cases occurred in 6 of the 69 houses, or in 8.7 per cent. Of diphtheria, out of a total of 222 local cases, 138, or 62 per cent., were removed to hospital, and among the 84 isolated at home secondary cases occurred in 5 houses, or in 6 per cent. Of scarlet fever, out of a total of 365 local cases, 112, or 30 per cent., were removed to hospital, and among the 253 isolated at home secondary cases occurred in 28 houses, or in 11 per cent. In all these instances of secondary infections the houses were of the poorest sort, where effective isolation was impossible.

The following tables contrast average rates for the period of five years immediately preceding the passing of the Health Act, and the period of five years during which the Health Act has been in operation:—

	Per 1,000 Population.		Per 1,000 Births.
	Birth Rate.	Death Rate.	Infant Mortality.
First period	27.79	18.88	160
Second period	23.84	16.99	138
Last year of second period	22.35	15.77	117

It should be stated that, while correction has been made for deaths in the Adelaide Hospital, Destitute Asylum, and Lunatic Asylum, this has not been done in the case of the Children's Hospital, private hospitals, maternity homes, and private houses. This accounts for the apparently higher death

rates compared with some other Australian cities. The same vitiation affects the next table. This want of uniformity in regard to statistical rates ought to engage the attention of this Section :—

	Mortality per 10,000 Population.			
	Typhoid.	Diphtheria.	Diarrhœa.	Phthisis.
First period	4.6	1.2	8.1	19.1
Second period	1.8	1.3	4.6	16.8
Last year of second period	0.7	0.7	3.0	15.8

I wish to emphasize the fact that the condition of water supply and drainage have been the same during these ten years and for a period antecedent sufficient to eliminate any effect on them. The diminution in the above death rates must thus largely be accounted for by more effective sanitary administration. During the first period no attention was given to infectious disease, and ordinary sanitary work was at a very low ebb. With the Health Act came a revival in the direction I have already indicated.

I venture to submit that the results obtained by the trained nurses' work in regard to isolation and disinfection have been satisfactory, and might raise the question whether it is necessary to erect expensive isolation hospitals to accommodate all, or nearly all, cases of infectious diseases. Without entering into a discussion on this matter, I may point out that experience in Adelaide shows that it is absolutely essential to have hospital accommodation sufficient to take in cases occurring in lodging-houses or in houses too small to afford satisfactory isolation, among dairy employés and others concerned in the manufacture or distribution of food, and among domestic servants.

I have attempted to describe the sanitary administration of Adelaide, with some of the results obtained under it. The appointment of a trained nurse to supervise cases of infectious diseases at their own homes is the special feature which I would urge other cities to imitate. Her work has been invaluable, and could in no sense be replaced by that of a male inspector; moreover, its educational value must tell indirectly. The other feature of administration which has been prominently successful is the combined action of the metropolitan boards of health in appointing a highly qualified veterinary surgeon to safeguard the milk supplies. So satisfactory has this action proved that it appeals to me as a solution of many problems in the sanitary administration of such areas, and I should like an expression of opinion on the results obtained by any similar combinations from health officers with experience of their working. I have also pointed out in what directions we fail in equipment. In my opinion Adelaide requires the addition of at least one more inspector to relieve the others of special duties. Adelaide is a city naturally favored in its situation and environment, and its staff of inspectors cannot be regarded as a standard for other cities less favorably placed. Adelaide also stands in need of an improved system of removing house refuse, a refuse destructor, an abattoir on modern lines, a steam disinfector with the necessary adjuncts, and a small hospital for the isolation of the cases I have mentioned. I take it we may safely affirm that other cities of Australasia require similar equipment, if not already provided. I trust that city health officers will give us the advantage of their experience generally, and so enable this Section to formulate some resolutions which will carry authority when presented to administrative boards.

NOTES ON THE PUBLIC HEALTH LEGISLATION OF AUSTRALASIA.

By J. R. Baker, LL.B., Cantab.

It may be fairly said that the Legislation of Australia and New Zealand, for the promotion and protection of Public Health, has not kept pace with the advances in the application of the principles of public hygiene. No doubt of recent years Act after Act has been placed on the Statute books of the different States, but none of them come up—although some of them approximate—to the ideal.

Speaking of the States now under review, the unavoidable mortality of any particular city differs little from that of any other city of about the same size, but it is in the avoidable causes that there is much room for improvement; and, to effect this improvement, recourse must be had, in the first instance, to the Legislature.

Now, sir, I purpose to approach the subject from the point of view of the layman who partially occupies his attention in administration, and not from the standpoint of the expert. Looking forward from the vantage ground of the expert there is only one unit really worthy of consideration—namely, the expert. From a layman's point of view, handicapped by his limitations as regards his special knowledge, it is apparent from the outset that there are three units, which are elementary, but which each demand attention in their respective spheres of action.

We have, first, the scientific unit—the expert—who is embodied in the highest degree in the medical officer especially versed in this particular branch, and in a lesser degree in his under-studies, the inspectors, of varying degrees of intelligence.

Second—the lay administrator, who is exemplified in central or local boards of health.

Third—the general public, for whose benefit and advantage sanitary laws operate.

These three units make up the whole world within which public health enactments have their place. We find in the vanguard, right in the front, the expert; always enthusiastic, though at times perhaps apparently unreasonable, and always impatient of the drag that is exercised on his actions by the other two parts that make up the whole.

We have next the lay administrator, anxious on the one hand, so far as his knowledge will allow him, to follow the expert, and dragging after him—slowly, perhaps—the third unit, the general public; apprehensive on the one hand of not being able to keep up with the leader, and on the other hand of getting ahead of his followers, at all times, however, a salutary check—or perhaps you would put it a necessary evil—on the impulsiveness of the expert, who is generally ready to break away from the main body and fight too far in front, running the danger of having his lines of communication cut. I claim, on behalf of the second unit, not only is he a necessary link in the chain, not only does he exercise a beneficial break on the impulses of the expert, but, without him and without his co-operation, and without the exercise of his more limited technical knowledge, the administration of public health matters would—at any rate in a democratic community—be practically impossible.

We have in the third place the third unit—the general public, the body that has to be operated upon; always for its good, but not always to its liking. This unit has to be persuaded, urged forward, sometimes coerced, sometimes perhaps cajoled into acquiescing in the forward movement of the expert and

less speedy movement of the lay administrator. I have no doubt that you, who for the most part consist of experts, are somewhat impatient of the check exercised by the second unit, and I have little doubt that you all really think, although you may not state so in so many words, that you could very well do without this second unit, and the public health matters would benefit generally by its exclusion. You cannot expect me, of course, to acquiesce in this view; nor do I in fact believe it to be a correct one. In my opinion the ideal health legislation would be one that would best effect the synchronised working of all these three units, or at any rate would most approximate to such synchronisation; and it is having this in view, and having the operations of these three units in their various spheres in immediate consideration, that I propose to very shortly review the health legislation of the various States of Australasia and of New Zealand. For the purpose of comparison I deem it advisable to broadly state my ideas of the ideal, without, however, attempting detail.

The Ideal System.

1. A Commissioner of Health, under a Minister, with the necessary officers and inspectors. I say advisedly "necessary," as the expert is apt to overrun the constable in this respect.

The Commissioner would be, of course, an expert specially suited for the purpose—a man of science and a man of tact, not altogether an impossible combination. This quasi-autocrat should have especially the following powers:—

(*a*) Complete control of quarantine. This may become, in the near future, federal.
(*b*) Power to act in all cases of emergency on his own initiative, subject to a modicum of control of his Minister. These cases would be rare, *e.g.*, plague.
(*c*) Power to make by-laws for government of district boards and local boards, subject to the approval of the Government. So far as district boards are concerned, power not to be exercised except in case of default or inefficiency. Minister to be the arbitrator.
(*d*) Concurrent jurisdiction with district and local boards, in any place where neither exist, then exclusive jurisdiction.
(*e*) Power to recover expenses from boards in cases of non-compliance.

2. District or county boards for centres of population, consisting of delegates from associated local boards, with powers limited to—

(*a*) Protection of food supplies.
(*b*) Control of infectious diseases.
(*c*) Construction and control of abattoirs, and, in particular in this respect, an exclusive jurisdiction, *e.g.*, the prohibition of the sale of meat slaughtered elsewhere.
(*d*) The use of hospitals and sanatoria for the isolation of infectious and contagious diseases, with power of compulsory removal of infected persons where necessary.
(*e*) The cremation of refuse.
(*f*) Milk and dairy inspection and regulation.
(*g*) Revenue to be raised from constituent boards.
(*h*) By-laws. In case of dispute Minister to be arbitrator.

3. Local boards, being the present local authorities where they exist, to deal with the more elementary but no less important duties of health authorities.

In order to attain the above ideal the question of increased expenditure must be faced, but this should not fall altogether on the local bodies. Refuse

destructors must be erected out of municipal funds, and will bring in little or no revenue. On the other hand, abattoirs, erected also by municipal funds, will pay for themselves. But, in my opinion, isolation hospitals and sanatoria should be erected and maintained out of the general revenue of the State.

Matters in connection with water supply and drainage are not included, as in the majority of the States they are in the hands of the Government.

Type I.—South Australia.

I take this State first, not because it is by any means the best—far from it—but because I am better acquainted with the subject-matter. We find—

1. Central Board of Health, with a medical chairman, which has the general control of health matters in the State. Its powers are concurrent with local boards, with a right to recover expenses in any cases in which it has to step in and compel local boards to carry out their duties. This board deals with quarantine and similar matters, and in case of any serious epidemic, such as plague, it is the only body which could deal with it promptly; but its machinery is inadequate. It might with advantage be abolished in favor of a Health Commissioner.

2. Local Boards of Health are constituted by every municipal and district council: they are charged with the administration of the whole of the health machinery within their respective areas. Their powers are full enough for all practical purposes. They fail, however, to satisfy in many respects the requirements of metropolitan areas, by reason of the independent action of each board within its own boundary, and the insufficient appliances for inspection of food supplies and control of infectious diseases. With the help of the Government the metropolitan boards have done what they can, having regard to their independent instead of combined jurisdiction in these matters; but they are not capable of much further expansion in the direction of efficiency, except on the lines of voluntary combination for specific purposes, which have proved to be unsatisfactory. I except from these remarks a voluntary combination in regard to inspection of dairies, which has turned out very well.

3. There is provision for a combination of any number of local boards to exercise all or any of the provisions of the Health Act—called a County Board. This has not "caught on" generally, although a partial suburban combine on a small scale exists, which, so far as I know, works satisfactorily.

We are a long way short of my ideal in this State.

Type I. may be taken as representative of Victoria and Western Australia, where, however, their legislative Acts are more comprehensive—and, let us hope, more effective.

Type II.—New South Wales.

1. There is the Board of Health, which supervises generally the carrying out of the Health Acts by local authorities, and, outside municipal areas, exclusively administers the Acts. It has concurrent jurisdiction with local boards. There is, I believe, no system of local government in New South Wales similar to that of other States, and consequently outside towns the Board of Health takes charge by means of police officers.

2. The municipal councils are local boards of health: they may combine to work disinfectors and establish abattoirs.

3. The Governor may, and has in at least two instances, proclaimed districts. The local authorities remain distinct, but the medical officer of health is appointed by the Governor for the whole of the district, and, by this means, to some extent concerted action is possible. No doubt this is a step in the right direction; but, taken altogether, Type II. does not correspond

with my ideal in several important respects, more particularly in defective metropolitan combination and absence of administration by means of local bodies which do not exist in New South Wales.

Type III.—Queensland.

1. Here we have a Commissioner of Health acting under a Minister of the Crown, who is charged generally with the administration of the Health Act. There appears, also, to be in connection with the Commissioner a Central Board of Health, but it is merely advisory, and has no executive or administrative functions—in fact it appears to be a body whose duty it is to hold up the skirts of the Lord Chancellor. The Commissioner has the usual concurrent jurisdiction with local boards, and the necessary powers to enforce such jurisdiction.

2. Local boards within their own area administer health matters.

3. There appears also to have been a metropolitan joint board for the prevention of infectious diseases, which had a jurisdiction within a radius of twelve miles of Brisbane. This board, however, did not prove to be a success, and was abolished.

Speaking generally, the Queensland Act seems to place somewhat unrestricted powers in the hands of the Commissioner. Whether these produce friction or not would of course depend to a large extent on the person who administered them. The general scope of the Act recognises, for metropolitan areas, a jurisdiction too divided to coincide with my ideal.

The Tasmanian Act gives the Chief Health Officer under a Minister of Health very large powers. There is no Central Board of Health, and the Act is to some extent intermediate between this and the following type.

Type IV.—New Zealand.

1. We find in New Zealand no trace of the central board, or its equivalent, but we find in lieu thereof that the State is divided into several (at present six) health districts, by proclamation.

2. The Chief Officer of Health, as well as the district officers of health, are appointed by the Government. The latter administer the Acts in the several districts in conjunction with the local boards; their powers seem to a large extent to run together, the district health officer being enabled, in case of default, to carry out the provisions of the Act at the expense of the defaulting authority.

3. The local authorities are local boards of health in their several departments, and, in conjunction with the district officers, administer all health matters. I am given to understand that very little friction arises between the district health officer and the local authorities: this speaks well for the intelligence of both. There being so many towns of importance in New Zealand, it may be thought my ideal would be difficult of application; but I do not see any difficulties in the way of such application, and, though I must plead entire ignorance of local circumstances, there seems no reason why in any country metropolitan combines should not be in vogue and operate for the general benefit.

The ideal which I have set up aims at an improvement in the administration of the higher branches of sanitation. The chief fault in all existing legislation is that there is no discrimination between populous centres and country districts. More especially is there no serious attempt to deal with metropolitan areas, and other populous centres (which, after all, demand the greatest attention in every State), in a truly scientific manner; and it appears to me that the chief advance in future legislation must be in remedying these defects.

SCHOOL HYGIENE.

By J. S. C. Elkington, M.D., D.P.H.

The enactment of compulsory education by the various Australian States, in common with other civilised peoples, has carried with it certain far-reaching consequences of profound importance to the race, and it is only within the past few years that these consequences have come to be recognised in their true light. It is not my intention here to deal with the social changes which, within the past twenty or thirty years, have arisen from the operation of compulsory education in its spiritual aspect, nor to attempt to forecast in detail the still more profound changes which appear to be inevitable in the near future. These are not by any means foreign to the subject of School Hygiene ; but, in the time at my disposal, it is impossible to attempt to discuss anything more than elementary principles, and to incidentally allude to their application.

In adopting compulsory education the State practically reduces education to a State monopoly. Private schools are recognised co-equally, however, with State schools as qualifying children for conformance with the requirements of the Acts, and School Hygiene cannot be regarded as State school hygiene alone. The State responsibilities cover all classes of schools, however, and the State directly undertakes the care of the vast majority of all school children. Hence it behoves the State to lead the way in any system of educational reform, and particularly in that aspect which is now under discussion. Education is a biological process, whereby certain discrete growing organisms are in process of being fitted to their environment in order to render them more useful to that concrete organism, the State. The environment has a physical as well as a spiritual side, and nine-tenths of those children who undergo the process of fitting will depend for their success and happiness in life almost wholly upon the physical side--in other words, upon the integrity of their bodies and not upon the degree of culture of their minds. Some two-thirds of them, moreover, will be the parents of the next generation, and, in the nature of things, the stamina and *æquanimitas*—to use that portmanteau-word in which Professor Osler sums up, as did Pius Antoninus before him, so much of import to the race and to the individual—of that generation will largely depend upon the bodies of the children now in our schools. The greatest of the faults committed in the name of education in the past has been the non-recognition of this physical side, either as an all-controlling factor in the development of the spiritual side, or as an all-important influence upon the child's future. It is impossible to deny that this oversight has resulted in an incalculable waste of public and private money, and in a monstrous injustice done to thousands of Australian children. These are strong words, and I utter them with a due sense of their seriousness. School is not merely a preparation for life, but an important part of life itself, wherein body and mind are alike plastic, and capable of almost infinite development for good or ill. In the German Imperial Health Manual a great truth is summed up in a pithy sentence—"The sense of justice demands that, in a State where attendance at school is compulsory, the children shall be exposed as little as possible to dangers to health while in school." Far-reaching as is this principle, I submit, however, that a sense of justice demands more than this. It requires the practical recognition of the fact that, not only shall children be protected from the many physical risks induced by the entirely artificial process to which compulsory education submits them, but that they shall be practically and intelligently instructed in the few simple rules by which they may carry the process of protection on to their after-life.

It requires, further, that justice shall be done to the taxpayer, in that the immense amount of money which he contributes for the purpose of compulsory education shall be expended to the best purpose; in that, by sensible instruction and care at school, there shall be saved a proportion of the further sum which he contributes for the treatment of unnecessary preventable disease, and in that large sums will not continue to be wasted in attempting to force symbolic information through eyes that cannot correctly see and ears that cannot properly hear, in order to reach brains somnolent and unresponsive through carbonic-acid poisoning.

It is scarcely necessary to remind this learned audience that the school hygienist is no longer to be regarded as a crier of trifles in a wilderness of fads. His methods and aims are based upon the common sense of everyday professional practice, and upon the elementary laws of physiological knowledge. The educationist and the hygienist must work hand in hand, and nobody recognises this more fully than does the educationist. Professor Nicholas Murray Butler has stated the question from an authoritative educational standpoint—"The most grievous single obstacle in the way of the spread of sound educational principles," says he, "is the popular view that the essentials of education are limited to instruction in reading, writing, and arithmetic. It might fairly be argued, and with no small force, that the possession of so much knowledge alone is a positive detriment to a human being; especially if that knowledge has been gained at the expense of physical and moral habits, which, in educational value, far outweigh any such meagre intellectual attainment . . . . It is not too much to say that health, its provision and protection, is all-controlling in present day educational theory, although it is unfortunately far from being so in practice. The chief reason for this discrepancy between the ideal and the real is simple ignorance. Teachers and parents do not recognise that eyesight is being impaired, normal growth prevented, blood poisoned, and the body starved because the hours of school life are so often unhealthy and abnormal hours. . . . . School buildings are constantly erected with a view to external effect alone, and an adequate system of ventilation and a proper site are pronounced too costly. It is not true that a child is always and everywhere better off than running at large in any village or city. If the classroom is already overcrowded, if there are already too many pupils assigned to a teacher, then every additional pupil who is brought in injures those who are already there, and receives injury himself." These weighty words were uttered from a chair of Education. Could we, as reasonable beings, expect plainer speaking or more destructive criticism of the "three-R's-at-any-price system" from the most iconoclastic professor of Hygiene that ever laid sacrilegious hands upon a domestic or a national muck-heap?

The work of the educationist and of the hygienist are then complementary. The neglect of hygienic requirements in school life not only injures the most valuable assets possessed by the State, but it absolutely debars the most skilful and accomplished educationist from obtaining the best mental re-action, and hence wastes time and money. In turn, the educationist must awaken the sanitary conscience early in life before any real or lasting advancement can be made in national hygiene. I am not of those enthusiasts who predict that the schoolmaster will be the sanitarian of the future. The schoolmaster will never replace the sanitary administrator, but it is only through his—and particularly her—work and co-operation that we can hope to attain to that national physical morality wherein preventable disease shall appear in its true light as somebody's crime, and the preservation and protection of health shall be accepted as a sacred duty. This imples no ardent forecast of a sanitary millennium, in which all minds

shall go attired in the chaste drab garb of hygienic seriousness, and the young man's fancy, before turning lightly to thoughts of love, will impel him to look up the family history of the beloved for traces of tuberculosis or alcoholic tendencies. It does, however, imply a hope that, from due recognition by the State of the vast economic importance of School Hygiene and of properly directed hygienic instruction, there will accrue, amongst other important results, a notable increase in the national common-sense, an intelligent appreciation of scientific medicine as distinguished from quackery, and a notable decrease in preventable disease in all its protean shapes and all its far-reaching consequences. The life of civilised mankind grows more and more artificial every year, and the standard for even moderate success rises higher and higher as man's busy brain overcomes the impossible of to-day and fits it into the daily round of to-morrow's work. The need for a sound well-cared-for body remains as great as in the days when the primal savage wrested a living from uncompromising nature. Children must, then, be taught to live ; they must be protected against the early consequences of that civilisation which afterwards demands so high a standard of fitness, and which treads down so remorselessly the unfit. The most daring cannot reconcile the cruel demands of racial fitness for the physical extinction of the weak with the humanitarianism which inspires modern hygienic practice. We must not—cannot—obliterate the unfit, we cannot simplify the ever increasing demands of civilisation ; hence we must strive, by all possible means, to prevent the originally fit from being rendered unfit, and to eliminate, as far as possible, the conditions which tend to accentuate existing unfitness. It is a common-sense business proposition after all—the fitter the population can be rendered in our schools, the better value do we get for our money. Conversely, every child who leaves school damaged in health by defective school conditions, every child who passes through school life hampered by practically removable causes which have prevented him from getting twenty shillings' worth of useful knowledge for each pound of taxes or fees spent upon him, and every child whose time has been wholly spent in being fitted to a comparatively useless spiritual side of an environment which is almost wholly physical, represents an unwarrantable waste of money, energy, and time—practically amounting to criminal negligence.

Destructive criticism is proverbially easy, and I need scarcely say that School Hygiene does not confine itself to pointing out defects. Practical and readily attainable measures for their removal or mitigation are essential. The object of School Hygiene in all its bearings is to increase and enhance educational results, and it must be severely subordinated to this object by the study of pedagogic requirements, and by due appreciation of the numerous difficulties—structural, administrative, and financial—which stand in the way of a perfect system. All hygienic defects may be classed under two main headings : defects of structure and apparatus, and defects of management. Each is complementary to the other : a defectively constructed school may hamper excellent management, and, on the other hand, ignorant or careless management may render nugatory the advantages of a well-planned and well-fitted building. In practice, however, defects of management are, for several reasons, more important than are defects of structure. A skilful teacher, versed in elementary hygienic principles and practice, can overcome or mitigate much structural wrong-doing ; and no proposal to pull down and re-erect on proper lines every structurally defective school building, or part of a school building, can come within the scope of practical politics, however sapiently we may argue from the standpoint of national economy. Further, as has been already indicated, we have to depend largely upon the teacher for the intelligent and effective employment of any improvements which may

be introduced into future schools. For my present purpose, State schools and State educational administration only will be considered, as affording at once the widest field and the most useful example; although it is necessary to avoid the fallacy of believing that private schools are free from defects, or require less attention. In my experience, defective as State school buildings have often been found to be, they are, *qua* hygienic structure and management, far superior, as a class, to private schools.

The training of teachers and inspectors in School Hygiene, hence, assumes the place of first importance; and it cannot, I think, be argued that the compulsory inclusion in the teacher's curriculum of such training would impose an unnecessary or unbearable burden upon these usually much-trained individuals. Certain pertinent arguments may be here suggested as showing that the question is one of vital interest to the teachers themselves. Where a superannuation fund exists, as in South Australia and Tasmania, wholly supported by State teachers, it is to the indubitable financial advantage of this class of individuals to eliminate or dilute as far as possible all causes which tend to disable or kill contributors, and hence to draw upon the fund. No improvement is likely to cripple the fund by turning teachers into a race of centenarians. The teacher is susceptible to bad hygienic conditions in schools—not to such a marked extent as are the children, perhaps, but nevertheless to a sufficient degree to produce serious damage to a body already weakened by any cause. This effect of insanitary school conditions upon teachers forms by itself an important question in School Hygiene, and one which should appeal strongly to the influential and numerous class which it affects. Further, under a system whereby a teacher's prospects in his profession are largely influenced by the educational results obtained by him, as ascertained by inspection, it is obviously only fair that physical obstacles to mental re-action—such, for instance, as adenoid growths, or myopia—should be assessed at their real value, and that the children who are thus defective should not be lumped in with their normal brethren. For this to be carried to its best conclusion, a system of medical inspection becomes necessary; but, failing this most desirable provision, a fair degree of practical and skilful training of teachers and inspectors would enable the majority of cases of this kind to be at least suspected and allowed for. I am aware that, by such exemption or separate consideration, some special provision for certain defectives would ulitmately become necessary, as has been the case in London, in order to secure such degree of education as they are capable of receiving, and to prevent neglect. In addition to the identification and separate consideration of defectives of these classes, and the consequent removal of much misery and heartburning now undergone by certain unfortunate children who are punished for faults beyond their control, results obtained elsewhere show a distinct increase in mental re-action from measures of School Hygiene. To quote a single instance, it was found by Carnelly that the installation of mechanical ventilation in three schools resulted in an increase of passes in reading, from 95.6 to 99.6; in writing, from 90.4 to 97.8; in arithmetic, from 84.2 to 92.4; and in the grant earned, from 19s. 6d. to 21s. 8d.—as compared to fifteen schools ventilated by natural means only. I do not desire to use this as an argument for the universal introduction of mechanical ventilation, but as an instance of the beneficial influence of improved hygiene upon the prospects of a conscientious teacher working under a system of classification. Reasoning upon these lines, a series of twelve lectures in School Hygiene for Teachers has been delivered in Hobart during the present year, with the cordial co-operation of my valued colleague and friend, Mr. W. L. Neale, late inspector of schools in South Australia, and now Director of Education in Tasmania. The results have been most gratifying; and although sufficient time has not elapsed to permit of the

collection of any data as to the possible educational influence of such instruction from an examination point of view, a notable advancement has occurred in the hygienic management of a large number of our schools.

For these and other reasons the instruction of all teachers in School Hygiene is likely to be of much benefit to themselves, as well as to their pupils. A further argument in its favor is to be found in the necessity for recognising those signs of fatigue, physical or mental, which are characterised by the late Inter-Departmental Committee on the Model Course of Physical Exercises (England) as forming to the experienced teacher "the gauge on which he keeps his eye in regulating the work of a class." School stress is far more a matter of defective hygiene than of mental strain; and I view with extreme disbelief the attacks which are sometimes made on the State educational curriculum as being, *per se*, a cause of breakdown in children. There may be more of it than is absolutely necessary from a utilitarian standpoint, but, given sound hygienic conditions, there is not enough in the most comprehensive State school curriculum in the Commonwealth to produce any worse effect than a little intellectual dyspepsia.

Two arguments are occasionally heard, which may be here quoted. One is to the effect that physical culture will probably overcome any bad effects from school conditions. I need not attempt here to explain the absolute futility and unsoundness of such a statement, or to demonstrate the impossibility of attempting to overcome the effect of a couple of hours of somnolence in an atmosphere heavily charged with air sewage by a few minutes of club-waving and toe-touching. I have no intention of decrying the excellent results which may be expected to accrue from a well-planned and carefully executed system of physical culture for schools, but it is, in my opinion, a dangerous procedure when carried out in the absence of a system of medical inspection, whereby the weak lungs and weak hearts may be safeguarded. As Dr. Mackenzie states, "The intention of physical culture is not to cultivate the organism in spite of existing defects. That would only lead to disaster, as no doubt it frequently does." The Royal Commission on Physical Training (Scotland) has collected some startling information on this subject, which, despite the difference in conditions, may well be studied by Australian educational authorities. The second of these objections is summed up in that single word "expense," which almost invariably forms the earliest war cry of the opponent to reform. I have already stated certain arguments which go to show that the maintenance of existing conditions is likely to result in the waste of much more money than would be required for the most drastic hygienic reformation of State schools. Setting this aside, however, I am prepared to state, from actual experience, that not only is it no more expensive to erect schools on moderately hygienic lines, but that it is actually cheaper. There is no need, therefore, to perpetuate, in future schools, the hygienic defects of many existing ones.

The addition of an extra subject to the teaching curriculum carries with it no gigantic financial responsibility, and may even be calculated to relieve the State finances to some degree in respect of sick-leave, compensation, &c., granted to teachers for temporary or permanent break-down. It will further tend to limit the interference with educational work, and consequent expense now caused by certain preventable school diseases, and will enable better value to be got for the taxpayers' money. The inauguration of a system of medical inspection of schools will, undoubtedly, require the outlay of money, although not to a very great extent commensurately with the benefits to be expected. Without going at present into its economic features, I would mention that in most of the American States—and particularly in New York, that home of the hard-headed utilitarian—medical inspection is in full swing; that in Switzerland, the admitted leader of the educational world, every school child and every

school is frequently and thoroughly inspected ; that in Belgium, Hungary, and several other European countries, it has existed for years ; and that Japan possesses a vast organisation, wherein are utilised the part or whole time services of some 8,400 medical men. The Scottish Commission on Physical Training has recommended it, and England is shortly going to get it—unless the portents read much awry. We may hence assume that it pays, and that it is no new and daring device of wild sanitarians for ruining the country.

The practical instruction of children in the elementary laws of health, and of health-protective purpose, involves no material expenditure, and can be productive of nothing but good. Unfortunately there has been in the past rather too much done in this way for the "child as he should be," and all too little for that common object of the schoolroom the "child as he is." There has been too much book, and too little treatment of the subject in a live practical fashion calculated to render the knowledge of Elementary Hygiene as wide spread as is the knowledge of reading : there has been too much of a tendency to gravely inform the Australian twelve-year-old that "indulgence in alcohol is detrimental to the human organism," and too little instruction in the benefits to be derived from the proper use of a toothbrush, and the disgrace and danger of spitting, with regular practical demonstrations of the methods of ventilating and even of cleaning the schoolroom. The formation of hygienic habits of breathing, and above all the strenuous enforcement of correct habits of working attitude, from the day the child takes his place at his usually misfitting seat, have been neglected in the past. All such teaching must be intensely practical, and the habit-formation tendency of the youngest children must be fully utilised. Physiology is useless until the basic laws of healthy living have been learned from example and precept, and those pretty demonstrations with lamp glasses and a cigar box, or with limewater and a glass tube, so affectionately described in the books, should be only employed after a working school sanitary organisation has been formed amongst the children themselves, coincidently with a rigid standard of personal cleanliness. It is not implied that any preponderating amount of the school day should be occupied with Elementary Hygiene, or that all other teaching should be subordinated to it ; its economic importance, however, warrants its inclusion as a frequent subject of instruction. As a pedagogic subject it needs no apology, in that it teaches the child to see, to reason, and to remember ; it lends itself both to training and instruction, and it possesses high ethical value. In conjunction with its allies, domestic economy and civics, it practically conforms to that science of Eugenics towards which the highest educational thinkers are striving as the coping-stone of modern education.

It will be well to consider a little more in detail certain of the defects, particularly of structure, to which I have alluded. Here I do not refer to Tasmanian schools alone, but to a number of Australian schools concerning which information has become available, either by study of plans or by personal visitation. The trail of the "practical builder"—that curse of school construction—is to be picked up almost everywhere in the evidences of disregarded orientation, neglected lighting, defective ventilation, absence of perflatory facilities, expensive bell towers, and fretted stone window-borders, dust trap interiors, and all the other evidences of his diseased love of architectural frippery. The educational authorities in Victoria, at the instigation, I believe, of Dr. Norris, have apparently succeeded in checking to some extent the machinations of the school architect ; and in Western Australia, well-meant attempts are made to secure hygienic decency in the newer schools. I trust I will be excused for saying, however, that most of the larger and more

ornate school buildings of the Commonwealth, other than those of quite recent date, display the methods of the slattern—all show and pomp without; all dark, foul, and difficult to cleanse within.

A school building is erected for a particular purpose, namely, to contain, for several hours daily during a large number of days in the year, a number of young animals of naturally dirt-collecting and dirt-depositing tendencies, particularly prone to attack by certain pathogenic agencies, and assembled for the purpose of receiving instruction principally through their eyes and ears, of which the former organs are physiologically liable to injurious strain. It is thus only reasonable that, in planning and constructing such a building, very particular attention should be given to its lighting, to its air supply, and to those internal and external conditions which have to do with preventing the accumulation and facilitating the removal of organic and possibly infective material. The children assembled in such a building come from scores or hundreds of separate abodes, and a school is, hence, capable of forming a remarkably effective centre for the dissemination of disease. That schools do act in this way is well known to us all; and the masterly work of Dr. (now Sir) Shirley Murphy, and of Dr. Kerr of the London County Council Education Department, has put this question on a new and significant footing. Nevertheless, the sole object of the school-builder in the past appears to have been to devise a fabric as much as possible resembling a cross between a Chinese pagoda and a Gothic barn, and to set that building square with the nearest street or road, regardless of the sun's track or of anything else, excepting a desire for apparent neatness and regularity. Having outlined his building, he appears to have proceeded to cut up its interior into various rooms, passages, assembly halls, and so on, according to its size, and to stick on—still with a pathological desire for neatness—such adjuncts as were required to make it resemble the last school of that type which he perpetrated. He then put in the windows at regular intervals all round, quite irrespective of size, or of direction of existing light relatively to the seating lines; pepper-boxed a few fireplaces and things about in convenient places, added a belfry, and made a few holes in the ceiling (if there were a ceiling) for "ventilation." If he remembered inlet-ventilation, he specified something like "Tobin's tubes, price not exceeding five shillings each." This may perhaps appear to be a flippant and even malicious description of the methods pursued; but, if I am dealing unjustly with the school designer of former days, he has managed to effectually conceal all evidence in his own favor.

Time does not permit of the detailed consideration of principles of school-construction, but certain general matters in conjunction therewith may be mentioned. Before doing so I desire to disclaim any intention of formulating a cut-and-dried system suitable to all States of the Commonwealth and all parts of all States. Like the children they are intended to accommodate, schools require to be fitted to their environment with a due sense of proportion; and what will suit Hobart, for instance, will obviously not meet the requirements of Normanton, any more than it will necessarily suit the Tasmanian west coast. An "Australian Type" of school building is impossible; and, in fact, "type plans" are open to grievous misuse in any single State unless carefully and intelligently used. Certain architectural principles must be embodied in all, for reasons of economy and of administration; but although the possession of a neat set of plans, all fitting together and interchangeable, like the works of a Waterbury watch, may appeal to some, it will be found in practice that when school type A is converted into school type B, by rule of thumb, the result is seldom satisfactory. It is equally cheap and far more satisfactory wherever the facilities of staffing will permit, and particularly in large schools, to adopt the system of school

units, whereby, in place of outlining a block and cutting it up into rooms, the block is built up from the rooms to whatever size may be required, with adequate provision for subsequent extension on similarly correct lines. By regarding the teaching unit as the principal objective to be considered in planning and placing the school, and by adopting definite structural requirements for each such teaching unit, the problem of hygienic school construction is much simplified. The adjuncts can be fitted in afterwards ; and once this principle is grasped we will no longer find teaching rooms faced to the south, with a blank wall to the northern sky, and an assembly hall fronting east.

The accommodation in any one teaching room of more than forty children is undesirable : for reasons of lighting the rooms should not be over 24 feet in width, and cubic space must not be measured over 13 feet vertical. The existing *minimal* standards of floor space and cubic space are undoubtedly too low, but they have been apt in the past to be regarded as *maxima*. Any notable increase involves large expenditure, however, and as schemes of mechanical ventilation are out of the question—for the majority of Tasmanian schools, at least—ideal conditions of air supply are unattainable in cold weather. The provision of adequate and properly placed ventilating and perflating openings, of effective pattern—*not* builders' " ventilators," but real ones that will, under ordinary conditions, admit or extract a definite amount of air, and can be cleaned—and their intelligent use in conjunction with the windows, by teachers, together with hourly perflation at recess or change of classes, will tend to keep the air sewage within reasonable limits. All ventilating provision should be primarily intended to serve the " breathing line." The requirements for windows, both as regards amount and direction of entering light, and as adjuncts to ventilation, deserve careful study. I am in the habit of advising for Tasmanian teaching units a *minimal* glazed area of one-fifth of the floor space, with bevelling of piers, and the use of practicable windows—preferably of the well-known " Magic " pattern—surmounted where necessary by hopper top-lights with side-checks, and reaching to within 6 inches of the ceiling. The seating-line must be planned with the windows as a definite portion of the lighting scheme, and rigidly adhered to. School lighting has a literature of its own, however, and it is unnecessary to go further into it.

Interiors must be so designed as to avoid the accumulation of dust and dirt, to facilitate its early detection and removal, to assist in the diffusion of light, to relieve eye-strain, and to necessitate as little attention and repair as possible. All these requirements may be easily and cheaply met. School fitments, and particularly desks and seats, form a subject quite capable of occupying the whole of this paper ; but, beyond bestowing a whole-hearted condemnation upon that survival from educational dark ages—the long moveable form and desk—this can receive but passing mention. Backs to school seats are essential, to avoid the pernicious effects of a " forward sitting " position, and even where accurate difference cannot be obtained, proper distance should be secured by fixing seat and desk in correct relative position. To my mind the most practical solution of this very serious question is to be found in the " Sheffield " method advocated by Dr. Kerr, wherein separate backed seats, each supported on an iron standard, are used at a long desk similarly supported. This takes up no more space per head than do dual desks ; is very easy to clean ; is cheap, strong, and simple ; and can, by a simple mechanical device, be rendered adjustable in difference for a proportion at least of the scholars in each room. Heating apparatus is of importance, and although the more expensive pattern of school-ventilating stove is often impracticable, a very fair substitute is obtainable at small cost by using an ordinary circular stove surrounded by a metal cylinder to the bottom of which air is admitted by a trunk under the floor. The heat of the flue can be employed to

pull used air through galvanized iron exhausts opening near the floor at the opposite end of the room. Undue drying of the entering air can be avoided by a pan of water. The saving of fuel, as compared to the wasteful open fire-place, will amply repay the cost of such a device.

Certain adjuncts to the teaching unit, or group of units, require consideration, both in the main building and outside it; as do the arrangements for rotation of classes in respect of the degree of lighting, &c., rendered necessary by their work. All schoolrooms used for teaching cannot, in a large school, face to the north-east; but, as far as possible, only those teaching units which do command the best light should be employed for fine work involving eye strain. The placing of the building on the site should be such as to secure main-lighting from the north-east, and I have no hesitation in stating that this should completely override all considerations of alignment with the street or other relatively subordinate objections. The school is erected for the benefit of the children, and as a necessary adjunct to that education which the State has ordered. No æsthetic considerations will justify any unnecessary risk to their eyesight and health, and at the worst the lack of correct alignment will but form a standing testimony to the care of the State for its most valuable assets.

Accommodation for outer garments and lavatory provision can be secured at little outlay. Latrine provision is of great importance, but need not be here dealt with in detail, beyond a passing reference to the excellent results which have been obtained from the use of sawdust or dry earth urinals, slightly modified from the pattern advocated by the late Dr. Poore.

For obvious reasons the inside of a school requires far more attention than does the outside, and all unnecessary and expensive external architectural vanities should be ruthlessly swept away. In the case of many large schools the money which has been wasted on bell-towers, carved stone porches, and other meretricious adornments of no practical use, would have enabled the interior to be fitted with modern hygienic facilities. There is no need to aim at the severe simplicity of a gaol, but rather, within reasonable limits, to adopt the utilitarian but yet æsthetic principles of modern hospital construction.

By a useful inter-departmental arrangement all plans of new State schools, and additions to existing school buildings, in Tasmania, are now submitted for inspection to my Department, and are not passed until approved by the Director of Education and myself.

The hygienic training of teachers requires to be carried out on practical—even clinical—lines. Reading is, of course, necessary; but in any set course of this nature, practical and clinical demonstrations, plentifully illustrated by lantern slides taken in the locality, and not imported from other places where conditions differ, should form a large part of the training. Women teachers should be practically instructed in the care of infants, and in the principles of clothing, washing, and feeding them. The true position of artificial feeding, as a last resort, and *not* as an admissible alternative to breast-feeding, should be insisted upon. Domestic economy and civics should be freely interwoven with any such teaching, and the whole made to form a living subject, and not a mere collection of book aphorisms and half-digested formulæ. Comparatively little physiology is necessary, and what is employed should be carefully subordinated to its practical application. The use of Snellens' types, together with their range of application in lay hands, the methods of conducting head-and-hand-drills, of detecting adenoids, eye defects, and other physical obstructions to mental re-action, should form a part of the training, freely illustrated by clinical examples and slides. Practical elementary hygiene, including methods of dealing with organic refuse, of ventilating, warming, and lighting—and particularly methods of managing and improvising facilities of

this character—should be explained and demonstrated. The signs of fatigue, and the conclusions to be drawn from such signs, together with measures for remedying the same, and the principles of correct working attitude are further subjects which may well receive attention. These form but a part of any full course of hygienic training; but, for teachers possessing live knowledge of this character, the systematic observation of pupils for physical unfitness, hygienic school management, and the organisation of practical instruction in the ways of healthy living should present little difficulty.

A definite hygienic purpose can be made to enter into, and influence, every day of the child's school life. In the infant classes habits of correct working attitude and of proper breathing may be fixed for ever by a skilful patient teacher. The spitting habit should be here vigorously attacked. I trust that I may be excused for referring to a remarkable fact stated at the 1905 Teachers' Conference in London, namely, that the work of the Kindergarten teacher and the nursery governess far outweighs in educational importance the most brilliant displays of professorial erudition. This individual dominates the whole educational system; and unless her work is skilfully and conscientiously performed no subsequent teaching can remedy her errors; but, on the other hand, no subsequent mismanagement can entirely negative the beneficial results of skilful infant teaching. It is the age of habit-formation with which she deals, and her fingers mould the nation. No books, no "set talks," no detailed explanations, are of any use in her work. The child is strenuously learning life, and the results of that learning are expressed in fixed habit. He educates himself from anything that is available, and the infant teacher's business is to render available the right things in the right way, and to ensure that they are rightly used. It is the most skilful of all teaching, and the most momentous in its results.

In the middle school a little set instruction, and much practical demonstration and application, will carry on the teaching. The selection at monthly or fortnightly intervals of a sanitary officer for each class, and the deputing to that important functionary of direct responsibility for the ventilation, regular perflation, and general hygienic management of the room, would, in the hands of a judicious and skilful teacher, awaken much interest in such matters amongst the children. To this should be added practical demonstrations of cleansing methods, and of other principles of domestic hygiene, strict adherence to a high standard of personal cleanliness as checked by daily head-and-hand-drills, and occasional talks on elementary principles. Books here require to be very simple, but artfully constructed withal, and absolutely free from any trace of that enemy of our youth, Mr. Barlow.

In the upper classes the appointment of a sanitary officer should be continued, but such appointment should be made by ballot, as forming a peg on which to hang much useful information anent civic and municipal government and methods. Some reasoning may be expected from children in this division, and principles may be explained. Elementary physiology is useful here if employed for the purposes of explanation and illustration. Instruction in elementary civics and domestic economy should be interwoven, and the physical side of the environment kept clearly in view. Communicable diseases, their results to the individual and the community, and measures for their limitation and extinction by domestic or civic isolation and disinfection, may be explained in some detail, together with the common-sense rationale of health protection and preservation in the individual and in the community. In the larger centres regular demonstrations of proper methods of clothing and caring for infants may, with advantage, be given to the older girls by a trained nurse or other skilled person; artificial feeding being explained, and illustrated, whilst emphasizing its true position as an

undesirable alternative. By the exercise of a little tact, it would generally be possible to borrow a live subject for purposes of demonstration. The attendance of mothers, and particularly of young mothers, should be encouraged.

In large schools a sanitary monitor of each sex should have the general supervision of all school sanitary matters outside the teaching rooms, reporting daily to the head teacher, or principal female assistant respectively. These reports should, after investigation, where necessary, be recorded for examination by the inspector.

All this may perhaps sound rather formidable, but there is good reason to believe that it and a good deal more might—and, as I have no reason to doubt, eventually will—be made to form a part of the educational administration of at least one Australian State, without much risk of hygienic overfeeding. In this I am supported by the opinion of educationists of wide experience, and by the keen interest and appreciation with which Tasmanian teachers have welcomed my own efforts in this direction. Time does not permit me to deal with the practical details of medical inspection, but it will be only through the co-operation and intelligent assistance of teachers that such procedure can be organised or conducted on economic or effective lines. That this co-operation and assistance will be forthcoming I have good reason to believe, so far at least as my own State is concerned. All measures of medical inspection and of hygienic school management, all measures in fact of School Hygiene, have for their object the enhancement of school efficiency. The leaders of medical and educational thought in the older world recognise this; and I am of opinion that the time has come when educationists and medical men in Australia should work manfully together for the physical and mental advancement of our race. Such co-operation is not confined to professional hygienists and to educational administrators: every practising medical man, and every teacher, has a definite interest in the question, and can afford useful assistance in the cause of common-sense hygienic education, and of common-sense school management.

I am painfully aware that, in setting forth to address this learned audience upon so wide a subject as that of School Hygiene, I have been unable to do more than touch upon a few of its more important aspects. The treatment has of necessity been sketchy, the chain of reasoning as presented lacks many of its links, and the suggestions for rectification of existing conditions have not been worked out to meet particular cases. Nevertheless, I present this paper with all its imperfections, in the hope that it may perchance serve to elicit the opinions of others who may have worked at or given thought to the subject, and to attract, in some degree, the attention of our profession as a whole to what is without doubt a matter of great national importance to Australia.

COMPULSORY NOTIFICATION OF PHTHISIS.

By T. Borthwick, M.D., Edin.

I propose to open the discussion on this subject by simply referring to the sections of the South Australian Health Act which deal with this disease, and to the administration of these sections as far as Adelaide is concerned, and the experience gained thereby.

Phthisis is termed Pulmonary Tuberculosis in the Health Act, the reason being partly to define the disease with greater accuracy and partly to avoid a term which conveyed a rather deadly significance to the public. It was felt

that it would be more easy to convince the public of the curability of the disease under the one term than under the other at what was an early period in the campaign against consumption.

Phthisis is not included under the term "infectious diseases," as defined by the Health Act. If it had been so included, patients would have been subject to the restrictive clauses applying to these diseases. This would have caused much hardship in many cases, and, in a new departure which might prove to be premature, it was incumbent to disarm opposition as far as possible.

Section 128 renders it compulsory on medical practitioners to report a case as soon as the fact becomes known to them. The head of the family, or nearest relative, is not required to report. Section 131 gives power to local boards to carry out disinfection, and section 132 gives power to remove a patient from a building used for the storage of milk, or the storage or manufacture of any article of human food. There is no other restriction imposed on the movement of persons suffering from phthisis.

It will be seen that the framers of the Health Act took into consideration the susceptibilities of patients, and also tried to obtain the confidence of the public. But there was no discretionary power given to the medical practitioner in the matter of reporting cases, and many of these looked on with passive disapprobation, fearing that the interests of their patients might suffer. In order to disarm this opposition, it was decided by the city and some other local boards to take no action in cases where the medical attendant, in notifying, undertook to see that all necessary precautions were carried out, and to report to the board any change of address or the death of the patient.

The number of notifications of cases, and of deaths, from phthisis, in Adelaide, since the Health Act came into operation are as follows :—

Year ending September 30th—	Number of Deaths.	Number of Cases.		
1899-1900	70	72	188	94
1900-1901	67	116		94
1901-1902	64	79		
1902-1903	67	82		
1903-1904	63	89		

Cases coming into the city for treatment are not included, but it is impossible to make any other correction in regard to the numbers. I do not think any conclusions can be drawn from this table. What decline there is seems to be in the number of deaths rather than of cases, and it is possible that the success of the campaign against consumption will show itself first in this direction. The disparity in the number of cases in the first two years is probably accounted for by the hesitation of medical practitioners to notify cases during the first year, and the complete abandonment of that hesitation afterwards. By striking an average for these two years, we may arrive at the approximate number for each. So completely has the administration of the Health Act in the city satisfied the medical profession that it may be safely assumed that every case is now notified, and very rarely do medical practitioners request us not to take any action.

The routine action in Adelaide is practically the same as in a case of infectious disease. When a case is notified, the trained nurse attached to the staff of the Health Department visits the house and interviews, in the first place, some relative of the patient. She advises as to the precautions to be taken by the patient and attendants, and also as to the use of disinfectants, leaving printed instructions for further guidance.

Disinfectants are supplied to the poor free of charge, and sputum is also examined gratis. I personally rarely visit cases (only for some special reason), partly because the nurse gets all the information I require, and partly because I think it advisable to come between the patient and the medical attendant as little as possible. The nurse keeps in touch with the patient during the course of the illness. At its termination, by recovery or by death, the contents of the room or rooms are disinfected under her supervision, and the rooms themselves are afterwards disinfected by a sanitary inspector without any charge being made. Insanitary conditions of the house are also attended to by him. It is found that the poorer patients frequently change their place of residence, and all houses so vacated are disinfected before they are again occupied. In order to minimise leakage, the city local board decided to pay to medical practitioners, in addition to the ordinary fee for notification of cases, a fee for notification of change of address or of the death of patients. When it is found that the patient has moved into another district the local board of the district is informed of the fact; and a few boards adjoining the city reciprocate this action. Experience has shown that it is especially in relation to phthisis that a woman has the advantage over a male inspector. At first she was looked upon with suspicion by patients and relatives, but, with the exercise of tact, this was soon overcome, and all classes now welcome her as a valuable ally. She is cheerfully admitted into the sick-room, and, by establishing friendly and confidential relations, succeeds in persuading the most obdurate patients to carry out instructions. The educational effect of the nurse's work on the patient's family and on the neighbors must not be overlooked. In addition to the appreciation of our methods by patients and relatives, there is a more general recognition in the shape of requests to disinfect offices where a person suffering from phthisis has been employed, and where the fact has been unknown to the health department owing to the patient having lived outside of the city. It is coming to be recognised that such patients need not be shunned in the event of proper precautions being adopted, and I do not know of any case where the material prospects of a patient have been injured as a result of notification of phthisis.

Notification of phthisis, to be of practical value, must be followed by other measures—disinfection, facilities for bacteriological examination of sputum, and the provision of sanatoria for isolation and treatment. I shall content myself with saying that the latter institutions have been fairly provided in this State; but experience has shown that there is a great difficulty in getting early cases—especially when the patients are bread-winners—to enter sanatoria.

To meet this difficulty, the establishment of a fund to assist the family to live during the absence of the bread-winner is a necessity.

A REVIEW OF SOME RECENT WORK ON THE ETIOLOGY OF DYSENTERY.

By Frank Tidswell, M.B., Ch.M., Syd., D.P.H., Camb., and James Froude Flashman, B.A., B.Sc., M.D., Ch.M., Syd.

I. Introduction.

Although the term "Dysentery" has been in use from a remote period, it does not seem possible at the present day to express its meaning with definite precision. Clinically, it is associated with the passage of blood and mucus, per anum, attended with pain; and, in this sense, embraces "several

varieties of intestinal flux" (1). It is recognised that such a condition may result from the operation of mechanical and toxic agencies, or be a part or merely an event in the course of well-defined maladies (heart affections, Bright's disease, tuberculosis, pneumonia, &c.). But, apart from this symptomatic significance, the word is used also to denote a particular morbid entity—a specific disease—which finds a place in systematic nomenclature under the designation "Dysentery." It is with this latter connotation of the term that we propose to concern ourselves in this place.

To the epidemiologist dysentery formerly signified a disease of endemic prevalence in certain tropical and sub-tropical lands, but which appeared now and then under the guise of epidemics in more temperate regions; as such it was regarded as a specific disease. Modern views, however, have come to look upon this conception as of questionable validity. "We cannot say for certain," states Sir Patrick Manson (2), "whether there is but one disease having grades of severity, or a dozen specifically distinct diseases included under the term dysentery." B. Scheube expresses himself to the same effect. "It is a question that is still waiting to be solved," he says (3), "whether dysentery is a specific disease, or, as is largely assumed, whether it is attributable to a variety of causes; nor has it been settled whether tropical dysentery is etiologically identical with the dysentery of temperate climates."

It is to be observed also that clinicians nowadays are equally sceptical with respect to this matter. "In individual cases," writes Andrew Davidson (4), "dysentery often arises out of what seemed to be an attack of simple diarrhœa, so that no hard and fast line can be drawn between diarrhœa and dysentery." Hale White refers (5) to the possibility of confusing dysentery with ulcerative colitis, and even with simple colitis; and Pye Smith remarks (6) "whether cases of ulcerative colitis, with passage of blood and mucus . . . . . should be called dysentery is, from a clinical point of view, little more than a question of terms." L. Emmett Holt prefers to drop the term dysentery as applied to these conditions in children, and to group all such cases under the general head of ileo-colitis, which he considers is "to be regarded as a condition into which any case of gastro-enteric intoxication (gastro-enteritis) may develop" (7). Mott and Durham believe asylum colitis to be identical with the condition which, when occurring elsewhere, is called dysentery. "The disease we have had to deal with," they report (8), "agrees in its anatomical signs with the dysenteries described by observers in this and other cooler climates as well as those of warm regions: the range of variations in the clinical and anatomical conditions is the same in all, and we see no reason why they should not be grouped under the one title—dysentery."

It would thus appear that the idea of dysentery as a specific disease is far from being accurately delineated. One gathers from such statements as those cited that conditions referred to as colitis, whether sporadic or occurring as institutional outbreaks, as well as some forms of diarrhœa—including infantile diarrhœa—may perhaps be identical with the endemic or epidemic malady or maladies actually called dysentery. Yet, in spite of this inclusiveness, there is pretty generally displayed an undercurrent of thought to the effect that there is somewhere a line of cleavage separating a true specific dysentery from resemblant conditions. The suggestion is, that although present sub-divisions may need to be abandoned, there will come a time when a re-arrangement may be made on a satisfactory basis. It is in accordance with the spirit of the times that such a basis should be sought for in the realm of etiology.

The ordinary etiological circumstances mentioned in connection with dysentery—water-supply, food, exposure, chill, &c.—are not peculiar to it.

They are to be regarded either as predisposing conditions, or as vehicles for the conveyance of the real causal agent: this latter being presumably of a microbic nature. In view of this hypothesis, numerous investigators have engaged in microbiological researches with the object of determining whether any particular micro-organisms are associated with the disease. The issue as it stands at present incriminates two kinds of parasites—amœbæ and bacteria—and upon both an abundance of attention has been bestowed. So much is this the case, so numerous are the reports and so conflicting in their advocacy of different views, that a clear conception of the actual state of affairs is by no means easy to attain. We have been obliged to attempt this task recently as a matter of official duty, and we have put together in this paper the information we were able to gather, in the hope that it might prove of service to others who have not the time or opportunity to collect it for themselves.

II. Amœbic Dysentery.

The existence of living protozoa (cercomonas, paramœcium) in the bowel excretions of human beings was demonstrated about half a century ago, and it is stated the occurrence of amœbæ therein was first recorded by Lambl in 1860. In his case the amœbæ were found in the stools of a child suffering from diarrhœa. In 1870 Cunningham reported their presence in the evacuations of cholera patients. In neither of these cases was any pathological significance attached to the amœbæ. But in 1875, Losch, as the result of a careful investigation of a case of chronic dysentery at St. Petersburg, suggested that the amœbæ found in the stools might have a causal relationship to the disease. Since that time this aspect of the question has been the subject of numerous investigations (9).

The occurrence of amœbæ in the stools of dysenteric patients is no longer a matter of dispute. They have been found in Egypt (Kartulis, Koch, Kruse, and Pasquale, and others); Russia (Losch, Massiutin); Germany (Nasse, Quincke & Roos, and others); Austria (Hlava, Kovacs, Epstein); Greece, (Kartulis); Italy and Sardinia (Calandruccia and others); the United States of America (Osler, Simon, Councilman & Laffeur, Howard & Hoover, Thayer, Stengel, and others); in Brazil (Lutz); in the Philippine Islands (Flexner, Musgrave, and Clegg); Australia (Isbister) (10), &c., &c. The instances given suffice to display a very wide-spread occurrence of the amœbæ in association with dysentery cases.

But whilst it is admitted that amœbæ do occur as stated, it is pointed out that they occur also in non-dysenteric conditions, *e.g.*, in cholera stools (Cunningham), in typhoid stools (Massiutin, Casagrandi & Barbagallo-Rasipardi), and in the stools of healthy persons (Cunningham, Schuberg, Kruse, and Pasquale, Gasser, and others). In view of the abundant occurrence of amœbæ in the surroundings of places where dysentery is common (Egypt, Philippine Islands), the presence of the amœbæ may be merely accidental.

It is further urged, however, that the amœbæ are not merely loose in the bowel, but penetrate the tissue, being found at the base and in the vicinity of dysenteric ulcers, and in the pus of liver abscesses associated with dysentery. This also is admitted, but the significance to be attached to their occupancy of these positions is a matter of dispute.

As regards the intestine, it has been suggested that the ulcerative lesions are caused by other agencies (*e.g.*, bacteria), and only secondarily invaded by the amœbæ, which themselves are not able to cause ulceration (Casagrandi and Barbagallo-Rasipardi, Janowski, and others). But it would appear that the amœbæ do really possess the power of setting up lesions of the intestine

(*vide infra*), and, further, that although digestion in amœbæ is usually regarded as occurring in vacuoles, some of them at least can exert a solvent action on external materials.

Again it is stated that the liver pus does not always contain amœbæ, and very frequently does contain bacteria, *e.g.*, bacillus coli communis, streptococci. Some authors argue that the abscess is due to these bacteria (streptococci, Zancarol). On the other hand it is urged that the pus in some cases contains only amœbæ, and is bacteriologically sterile ; and further that the peculiar character of the pus (few leucocytes, much debris and blood), and the absence of a definite abscess wall, is more consonant with its production by amœbæ than by bacteria.

The advocates of the pathogenicity of the amœbæ point to the production of dysentery by experiments on the lower animals. It appears that the usual laboratory test-animals—rabbits, guinea pigs, &c.—are insusceptible, and most of the experimental work has been done on dogs and cats. According to Kartulis, dogs naturally affected with dysentery in Egypt have amœbæ in their stools, and dogs have been several times experimentally infected by rectal injection of amœbæ-containing stools (Losch, Hlava, and others). Cats appear to be even more susceptible ; at all events they have been more commonly used in experimental work. Successful results by the rectal injection of dysenteric stools or liver pus with these animals have been reported by many observers (Kartulis, Quincke & Roos, Kruse & Pasquale, Hlava, Vivaldi, and others). With respect to reported failures it is recognised that attempts are not always successful; the results may differ even with the same material.

Concerning this infection of cats it is urged that the intestine of these animals is very easily disturbed, and that a condition resembling dysentery can be produced by merely mechanical means, *e.g.*, injection of sand. Some authors assert that when dysentery follows the injection of the stools there are no amœbæ in the cat's evacuations (*e.g.*, Celli & Fiocca), but others report the presence of amœbæ therein (*e.g.*, Kruse & Pasquale). It is stated further that dysentery in cats is producible by the injection of stools in which the amœbæ have been killed by heating (Gasser, Celli & Fiocca), but this is denied (Kartulis) ; and also by the injection of amœbæ-free pus from a liver abscess (Zancarol). It is obvious, of course, that this experimental method is not free from the objection that the condition so produced may be due to toxic substances, or to bacteria ; although, as regards the latter, it is stated that the bacteria associated with the amœbæ do not produce dysentery (Kruse & Pasquale, Harris). This objection is in course of removal by observations made, not with stools or pus, but with amœbæ isolated therefrom by cultures in hay infusion (Kartulis), and on solid media (Ogata, Schardinger, Muller, Celli & Fiocca, Schaudinn, Musgrave, and Clegg). Pure cultures have rarely been obtained ; as a rule the amœbæ only grow when there are also present bacteria upon which they may feed. But it is possible to secure growths containing the amœbæ and only one kind of (non-pathogenic) bacterium ; what has been rather paradoxically, called a "pure-mixed" growth. By the use of such cultures results have been obtained which indicate that there are in the intestine both harmful and harmless amœbæ, *i.e.*, the rectal injection of one kind produces dysentery, that of the other does not (Kruse & Pasquale). From his investigations in this direction, Schaudinn has come to recognise at least two species of amœbæ habitually parasitic in the human intestine (11). One of these—amœbæ coli of Losch, re-christened entamœba coli by Schaudinn—is harmless; the other—entamœba histolytica of Schaudinn—is pathogenic. The distinction between the two is made on the basis of their morphology and method of reproduction.

Schaudinn believes that dysentery is produced by ingestion of the "spores" of entomœbæ histolytica, not by the naked (amœbæ) forms. The administration of these with food produced dysentery in cats. Quinck and Roos had previously reported that dysentery could be produced if encysted forms were given by mouth. The objection raised by Calandruccio, that he failed to produce dysentery in himself by swallowing amœbæ, including encysted forms—although amœbæ appeared in his stools—may perhaps be based upon the use of a harmless species. No doubt much of the previous work was vitiated in the same way. The observations of Musgrave and Clegg (12) in the Philippine Islands generally confirm Schaudinn's results. These investigators produced the intestinal lesions of dysentery in monkeys, by the administration of "pure-mixed" cultures by mouth, and hepatic abscess by injection of such cultures directly into the liver. They report that the feeding of dogs and cats was generally negative; but that a human being, who took the amœbæ in gelatine capsules, acquired dysentery. They express the view, however, that all amœbæ found in the intestine are, or can become, pathogenic; and even those found outside the body, *e.g.*, in water, straw infusion, &c., can be rendered harmful by passages through the liver of animals. The validity of this contention remains for future determination.

In the meantime it is reasonable to conclude from these various results that amœbæ, or at all events a particular species of them, can produce dysentery. But it is not believed, nor indeed is it contended, that dysentery is exclusively caused by amœbæ; the particular form of malady for which they are held responsible is that commonly referred to as "tropical dysentery"; but this term does not now imply that all the dysentery of the tropics is caused by amœbæ, nor that amœbic dysentery is restricted to the torrid zone. It is considered to be more prevalent in warm climates, and to have endemic centres in Egypt and in Greece; but it has been observed also in North America, and in more northerly parts of Europe (Russia, Germany, Austria, &c., *vide supra*). This form of dysentery is characterised clinically by an irregular course and a tendency to become chronic, and, pathologically, by the formation of undermined intestinal ulcers and a liability to hepatic abscess. But, in addition to this ulcerative form, there are described "catarrhal" "diphtheritic," and "gangrenous" varieties of dysentery in which amœbæ are not found. These three conditions, it is suggested, may be merely grades of one and the same disease: a disease which, whilst "endemic" or "sporadic" in some localities, is apt to assume at times epidemic prevalence even in territory which has previously seemed to be free from it; and which, in individual cases, usually runs a more or less acute course. It is to this malady, if to any, that the idea of dysentery in the sense of a specific infectious disease applies. Hence the observations on amœbic dysentery, that we have just considered, bring us nearer to the object of this paper only in so far as they enable us to separate off from a group of maladies labelled "dysentery" a particular class of cases dependent upon the agency of amœbæ (amœbic enteritis); just as, for other reasons, there have already been separated classes of cases dependent upon toxic or mechanical influences, or which are incidents of other diseases.

III. Bacterial Dysentery.

The features of the non-amœbic dysentery just referred to are sufficiently analogous to those of other diseases known to be caused by bacteria as to suggest the operation of these agents also in its case. For some time, however, the searches made did not result in the detection of bacteria other than those which habitually occupy the intestine. For this reason many observers were induced to conclude that dysentery did not depend upon the activity of

any special micro-organism; other observers, however, having found a particular species predominating or permeating into the tissue, or possessed of particular virulence, have attached to it the onus of producing the disease. Of such species the micro-coccus of Durham, the streptococci of Zancarol and Petridis, the diplococcus of Silvestri, the streptothrix of Pottein, bacillus enteritidis sporogenes (Klein), bacillus pyocyaneus (Maggiora, Calmette, Kruse, and Pasquale), the bacillus of Ogata, and various unnamed bacteria (Orth, Ziegler, Krebs, Laveran, and others), need no more than mention in this place, since subsequent investigations have not endowed them with any special importance.

More interest attaches to the belief expressed by some that the disease is dependent upon nothing else than bacillus coli communis (Viellon & Jayle, Maggiora, Arnaud, Pottein, Alessandri, Escherich, Valugussa, Campbell, Galli-Valerio, and others). It is significant, however, that very often the species isolated is said to possess a much higher degree of virulence than ordinary bacillus coli communis, and quite commonly referred to as a variety of that species, *e.g.*, bacillus coli dysenteriæ of Celli. Chantemesse and Widal also isolated a particular bacillus of this kind. But the indications in this direction were not very definite until, in 1898, K. Shiga described as isolated by him from cases of dysentery in Japan a bacillus which, though resembling bacillus coli communis, was nevertheless distinct from that species. Whether or not any of the previously isolated coli "varieties" are identical with Shiga's bacillus, as some authors maintain, is a matter which it would be profitless to discuss in this place. We may therefore pass to the consideration of the evidence brought forward by Shiga in favor of his contention that the bacillus isolated by him is etiologically related to dysentery.

In a preliminary communication (13) Shiga reported the results of his bacteriological observations upon thirty-six cases of epidemic dysentery, and mentions the regular detection of a bacillus having certain specified characters. In particular the bacillus was agglutinated by the blood serum of persons sick with dysentery, and was not agglutinated by that of persons suffering from other diseases, nor by that of healthy persons. Other bacteria isolated were not agglutinated by the serum of dysentery cases. In a further paper (14), Shiga gives details concerning the morphological, cultural, and pathogenic characters of the bacillus, its toxins, the production of specific agglutinins by injection of killed cultures, &c. In a still later and longer paper (15) he reviews and extends his previous work; and, amongst other things, explicitly states the following as the points of evidence upon which his bacillus is to be regarded as the specific cause of dysentery:—

1. The bacillus is forthcoming in all cases of dysentery.
2. It is exclusively related to dysentery: is not found in other diseases nor in healthy persons.
3. In dysentery its occurrence keeps pace with the progress of the disease.
4. It occurs abundantly, or in the pure state, in the deeper layers of the intestinal mucous membrane: on the surface, and in ulcers, it is sparser and outnumbered by other bacteria.
5. The dysentery bacillus and its toxins cause hæmorrhages. In experimental animals there are developed hæmorrhages under the skin, serous, and mucous membranes, just as occurs in severe cases, as seen *post mortem*, in man.
6. The bacillus is agglutinated exclusively by the blood serum of persons suffering from dysentery: not by that of persons suffering from other diseases, nor by that of healthy persons.

7. The agglutinating power of the blood of dysentery patients is related to the stage of the disease. It describes a definite curve, which rises quickly at first, reaches its highest point at convalescence, and after that gradually declines.
8. The sub-cutaneous injection of killed cultures of the bacillus causes a marked inflammatory reaction in healthy persons, but no such reaction is produced by similar injection into dysentery convalescents.
9. The Pfeifer reaction is specially evident in convalescent cases.
10. The serum produced by immunisation with the bacillus has preventive and curative efficacy against dysentery.

The details with respect to these various points are such as to justify Shiga's contention that his bacillus is the specific cause of acute epidemic dysentery in Japan; and, indeed, the validity of this conclusion is no longer disputed.

Very soon it began to be reported that dysentery elsewhere than in Japan was caused by the same, or a very similar, bacillus. Simon Flexner (16), as the result of observations on dysentery in the Philippine Islands, reported that there also acute epidemic dysentery was of bacillary origin; and that the bacillus isolated by him was identical with that described by Shiga. R. P. Strong, Curry, Bowman, and others support Flexner's conclusion, as also does F. C. Craig, who, at the Presidio, San Francisco, examined soldiers invalided from the Philippines. Flexner further reports the occurrence of the bacillus in a case of dysentery from Porto Rico, West Indies; from cases occurring at Philadelphia, and at Morvin, N.C. He concludes that acute epidemic dysentery in the Philippines, in the West Indies, and in America is also due to Shiga's bacillus. In the meantime W. Kruse (17) had isolated from cases of dysentery at Laar, Germany, a very similar micro-organism. At first he regarded this as distinct, on the grounds of minor differences from the descriptions of Shiga and Flexner. But a later opportunity of making comparative observations with two specimens forwarded to him by Flexner led him to conclude that his own and one of Flexner's strain were identical; whilst the other Flexner strain (distinguished by Kruse as " Amerika ") was distinct from these two. The distinctions were ably based upon agglutination re-actions with sera prepared for each species separately. Flexner (18), as the result of comparative observations, concluded that his own, Shiga's, and Kruse's bacilli were identical; and Shiga (19), who covered the same ground, agrees with Flexner. A commission, consisting of E. Pfuhl, Schmiedecke, Schuder, and Lentz, pronounced in favor of the identity of the bacilli of Shiga, Flexner, and Kruse, as well as of bacilli obtained from an epidemic of dysentery at Doberitz (20). Numerous other investigators, *e.g.*, Vedder and Duval (21) (Philadelphia, &c.), Park Collins and Goodwin (22) (New York), Park and Carey (23) (Tuckahoe, N.Y.), Jurgens (24) (West Germany), Rautenberg (25) (East Prussia), Raczynsky (26) (Galicia), P. T. Muller (27) (Sudstiermark), Spronck (28) (South Holland and Gelderland), Rosenthal (29) (Moscow), Vaillard and Dopter (30) (Vincennes), Leonard Rogers (31) (India), Aldo Castellani (32) (Ceylon), Morgenroth (33) (China), and Foulerton (34) (cases from South Africa, Florida, China, London, and Hertfordshire) report the isolation of bacilli which they identify with one or other of the above-mentioned strains.

As the outcome of these different observations it appeared that the bacillus isolated by Shiga in Japan was also forthcoming from dysentery cases in various other parts of the world; the extensive area covered indicated that this bacillus was to be regarded as the specific cause of acute epidemic dysentery, wherever occurring. According to some observers, it is the cause not only of acute epidemic dysentery, but also of asylum dysentery (Vedder and

Duval (35), Eyre (36)), and of the summer diarrhœa of infants (Duval and Bassett (37), Litchfield and Hipsley (38)). This conclusion, however, was not universally accepted. It will have been noted that Kruse had separated one of Flexner's strains (" Amerika ") as distinct from his own and the other Flexner strain. It may be now further mentioned that Kruse, at the same time, drew a sharp distinction between the bacillus he had isolated from acute epidemic dysentery and certain resemblant bacilli he had isolated from asylum cases. By means of sera made separately for each, Kruse showed that three strains of asylum bacilli differed in their agglutination re-actions from the "true dysentery bacilli" (his own and Flexner's), and also from one another and from Flexner's " Amerika," although the latter had affinities with one of the asylum strains. To these asylum bacilli Kruse applies the name of pseudo-dysentery bacilli. He holds them responsible for the asylum dysenteries, which thus, in his view, are different from epidemic dysentery and also from one another, although exhibiting the same clinical and pathological features. Certain other observers also note variations from the strict Shiga or Kruse type, which lead them to refer to the bacilli isolated by them as " pseudo-dysentery " or " dysentery-like " bacilli (Lentz, Schmiedecke, Pfuhl) (39). As regards infantile diarrhœa, Martha Wollstein (40) states that the bacilli isolated by her from thirty-nine cases of summer diarrhœa agglutinated with serum prepared with the Flexner (Manila) bacillus, but not with that prepared with the Shiga bacillus ; and P. H. Hiss, jun., and F. F. Russell (41) report having isolated from a fatal case of infantile diarrhœa a bacillus which, though resemblant, was not quite identical with Shiga's bacillus. Further, Martini and Lentz, in a paper to be considered directly, take exception to the method of comparison—testing against the blood serum of patients—practised by most other observers. They quote instances which show that bacilli, otherwise alike, behave differently towards patients' serum, and bacilli otherwise unlike behave similarly towards such serum. Moreover, some of the strains of bacilli are agglutinated by normal human or animal sera, by sera of patients suffering from other forms of inflammation of the bowel (tuberculosis), and by sera of animals immunised against cholera and typhoid. It thus became apparent that the matter was not quite so simple as had been supposed : the notes of discord suggested the necessity for revision of the whole subject.

Towards this revision an important communication was made by Martini and Lentz (42). As already stated, these observers reject the test with patients' blood as being unreliable. For the purpose of differentiation they prefer to use the blood serum of a goat, specifically immunised against Shiga's bacillus, *i.e.*, against this type alone. The serum in question was such as to agglutinate the Shiga type bacillus in a dilution of 1 in 500. Martini and Lentz made use of this serum for the purpose of comparatively testing various strains of bacilli isolated from dysentery cases by various observers at various places. As the result of their investigation they report that, whilst some of the strains behaved as did the Shiga bacillus, there were others which failed to do so. They accordingly divide the bacilli tested by them into two groups : Group 1 comprised ten strains, which agglutinated with the Shiga serum in dilutions up to 1 in 400, viz., the Shiga bacillus, the Kruse bacillus, four strains isolated from acute epidemic dysentery at Doberitz, two strains isolated by Pfuhl from dysentery cases in China, a strain isolated by Flexner at Newhaven, U.S.A., and a strain isolated by Muller at Sudstiermark. The authors state that these strains also closely resembled one another in morphological and cultural characters. In Group 2 they placed all the other strains tested by them, including the strain isolated by Flexner in the Philippines, a strain isolated by Strong in the Philippines, and various " pseudo-dysentery " bacilli (Martini and Lentz, Schmiedecke, Lentz, Kruse, Pfuhl, and Deyche).

Apart from the distinction into two groups, it is noteworthy that Martini and Lentz report that the Philippine bacilli of Flexner and of Strong are different from the Shiga bacillus: other observers having pronounced for their identity. It is also to be noted that Flexner's Newhaven bacillus is classed with Shiga: it, also, is different from Flexner's Philippine strain. As further evidence of these differences, Martini and Lentz report that the serum of a rabbit immunised with Flexner's Philippine bacillus markedly agglutinated this bacillus, but did not agglutinate the Shiga bacillus, nor Flexner's Newhaven bacillus: further, it had only a slight action on Strong's Philippine bacillus, which thus appeared to be different from Flexner's Philippine bacillus; that is to say, the bacilli of Group 2 are not all of the same species as are the bacilli of Group 1.

Martini and Lentz conclude their able and interesting paper by formally stating the following conclusions:—

1. The serum of dysentery convalescent patients is untrustworthy as a means of identifying the bacilli obtained from the intestine of dysentery cases.
2. This identification can only be made by the use of highly agglutinative serum, obtained by active immunisation with one or other strain.
3. Shiga's, Kruse's, Muller's Flexner's Newhaven (U.S.A.), Pfuhl's Chinese, and the Doberitz bacilli of 1901 are identical; and it is a striking circumstance that they were all obtained from cases occurring in epidemics in the north temperate zone.
4. Other bacilli of dysenteric conditions—for example Flexner's Manila, Strong's Manila, Deyche's Constantinople, and Kruse's asylum bacilli—are different from those above mentioned.

In another communication Dr. Lentz reports (43) further studies upon the same types of bacilli dealt with in the paper just considered. From certain indications he believed it might be possible to differentiate between the two groups by cultural characteristics, as well as by the agglutination tests. The differences exhibited by growths upon ordinary culture media were too slight to be of service, but the different actions of the bacilli on various sugars gave some hope of finding a medium suitable for diagonstic purposes. This idea Lentz tested by growing the various types upon litmus agar containing 1.3 per cent. of one or other of the following substances—maltose, dulcite, dextrin, fructose, inulin, and mannite. He found the last—mannite litmus agar—would serve the purpose. Upon this medium the bacilli of Group 1 ("true dysentery") exerted no action: but the bacilli of Group 2 turned it violet red or red (acid); in the case of two pseudo-dysentery species, blue (alkaline). That is to say, with this medium the bacilli could be grouped in exactly the same way as resulted from the application of the agglutination test with specific (Shiga) serum. The other media were not suitable, because all (in the case of dulcite and inulin), or some (in the case of maltose, dextrose, fructose) of the bacilli of Group 2 behaved as did the bacilli of Group 1. The mannite medium, however, sharply marked off the two groups from one another. It is to be noted that the Flexner and Strong bacilli are again separated from the bacilli of "true dysentery." Both the Philippine strains behave similarly on mannite litmus agar—turn it red; but maltose litmus agar and dextrose litmus agar are turned red with the Flexner strain, but are left unchanged by Strong's bacillus; so that here again Flexner's strain is shown to be different from that of Strong. Lentz recommends mannite litmus agar as a simple and reliable medium for the determination of the bacilli of dysentery (44).

The applicability of the two tests just mentioned, viz., agglutination with a high-value specific serum and the mannite re-action, has been investigated by various observers, with the result of confirming the contentions of Martini and Lentz. By their use J. W. H. Eyre (45) was also able to recognise two groups: Group 1 containing the Shiga, Kruse, and Flexner Newhaven bacilli, to which Eyre added a strain isolated by himself from asylum cases; whilst Group 2 contained Strong's Philippine bacillus, another Philippine strain isolated by Harris, and a strain isolated by Duval from infantile diarrhœa. Essentially the grouping is the same as that made by Martini and Lentz. To their Shiga group of bacilli from acute epidemic dysentery Eyre adds an asylum strain isolated by himself. In Group 2 he notes that Duval's bacillus from infantile diarrhœa is different from the bacilli of Strong and Harris, obtained from acute epidemic dysentery in the Philippines. Charles Todd (46) similarly places Shiga's, Kruse's, and Eyre's bacilli in one group, and Flexner's Philippine and Duval's bacilli in another. F. P. Gay (47), Philip Eisenberg (48), Karl Leiner (49), and others, report corresponding results. W. H. Park, Katherine H. Collins, and Mary E. Goodwin, working with these tests in the research laboratory of the Health Department of New York, arrive at classifying the various strains of bacilli into three groups as follow (50):—

I. Bacilli of the Shiga type. Produce little or no indol: do not ferment mannite, maltose, or saccharose. Animals immunised with them furnish agglutinins very active against the bacilli of this group, but little active against those of the other two groups.

II. Bacilli which produce indol: ferment mannite, but not maltose nor saccharose. Animals immunised with them furnish agglutinins specially active for this type, but active also against the bacilli of Group 3.

III. Bacilli of the Flexner Manila type. Produce indol: ferment mannite, maltose, and saccharose. Animals immunised with them furnish agglutinins specially active for the type, but active also against the bacilli of Group 2 (and against bacillus coli communis).

The bacilli of the first type are regarded as the true dysentery bacilli; they are responsible for the most severe dysenteries. The bacilli of Groups 2 and 3, although widespread and associated with characteristically dysenteric conditions, are less specific of this malady because found also in cases lacking dysenteric symptoms. The authors christen the bacilli of these groups "para-dysentery bacilli": they are related to bacillus coli communis.

In addition to the indications afforded by the tests just considered, similar group relationships are manifested by re-actions of other kinds. For instance, Todd has reported (51) that the toxins of the Shiga and Eyre bacilli were neutralised by a serum prepared by means of Kruse bacilli. His attempts to obtain toxins from Flexner's Philippine and Duval's bacilli were unsuccessful. Eisenberg, working with a Shiga serum, found that Shiga bacilli absorbed all the agglutinins therefrom, rendering the serum inactive against Flexner's bacillus. But Flexner's bacillus could not so absorb all the agglutinins: after treatment with this strain the serum was still agglutinative towards Shiga bacilli. R. H. Firth (52) appears to have determined a similar differentiation on the basis of pathogenesis; but his original paper is not available to us, and the reference note at our disposal is not sufficiently detailed to enable us to correlate Firth's results with those of the other authors mentioned.

These different investigations all converge to one issue, namely, that associated with cases having the characters of dysentery there are bacilli which fall into one or other of at least two groups. They belong either to

the group ("true dysentery") of which Shiga's bacillus is the type, or to another group ("para-dysentery") the members of which show variations amongst themselves. The groups are differentiated one from the other by the results of agglutination with specific sera, and by the reaction with mannite, as well as by other means.

The bacilli which have been isolated from acute epidemic dysentery have belonged sometimes to the "true dysentery" group (Shiga, Kruse, Flexner-Newhaven, Pfuhl, Muller, &c.), and sometimes to the "para-dysentery" group (Flexner, Strong, and Harris Philippine strains). The bacilli which have been isolated from cases of asylum dysentery have belonged sometimes to the "true dysentery" group (Eyre), and sometimes to the "para-dysentery" group (Kruse). The bacilli which have been isolated from infantile diarrhœa have rarely belonged to the "true dysentery" gronp (Cordes (53), (Duval & Schorer) (54), more usually they have been "para-dysentery" bacilli (Martha Wollstein, Karl Leiner, Howland (55), Park, Collins & Goodwin, Cordes, Duval & Schorer).

Evidently it is necessary to abandon the idea that all bacillary dysenteries are caused by Shiga's bacillus. No doubt this species is responsible for a large number of cases of dysentery. It may even be justifiable to regard these cases as constituting a specific disease which various authors have called "true dysentery." From this point of view it might be separated off from other forms of dysentery, just as amœbic dysentery has been separated; but the factors available for discrimination are much less manifest. There is no present evidence that the symptoms, course, or pathological anatomy differ from those of other bacillary dysenteries, nor can a distinction be made on the basis of epidemiological prevalence. The distinguishing feature is the isolation and identification of Shiga's bacillus; and it would appear that even this may fail to decide the matter. Wm. H. Park and H. W. Carey, in connection with epidemic dysentery at Tuckahoe, north of New York, report (56) the isolation of a bacillus belonging to the Shiga-Kruse group. Neither it nor a type Shiga, agglutinated with Flexner (Baltimore) serum. The Flexner (Baltimore) bacillus with which the serum was prepared differs from Shiga in that it produces indol and ferments mannite; and is said to be identical with Flexner's Manila and American strains. The bacillus isolated did not do these things: it appeared to be a true Shiga bacillus. Nevertheless the blood serum of patients at Tuckahoe was found to agglutinate the Flexner Baltimore bacillus. From these results the authors are inclined to believe that, in the Tuckahoe outbreak, the Flexner strain was associated with the Shiga bacillus, although the latter alone was isolated. (But, as regards the validity of inferences drawn from the agglutination re-actions with patients' serum, see the views of Martini and Lentz, mentioned above.) It is to be noted that Duval and Schorer, in each of five cases, isolated both Shiga's bacillus and a "para-dysentery" bacillus. In another communication Duval and F. P. Gay report the simultaneous isolation of bacilli of both types from the intestine of a man dead of dysentery, and twice from the sanious stools of women who recovered. How far these complications can be resolved remains for the future to determine.

In the meantime it must be confessed that the etiological investigations hitherto made with respect to bacillary dysenteries, although they have added considerably to our knowledge of the subject, have not yielded such results as to clear away the lack of definition referred to at the beginning of this paper. Even if it be conceded that the disease caused by Shiga's bacillus is exclusively to be regarded as "true dysentery," the concession must be made with the reservation that a precisely similar complaint apparently can be produced by other species.

References.

(1) Osler. Principles and Practice of Medicine, Ed. v., 1903, p. 193.
(2) Tropical Diseases, 1903, p. 376.
(3) Diseases of Warm Climates, Eng. Transn., 1903, p. 463.
(4) Allbutt's System of Medicine, Vol. II., p. 408.
(5) Ibid, pp. 954 and 940.
(6) Fagge's Principles and Practice of Medicine, Ed. IV., Vol. II., p. 396.
(7) Diseases of Infancy and Childhood, Ed. II., p. 381.
(8) Report on Colitis or Asylum Dysentery; Ref. *B.M.J.*, 1901, Vol. I., p. 838.
(9) For references to amœbic dysentery not otherwise noted, see exhaustive papers by Councilman and Lafleur, Johns Hopkins Hospital Reports, Vol. II., p. 545; and by A. W. Campbell, Journal of Pathology and Bacteriology, Vol. VI., 1900; also a review of the subject by Miss Sheldon Amos, Journal of Pathology and Bacteriology, Vol. VIII., 1903, p. 346.
(10) Intercolonial Medical Congress of Australasia, Brisbane, 1899, p. 82.
(11) Arbeiten aus dem Kaiserlichen Gesundheitsamte, Bd. XIX., 1903, Ref. paper by Dr. McWeeney, *Lancet*, Vol. I., 1905, p. 783.
(12) Bureau of Government Laboratories, Biological Laboratory, Manila, No. 18; Ref. Bull. Inst. Pasteur, 1905, p. 154.
(13) Centralblatt fur Bakteriologie, XXIII., s. 599.
(14) Ibid., XXIV., s. 871.
(15) Deutsche Med. Wochenschrift, Bd. XXVII., 1901, 783.
(16) Johns Hopkins Hospital Bulletin, 1900, 231.
(17) Deutsche Med. Wochenschrift, 1900, 386.
(18) Centralblatt fur Bakteriologie, XXX., 1901, 453.
(19) Deutsche Med. Wochenschrift, Bd. XXVII., 1901, 785.
(20) Martini and Lentz, Zeitechrift fur Hygiene, Bd. 41, 540.
(21) Journal of Experimental Medicine, Vol. VI., 1902, p. 181.
(22) Proceedings of New York Path. Soc., III. (N.S.), 141; Ref. Bull. Inst. Pasteur, 1904, 161.
(23) Journal of Medical Research, Vol. IX., 1903, 180; Ref. Bull. Inst. Pasteur, 1903, 361.
(24) Deutsche Med. Wochenschrift, 1903, 841.
(25) Centralblatt fur Bakteriologie, XXXVI., 1904, Originale, 368.
(26) Ref. Centralblatt fur Bakteriologie, XXXVI., 1905, Referate, 131.
(27) Ref. Centralblatt fur Bakteriologie, XXXI., 1902, Originale, 558.
(28) Quoted by Kruse, Deutsche Med. Wochenschrift, 1901.
(29) Deutsche Med. Wochenschrift, XXIX., 1903, 97.
(30) Annales de' l' Institute Pasteur, XVII., 1903, 463.
(31) Indian Medical Gazette, 1903, 53.
(32) Journal of Hygiene, Vol. IV., 1904, 495.
(33) Arch. fur Schiffs u Tropenhyg., Bd. VIII., 1904, 27; Ref. Bulletin Inst. Pasteur, 1904, 300.
(34) Centralblatt fur Bakteriologie, Bd. XXXI., 1902, Originale, 205.
(35) Journal of Experimental Medicine, VI., 1902, 181.
(36) Edin. Med. Journal, 1904, 489.
(37) Centralblatt fur Bakteriologie, Bd. XXXIII., 1903, Originale, 52.
(38) Australasian Medical Gazette, 1905, 10.
(39) Martini and Lentz, Zeitschrift fur Hygiene, Bd. 41, 540.
(40) Journal of Medical Research, 1903, Ref. Bull. Ints. Pasteur, 1903.
(41) Journal of Medical Research, 1903, Ref. Bull. Inst. Pasteur, 1903.
(42) Zeitschrift fur Hygiene, Bd. 41, 540.
(43) Zeits. fur Hygiene, Bd. 41, 559.
(44) See also H. Hetsch, Cent. fur Bakt., 1903, XXXIV., Orig. 580
(45) Edin. Med. Journal, 1904, 489.
(46) Journal of Hygiene, 1904, 480.
(47) Univ. of Pensyll. Med. Bull. XVI., 1903, 171; Ref. Bull. Inst. Pasteur, 1903, 836.
(48) Wien. Klin. Woch., XVII., 1904, 1142; Ref. Bull. Inst. Pasteur, 1905, p. 26.
(49) Wien. Klin. Woch., XVII., 1904, 695; Ref. Bull. Inst. Pasteur, 1904, 704.
(50) Proc. New York Path. Soc., III. (N.S.), 1903, 141; Ref. Bull. Inst. Pasteur, 1904, 598.
(51) Journal of Hygiene, Vol. IV., 1904, 480.
(52) Journal R.A.M.C., Ref. Bull. Inst. Pasteur, 1904, 151.
(53) Ref. Bulletin Inst. Pasteur, 1904, 161.
(54) Ibid.
(55) Ibid.
(56) Journal of Medical Research, 1903, Ref. Bull, Inst. Pasteur, 1903, p. 361.

SECTION OF STATE MEDICINE AND MEDICAL ETHICS.

PRESIDENTIAL ADDRESS.

By G. A. Syme, M.S., F.R.C.S., Eng.

Allow me to begin this Address with a grateful acknowledgment of the honor conferred by the Executive of the Congress in asking me to preside over this Section. I appreciate the compliment very highly, and the more because I feel it to be undeserved.

I accepted the position, however, very reluctantly, and with much misgiving. It seemed the irony of fate to preside over a Section the utility of which I much doubted. The meeting together of our members, from widely separated and different parts of the Commonwealth and New Zealand, certainly affords opportunity for comparison of differences in methods in State Medicine, and, as the first President of the Congress observed, in his address in this city eighteen years ago, for " making common property what suggestions relating to medical ethics or legal status arise out of our colonial life." But will the discussion of such matters *coram populo* promote, what I venture to hold ought to be the main object of these Congresses, the advancement of our profession as a science, not only among ourselves, but in the eyes of the public ? We shall see.

At the first Congress, a section of State Medicine was instituted. Dr. Whittell, its President, said very truly in his address " that the term was an indefinite phrase, meaningless in itself, but which from usage is understood to express the action and duty of the State in relation to the physical welfare of its subjects." He did not say what he meant by State, but confined his attention to the duty of the State, evidently meaning the Government, in providing for the removal and prevention of disease. The term " Public Health " is now more generally used when speaking of this particular aspect of the subject, and in succeeding Congresses the title of the section was changed to " Public Health, including State Medicine," and sometimes other subjects, as Psychology and Forensic Medicine. Now we have two separate sections, one of " Public Health, &c.," and our own, called " State Medicine and Medical Ethics." I confess I have been somewhat puzzled to know what " State Medicine," as distinct from " Public Health," is supposed exactly to include. I propose to regard it as covering the relations of the State, *i.e.*, the body politic, both generally as the community, and particularly as the Government, to the medical profession, and its duty in caring for disease.

Prof. Metchnikoff, in that intensely interesting book, " The Nature of Man," has pointed out, more clearly and recently than others, that most of man's troubles and difficulties arise from mal-adjustment to changed environment, arising in the course of evolution. The medical profession is no exception. In its relations with the State, many such mal-adjustments exist. Many, probably the majority, of them arise from popular ignorance regarding the evolution of medicine as a science, popular misconceptions as to its true aims, and inability to appreciate both its immense potentialities and the nature of its limitations. In some instances, these mal-adjustments have become so great as to produce almost a war between the profession and

certain sections of the community. We constantly read in our journals, *e.g.*, of the "Battle of the Clubs." I venture to think that the profession, instead of fighting certain sections in the State, and especially fighting them with their own weapons, should try to educate them ; endeavor to make every member of the community realise that medicine is not a trade, whose business is the selling of charms and specifics, but a progressive science. When its true aims and aspirations are thoroughly "understanded of the people," the profession will be appraised at its due worth, and most of the grievances of its members, I believe, will be set right.

Educated people generally do appreciate us. R. L. Stevenson said of the physician—"He is the flower (such as it is) of our civilisation ; and when that stage of man is done with, and only remembered to be marvelled at in history, he will be thought to have shared as little as any in the defects of the period, and most notably exhibited the virtues of the race. Generosity he has, such as is possible to those who practice an art, never to those who drive a trade ; discretion, tested by a hundred secrets ; tact, tried in a thousand embarrassments ; and, what is more important, Herculean cheerfulness and courage."

When such an appraisement becomes universal on the part of the public, and is universally deserved by the profession, there will not be many maladjustments to rectify. In the meantime they exist, and afford matter for discussion in this Section. What I fear is, that the trade aspects of the questions brought before us may occupy too much attention. I hope that our discussions may be of educational value, to the public as well as to ourselves.

Let us now look a little more in detail at some of these relations between the State and the Medical Profession.

In Great Britain, the relationship starts at the very commencement of a medical student's career. The State, as Government, through the powers entrusted to the General Medical Council, determines the course of study he is to pursue. The Government does not, however, assist very materially in directly providing for his medical education. In Australia, the position is reversed. The Governments of New South Wales, Victoria, and South Australia contribute very largely to the support of the University Medical Schools, but do not interfere with the course of study. The Government does not, in any State, come into direct relation with the profession until the possessor of a medical diploma wishes to be registered as legally qualified to practise. The requirements for registration vary greatly in the different States of the Commonwealth. It is highly desirable that they should be made uniform, and that the portals to the profession should be the same in every part of Australia. Something has been done in this direction in recent years by the amendments of the Medical Acts passed in New South Wales and South Australia. It is surely an anomaly that while Australians, to obtain a qualification in their own country, must pass through a five years' course at their own Universities, in Victoria, Western Australia, and Queensland, anyone who has passed through a regular course of medical study of not less than three years in a foreign school of medicine, and received a diploma, can be registered. In South Australia the course must be four years ; in New South Wales only it must be five years. The Tasmanian Act, though passed in 1842, is very effective for preventing unqualified practice, and is the only Act that is. It expressly says, "No person shall practise as physician, surgeon, or apothecary, or prescribe or do any surgical act," unless certificated by the Board. Another serious defect in the Victorian Act is that, once a person's name is on the Register, it cannot be removed, although the body that gave him the diploma under which he registered may have taken it away for

misconduct. The State Premiers, at their next conference, might well consider the advisability of making the various Medical Acts uniform. In any new legislation, it should be provided that qualifications that are not registerable in Great Britain should not be recognised in Australia. It has been suggested that medical registration should be a function of the Commonwealth Government, with a Federal Medical Council for Australia, similar to the General Medical Council of Great Britain. Owing to the great distances between the different centres of the Commonwealth, such an arrangement would probably prove expensive and inconvenient.

Whether the requirements for registration as medical practitioners should be materially altered is a matter for grave consideration. The fulfilment of those now in force in most countries ought to be a guarantee to the public of theoretical knowledge, but is not something more necessary—some more satisfactory guarantee of competence for actual practice ? In that interesting, if morbid and exaggerated, book, "The Confessions of a Physician," the Russian author describes very graphically his despair on first entering into actual practice, and the pitfalls into which his "abstract bookish wisdom" led him, owing to want of practical experience. So great was his mental distress, that he threw up his practice and returned to St. Petersburg for further practical work, quoting Billroth—"That only a doctor without a drop of conscience can permit himself, without more ado, to make use of the rights which his diploma gives him."

Some years ago, the General Medical Council of Great Britain recognised that further requirements were necessary. The Council imposed a fifth year of study, recommended to the licensing bodies that it should be devoted entirely to clinical work, and provided that six months of it *may* be passed as a pupil to a registered practitioner. Is this enough ? Judging from my own experience, I should say that it is not. I was foolish enough, when just graduated, to take a position as *locum tenens* for a practitioner who had been suddenly taken ill. He had a very large general practice, with numerous lodges. My experiences were not so tragic as those of the Russian physician referred to. but they were extremely disheartening. I was quite satisfied that, in spite of my legal status, I was *not* qualified to practise, nor did I attempt to do so till I had done six years' additional work, holding various subordinate hospital appointments, in which I could acquire practical knowledge, technical skill, and a feeling of confidence. It seems to me a question for very serious consideration whether practitioners should be registered unless they can show evidence of practical work, either as resident medical officer, or pupil at a hospital, or as assistant to a medical man in practice, subsequent to the taking of a diploma. The difficulty is to obtain such positions, as the Russian writer of the "Confessions" found to his bitter sorrow. Much could be done if every hospital in Australia appointed Resident Medical Officers annually, and reserved the positions for recent graduates of our own Universities. At the metropolitan hospitals, also, provision might be made for resident senior pupils, as at the Johns Hopkins' Hospital.

Medical Acts, all the world over, have been passed, not in the interests of the medical profession, but for the purpose of protecting the public, and enabling them to distinguish between those who are regarded by the State as properly qualified, and those who are not. A considerable number of medical practitioners thinks that Legislative action should be taken for the repression of unqualified practice altogether. Most of those who clamor for the legal restriction of quacks do so openly in the interests of the profession, as much as in the interests of the public. I venture to take a different view. Personally, I do not believe that quackery does the slightest harm to the medical profession, from a pecuniary point of view. But that is not the important

question. What is requisite is that the public should be better informed on these matters, and more will be done by educating the people than by legal enactment. People must be made to realise both the limitations and possibilities of treatment, and must get rid of their antiquated and superstitious notions of the action of drugs, too many of which, I regret to say, are too often supported by some of our own profession. Much could be done by making public the real composition of quack preparations, and here legislation can be of service. The mysterious always appeals to the ignorant. Even the cultured can be attracted by artistic mysticism, as evidenced by the vogue of Maeterlinck, the so-called Shakespeare of Belgium. Take away the cloak of secrecy from quack medicines; let it be known that so-and-so's "Wizard Oil" is only turpentine, camphor, and ammonia; that somebody else's "Soothing Syrup" is composed of morphia, with essence of anise and balsam of tolu, and the occupation of their manufacturers would be, like Othello's, gone. Hence the angry commercial protest against recent legislation in New Zealand on the subject of patent and proprietary medicines. The New Zealand Parliament passed an Act, under which regulations were framed making it compulsory that the formula of all patent and proprietary medicines should be printed on the bottle, and that if any poison were contained in the preparation it must be labelled accordingly. The manufacturers of patent medicines, and their agents, raised such powerful protests, and brought such influence to bear, that the regulations were modified, to the extent that the formula, instead of being placed on the bottle, was to be lodged with the Health Department. The manufacturers still objected, and, as no further alterations in the regulations could be obtained, they decided not to send their medicines to New Zealand. Evidently they realise very clearly that, once the real nature and commercial value of their preparations became publicly known, they would have no demand at the extravagant prices at which they had hitherto been sold.

Having been registered, the legally qualified medical practitioner comes into relation with State authority in various ways. He is bound to attend a summons from a coroner to attend and give evidence at inquests, and to make *post-mortem* examinations. A coroner is invested by the State with very plenary powers, and the discretion with which he exercises them may sometimes be called in question, especially by the medical profession. It has been much debated, *e.g.*, whether he should instruct the nearest practitioner, or a pathologist, with special experience, to make *post-mortem* examinations. As a general rule, I think it is unquestionably best, both in the interests of the public and of the profession, that such examinations should be made by skilled pathologists. The coroner, however, should be very careful that, consistently with the safety of the public, the reputation and interests of general practitioners giving evidence, or connected with the case, are protected. The main object of the inquiry is to ascertain the mode of death, and for this the medical evidence is generally all-important. If the coroner, by his attitude to medical witnesses, practically closes their mouths, as happened recently in Melbourne, he may be unable to ascertain the exact cause and mode of death.

The Coroner's Acts in Australia have been copied from the English Act, without taking account of altered conditions. Thus, if the practitioner reside more than ten miles from the place where the inquest is held, he can only be paid 1s. a mile for every mile of such extra distance. In the scattered districts in the interior of Australia this arrrangement, probably fairly equitable in England, is very unfair. Cases have been brought under the notice of the Medical Defence Association of Victoria where men have had to travel long distances, involving absence from practice of two days and nights, with

attendant expenses, for which the payment authorised by the Act was very inadequate. Even in England complaint is made as to the inadequacy of fees to medical witnesses, and some time ago a joint deputation from the British Medical Association and the Police Surgeons' Association memorialised the Home Secretary, asking that the fees for medical witnesses should be raised. The application was not granted. In almost all cases where the Government demands service from medical practitioners it pays for them very poorly. The fees for vaccination and notification of infectious disease are ridiculously small. In Victoria, the post of Government Vaccinator in country districts entails a good deal of badly paid work, inasmuch as the vaccinator has to arrange beforehand to visit certain distant townships on a fixed day. He then probably has only a very few cases to vaccinate at the ordinary fee, for which he has had to sacrifice perhaps a whole day's remunerative work. As it is not compulsory for men to take these appointments, a little combination among medical practitioners in a district would probably compel the Government to pay more adequately in special cases.

Let us next consider the duties and responsibilities of the State in caring for disease. Here we find the greatest diversity of procedure, not only in Australia, but all over the world. In all the States of the Commonwealth, the Government contributes largely to the support of public hospitals, and often provides some hospitals entirely. The amount contributed by the charitable public, and the degree to which the Government can interfere in the management, vary, not only in each State, but in each hospital. Here, in South Australia, I understand, the Government practically maintains all the hospitals, and, if it chooses, can exercise control over the management, as notoriously exemplified some years ago in the appointment of Drs. Leith Napier and Ramsay Smith to the Adelaide Hospital. In New South Wales, the Government has contributed very largely to both the building and maintenance of hospitals, and public subscriptions have not been at all proportionate. Last year, *e.g.*, in Sydney, the Royal Prince Alfred, Sydney, and Children's Hospitals cost £49,236 for maintenance, towards which the Government contributed £31,781, and the public subscribed £9,282. In addition, the Coast Hospital, entirely supported by Government, cost £20,130. It is probable, however, that the Government grant to hospitals in New South Wales will be much reduced in future. In Victoria, Government and public contribute about equally. The management is practically free from Government control.

When Prof. Senn, of Chicago, was visiting us a little time ago, he said to me—"You have far too many hospitals and charitable institutions in Australia, in proportion to your population." His observation was correct. According to Mr. Bruck, there was one hospital to every 12,299 people in Australia in 1899. Many of these institutions overlap in their work, and money is wasted in expenses of management and collection of subscriptions. Population is unsettled and shifting, and hospitals established in country towns, especially near goldfields, when they were thriving, are still maintained, though the population has left, and the hospitals are not really necessary. To quote Mr. Bruck again, the total number of beds available was 11,280, but the average number of in-patients a day was only 7,809. It is evident that some central board or authority is required, in each State, to control all the hospitals, and co-ordinate their functions. On this central board, and also on the boards of management of the individual hospitals, the medical profession should be represented. The executive officer of the Central Board, also, should preferably be a medical man.

In providing for the care of disease, it seems to me that it is the duty of the State, using the word in its widest sense, to furnish free accommodation

and treatment for the destitute sick, but for no others. It is not necessary, however, to provide palatial buildings, luxurious comforts, and the attendance, in an honorary capacity, of the most eminent men in the profession. To a large extent it is the very excellence of the public hospitals that causes them to be abused.

If we glance " into the dark backward and abysm of time," we see that the present system of State care for the destitute sick has not always existed. In pre-Christian Rome, the sick poor were attended by the public medical officers, who were appointed for every district in the city. They were well paid, and also enjoyed many immunities. In ancient Arabia, and other Mohammedan countries, hospitals, with paid physicians, were established by various rulers. With the advent of Christianity, the care of the sick became a religious duty, and hospitals became religious institutions, attached to monasteries, or founded by priests. St. Bartholomew's, the oldest hospital in England, was founded by Rahere, first Prior of the convent of Augustinian canons, and in charge of a master, brothers, and sisters. St. Thomas' was founded by the canons of St. Mary Ovaries, and held by the Prior of Bermondsey. The monks and priests were the first physicians at these hospitals, and naturally gave their medical services gratuitously. It might be imagined that out of this custom grew the practice of honorary medical service, when physicians and surgeons came to be appointed to these hospitals. But it appears it was not so. The first physicians and surgeons at both St. Bartholomew's and St. Thomas' were paid. The immortal Harvey was a paid physician at St. Bartholomew's. So late as 1738, a writer, describing St. Thomas', says that the physicians and surgeons had salaries, " as well as perquisites considerably better than the salary."

When and how these positions became honorary I have not been able to ascertain. St. George's Hospital, founded in 1733, was the first in England to be established and supported entirely by voluntary subscriptions, and its first staff was elected by a meeting of the subscribers. It consisted of six physicians and three surgeons, all of whom " had declared their willingness to serve without fee or reward." They were, however, to be " permitted to take pupils," which possibly explains the willingness to serve without payment. The need of clinical teaching in the training of medical men became recognised very gradually, being first properly instituted in Leyden, in the 17th century. For some time before this, certainly in 1561, the physicians and surgeons at the great London hospitals, St. Bartholomew's, St. Thomas', and Guy's, took pupils or apprentices, who paid large fees, and were instructed at the hospitals. The position of physician and surgeon at these hospitals thus became very valuable and lucrative, both directly and indirectly, and was much sought after. The profession is apt to take credit for the large amount of gratuitous work done by its members, and rightly. But, so far as large clinical hospitals are concerned, there is a good deal of cant about the noble generosity of the honorary staff in giving its highly skilled services to the suffering poor gratuitously. To men who desire to become and remain leaders of the profession, positions on the honorary staff of a clinical hospital are almost indispensable, and are accordingly keenly competed for and much coveted.

Unfortunately, the custom of honorary service has spread from large clinical to nearly all hospitals, and the amount of gratuitous work done by the profession, in British communities, is very great. In my opinion, it should be very much restricted. At clinical hospitals, it is necessary to have a staff of eminent men as teachers, who should be adequately paid for teaching. All the beds in such hospitals should be used solely by destitute patients, who, by providing clinical material, give all they can. In all other hospitals

the medical staff should be paid. Burdett, in his great work on hospitals, says—" We believe, as the result of prolonged investigation, an intimate knowledge of the facts, and great practical experience, that if the voluntary hospitals were to determine that every medical officer, both resident and non-resident, should henceforth be paid for his services, the cost per bed at every institution which thus asserted its independence would be considerably reduced. The medical profession has rendered a great deal too much eleemosynary service in the past." The ideal arrangement, to my mind, is such as obtains at the Johns Hopkins' Hospital, Baltimore. There the endowment is sufficient to provide handsome salaries for all the staff, who are also teachers in the Johns Hopkins' University. In no hospital in the world is better work done.

Besides hospitals for the really destitute, paying hospitals are required for the poorer middle class, who cannot afford ordinary private hospital charges, together with the fees of leading members of the profession. A great deal might be done by the Friendly Societies, as I suggested years ago in the leading columns of the *Australian Medical Journal*. They could establish hospitals for their members, where they would be attended by their own lodge surgeons. When required, the services of a paid consulting staff should be available, just as now lodge members obtain the services of specialists. This consulting staff might be composed of young men, not yet in the height of their fame, perhaps holding junior positions on the staff of a clinical hospital.

Most American hospitals have paying wards. When a hospital, like the Johns Hopkins', is founded and maintained entirely by private benevolence, and when its staff is paid, I see no objection to its having paying wards. Nor can there be much objection, under other conditions, if the paying patients are permitted to select their own medical attendants, whether on the staff of the hospital or not. But to have paying wards attached to a public hospital, subsidised by Government, and restrict the paying patients to the attendance of the staff, is very unfair alike to the patients, the outside profession, and those who keep private hospitals. I have had personal experience of the difficult situations such a condition creates, as a member of the staff of the St. Vincent's Hospital, Melbourne, when it used to take paying patients. I strongly opposed the practice, which, I am glad to say, was given up.

While on this subject, I would like to say a few words on a rather delicate question, viz., whether private hospitals ought to be conducted by medical men? I hold they ought not. It seems to me that the more all members of the profession keep to the "practising of an art," and away from "the driving of a trade," the better. Conducting a private hospital is purely a trade. Plenty of capable non-professional people are willing to carry it on properly, and to them it should be left. It is a trade which involves its driver in many petty annoyances and worries, which must distract a professional man, and interfere with his whole-hearted attention to his proper work, which is sufficiently exacting and anxious as it is. The more the medical profession presents its commercial side to the public, the more it loses in dignity and esteem, and the more difficult is it to induce the public to worthily appraise its higher aims. Further, a private hospital must be, more or less, a continual temptation to the medical conductor. It is so easy to make it a means of advertising, and it has to be kept filled.

Gratuitous attendance is another unsettled question, concerning which every member of the profession is a "law unto himself." It is desirable that some uniformity of practice should be established. It is a general custom not to charge fees to medical men, but Dr. Saundby says—" Medical practitioners have no right to the gratuitous services of their colleagues." To

what relations of a medical man should gratuitous attendance be extended? I would say only to those actually and absolutely dependent on him, and living in his house. What about the families of deceased practitioners? There seems to be no general rule. Each case must be treated on its merits. I was once adversely criticised for charging the widow of a medical man. She was living with, and apparently dependent on, her son, who was in a good position, and I did not even know at the time that her husband, long dead, had been in the profession. It is customary to see medical students and nurses without fee; some extend the custom to dentists and chemists, but I don't see why, and don't think it ought to be done. In fact, it would be more satisfactory to everyone if gratuitous attendance were limited as much as possible. As Saundby say—" Medical practitioners should only forego their fees to those persons who are unable to pay, and should refuse to recognise any other grounds for the concession. The clergy, as a class, have no right to medical attendance without payment." Dr. De Styrap devotes several pages of his " Code of Medical Ethics " to justifying the position that " gratuitous attendance on ministers of religion and their families should no longer obtain." The profession, however, is not by any means unanimous on the question. Some do, and some do not, charge clergymen. My own practice, in the past, has been not to charge. I have lately come to the opinion that it is right to adopt Saundby's principle, and only remit fees to the clergy when they are obviously unable to pay.

If our scales of fees were more elastic, and the minimum fees less, it would be much easier to make it the rule to charge everyone. The rates adopted in the scales in use in New South Wales, Victoria, South Australia, and Queensland are very different, and it is desirable that they should be made more uniform. Thus, in Victoria, the minimum fee for a visit or advice at the house is 10s. 6d. In New South Wales, South Australia, and Queensland a minimum fee of 5s. is permitted, and in New South Wales, for what is called a second class visit in town, 2s. 6d. Discrepancies occur, also, in the fees for operations. Any scale of fees must be necessarily elastic, and absolute uniformity is not to be expected. The scale adopted in New South Wales appears to me to be satisfactory, and the other States might very well come into line with it, at all events as regards the minimum. It may be interesting to note that the first Medical Society in Victoria—the Port Phillip Medical Association—in September, 1846, drew up a scale of fees for first, second, and third class patients ranging from 10s. 6d. to 2s. 6d. I have no hesitation in saying that fees will have to be lowered, especially in Victoria, and particularly the ordinary fee for general work. As my friend and colleague, Dr. Howard, pointed out, in his presidential address to the Medical Society of Victoria, at the beginning of the year, disease is less prevalent, population is increasing slowly (2 per cent. in ten years, while the medical profession has increased 20 per cent. in the same time), and the number of patients attending hospitals has increased (probably 20, or even 30 per cent.). Under such conditions the incomes of the profession must suffer, and one remedy is a reduction of fees. Had fees been lower, and hospital accommodation not so lavish, many of those attending hospitals would have been paying fees to practitioners as private patients.

Private patients! Now we come to the relations of the profession to the individuals of the State, and of its members to each other—that is, to questions of medical ethics.

It is commonly held that medical like all other questions of ethics can be summed up by saying—" Whatsoever ye would that men should do to you, do ye even so to them." While it is true that all professional conduct should conform to this golden rule, society is so complex, and its members so varied,

that different people may hold conflicting opinions as to " what men should do to them." Some generally recognised rules of conduct in professional relations are, therefore, desirable. Such rules, however, should be as general and as few as possible. Detailed complicated codes of medical ethics are, in my opinion, a mistake. The more stringent the code, the less likely is it to be carried out. The most ancient code of which we have any knowledge—that of Hamurabi, 1350 B.C.—was exceedingly drastic, and, if carried into effect, which it probably was not, would have almost prevented the practice of surgery. It is generally recognised that the more laws are minutely coded, as in Rome and France, the less is progress possible ; while in England, where there is no code, but Equity Courts exist, most advance has been made. Law and ethics, like everything else, must be modified with changed conditions and the gradual evolution of society. As regards medical ethics, it is necessary that every member of the profession should be informed as to generally recognised customs, or conventions, in his relations with his patients and his fellow practitioners. Very often so called breaches of medical ethics are committed in ignorance. It would be well that medical students should receive instruction on the subject. It is also highly desirable that the laity should be more familiar than it is with the general principles of medical ethics. The public is too apt to think that what it calls medical " etiquette " is some mysterious system for the protection of the profession, with peculiar laws, differing altogether from the principles which regulate the conduct of ordinary people. It cannot be too strongly enforced that the ethical conventions of the medical profession are quite as much in the interests of the patient as the practitioner, and are simply intended to promote fair and honorable dealing.

Unfortunately it is to be feared that some members of the profession are to a certain extent responsible for the view of the public, and hold what I cannot regard but as mistaken opinions as to the relations of practitioner and patient. One of these is the idea that a medical practitioner has a kind of proprietary right to a patient. We all recognise that it is to the patient's interest that a medical man who has attended a patient for some time, and knows his personal idiosyncrasies, and the various affections he may have had, knows his " constitution," as it is popularly termed, should continue to attend, and, if further opinion is desired, should, when possible, meet the consultant, and give all information. Again, it is not to the patient's interest that he should be attended by more than one medical man for the same illness and at the same time, except in consultation. Accordingly it is a rule of medical ethics that a medical practitioner shall not see a patient, wittingly, who is under the actual care of another practitioner, except in consultation, or in a sudden emergency. But such a rule does not imply that, once a medical man has seen a patient, that individual becomes as it were his commercial property, and that neither the individual nor any member of his family must ever consult any other practitioner, except with him, or on his advice. I have here two letters, bearing on these points, from general practitioners in the country to a leading consulting physician in Melbourne. In one, complaint is made because the consultant had seen a patient, who had at some previous time been under the care of the country practitioner, in a local hospital, and given a different opinion without communicating with the man in the country. The other complaint is that the consultant had seen a patient, who had been previously under the care of the country practitioner, as a lodge patient, and had not only seen him, but requested him to further communicate with the consultant by letter. I will quote the actual words used by this practitioner :—" The principle is a very wrong one which allows patients to see consultants without the advice of their regular medical attendant." . . " I understand now that a consultant is a man who consults with the patient, and not with that patient's ordinary medical man." The writer further accuses metropolitan

consultants generally of "seeking to get our patients." Another instance that came under my own observation recently may be given. A patient saw a consulting physician at his consulting rooms. After investigation, he advised her to go home, go to bed, and get a nurse. Finding she had some time previously seen a local practitioner, he arranged to meet him in consultation at her house, and leave the case in his hands. They met, but the local man was evidently annoyed, and demurred to going on with the case. He considered that his patient had no right to go to anyone unless he sent her, and that the physician had no right to see her and act as he had. I maintain that the attitude of these general practitioners is based on a misapprehension of the ethical rule previously mentioned. The first consideration must be the welfare of the patient. The general practitioner has no proprietary right in what he calls *his* patients. The patient has a perfect right to get any opinion he pleases. It may be that what he calls an "independent" opinion may not be so valuable as one obtained after a regular consultation with the previous attendant, who may give information considerably modifying the consultant's view of the case. On the other hand, a consultant may occasionally be unconsciously biased by what he is told.

While a patient has a perfect right to change his medical attendant, there are circumstances which debar certain practitioners from attendance. The ethical rule in such cases is put clearly and briefly by Saundby :—"No one is at liberty to take over the treatment of a case to which he has been called in consultation, or where he has acted as a substitute for the ordinary medical attendant." While preparing this address, my advice was asked about this very matter. *A* was away on a holiday, and arranged with a neighboring practitioner, *B*, to see patients for him. *A* had previously attended the household of *C*. The wife of *C* sent for *A*, and *A's* wife asked *B* to go as *A's* substitute. After *A* had resumed practice, *B* was asked by the lady to attend her. He seemed to think he would be justified in doing so, as *A* had never actually attended her. I advised him to explain the ethics of the case to the lady, and suggest that she should get *A*. He did so. She still wished *B* to attend, and he seemed to think he had no option but to do so. I think he had, and could have declined to attend, because he acted in the first instance as a substitute. The patient had the option of sending for *B* in the first instance, but did not do so. She sent for *A*, and but for *A's* introduction, *B* might never have been called in. Dr. Roderick Maclaren recently gave an address on the relations of medical men to each other, in which he remarks, "We are too chary of exercising our right to select our patients, which is just as definite as a patient's to select his medical attendant . . . . It happens to most men, when acting for another, to be asked to continue an attendance for themselves; to do so would be a breach of good faith towards the man for whom he acts, who has trusted him as a friend to take his work, or otherwise help him. A friend cannot honorably supplant him." In short, it is not a question of medical ethics, or the interests of the patient, but of general ethics, of what is fair and honorable between man and man. Indirectly, it may affect the patient. When *A* or *B* next go away, each will probably employ a paid *locum tenens,* in whom the patient may not have the same confidence as he has in either *A* or *B*.

Not only has a practitioner a right to select his patient, but also to select his consultant, or, rather, to refuse to meet a particular consultant. It is a right which should be seldom exercised; but I agree with Dr. Maclaren when he says, "It is not difficult to conceive of circumstances in which the request to meet some individual amounts to an insult, and it should be dealt with as any such occurrence would be in ordinary social life." It is generally accepted in this country that homœopaths should not be met in consultation. According to Saundby, it is the custom in England for consulting surgeons to meet

homœopaths. He also raises the question whether the profession should not reconsider its attitude to homœopaths. He suggests that "boycotting for differences of opinion is unworthy of a profession based on natural science, in which conflicts of opinion are the inevitable condition of progress." I take it, however, that the objection to meeting homœopaths is based on ethical grounds —not on differences of opinion. It is because no man who regards medicine as a science, and has grasped its scientific principles, can honestly say he believes in the principles enunciated by Hahnemann. Huxley, in his Lectures on Evolution, said—"Scientific men get an awkward habit—no, I won't call it that, for it is a valuable habit—of believing nothing unless there is evidence for it; and they have a way of looking upon belief which is not based upon evidence, not only as illogical, but as immoral." Homœopathy is, as the Royal College of Surgeons of Ireland states in its by-laws, a deception. A practitioner can only assume the designation of homœopath for reasons which are, therefore, unprofessional, unethical, and immoral.

Professional secrecy may, at times, lead to ethical difficulties in practice. As a rule, the obligation of the ancient Hippocratic oath is still binding—"Whatever in connection with my professional practice, or not in connection with it, I see or hear in the life of men, which ought not to be spoken abroad, I will not divulge, as reckoning that all such should be kept secret." The law as to professional secrecy is most stringent in France, and there a medical man is in no difficulty as to his position. Under no circumstances, not even as a witness in a criminal case, can he reveal a professional secret. The only exception is when a crime is about to be committed, and when, by keeping silence, the medical man might make himself an accessory. When a crime has been committed, a medical man must not give information, if, in so doing, he reveals a professional secret, not even when an innocent person is wrongfully accused. In great Britain there is no hard and fast law. While the Hippocratic oath is binding in ordinary circumstances, a medical man can be compelled to reveal professional secrets in a court of law. It was laid down by Mr. Justice Hawkins, in the case of *Kitson* v. *Playfair*, that no medical practitioner may state facts which have come to his knowledge in the course of medical examination without the consent of the patient, except in a court of justice, and under the direction of the judge, who has to decide each case on its merits. He is supposed not to allow unnecessary disclosures, but to see that no material facts which can assist the court in arriving at the truth are withheld. In Australia, the law of evidence in regard to professional secrecy differs in different States. In New South Wales and Tasmania it is the same as in England—the medical man is not protected. In Victoria he must give evidence in criminal cases, but shall not in any others. Section 47 of the "Evidence Act" reads—"No physician or surgeon shall, without the consent of his patient, divulge in any civil suit or proceedings (unless the sanity of the patient be the matter in dispute) any information which he may have acquired in attending the patient, and which was necessary to enable him to prescribe or act for the patient." While this is the law, and notwithstanding the Hippocratic oath, occasions arise in which, I think, one is justified in taking all risks and communicating professional information concerning patients. This is especially so with regard to communicable disease. Some time ago a lady saw me on what she described as a delicate mission. A patient under my care was engaged to be married to a friend, who was an orphan. The lady believed this man to be dissolute, and suspected that he was being attended by me for a communicable disease. Would I tell her? I declined, on the ground of professional secrecy. I sent for the patient, and told him he must break off his engagement. He declined. I then said I must advise the girl's friends not to let her marry him. He objected to my doing so. However, I took the risk. I told the lady that, while I could not reveal the nature of the disease from which my patient

suffered, I could tell her that he ought not to marry. Even in France, eminent authorities on legal medicine and ethics, as Tardieu and Garde, say that, in similar circumstances, "conscience must be heard above law."

I had intended to refer to some other matters, such as the relations of the profession to the public press, and also to the friendly societies. I fear, however, that I have overtaxed your patience already. I will, therefore, conclude by reminding you that, while the profession undoubtedly has many just grievances, there ought to be manifold compensations, at least to those of its members who "have a right spirit within them." Let me remind you also that much has been done to remedy these grievances by proper organisation, and more can yet be done. Our brethren in New South Wales have shown us the way, and it is to be hoped that the organisation of the whole profession, so satisfactorily achieved in New South Wales, may be extended throughout Australasia. It is to be hoped, further, that its aim will not be simply the selfish one of the material improvement of the profession, but will be primarily to promote and maintain the best traditions of the past, and to afford inspiration for scientific progress in the future. Let us not forget, amidst all our failures and disappointments, ethical and otherwise, that, in the eloquent words of Sir James Paget, our profession unites "those three qualities which have the greatest charm for pure and active minds—novelty, utility, charity," and that we can compete in the world, not for wealth and position, but "in the nobler ambition of being counted among the learned and the good, who strive to make the future better and happier than the past."

Now I am at last ended. You may probably say—"There needs no visitor come from distant State to tell us this, such poor obvious commonplace." I can only reply, like Hamlet—

"Why, right; you are i' the right;
And so, without more circumstance at all,
I hold it fit, that we shake hands, and part."

FRIENDLY SOCIETIES IN NAPIER, NEW ZEALAND.

By T. C. Moore, M.D., Univ. Dublin, Napier, New Zealand.

I have been asked by Dr. A. A. Hamilton to give some information to the Congress with regard to club practice in New Zealand. I am afraid I can only speak for my own immediate district. For some years I have done very little of this class of work, only holding one lodge, of the Druid order, for which I am paid 16s. per member per annum, without medicine. None of the members of this lodge are in such circumstances that they could afford to pay private fees. They are charged, also, 5s. a mile over three miles if they live beyond the three-mile limit—for these mileage payments the lodge is not responsible—also the ordinary fees for confinements and anæsthetics. All difficult operations are performed in the public hospital, to which the lodge subscribes £5 a year. All the other town lodges are held on much the same terms. The Oddfellows, M.U., many of whom are in good positions, and unfitted to be in a benefit society, pay their doctors £1 a year for attendance, and are dear at the price, in my opinion. There is little competition for lodges here, and the medical men who attend Foresters' lodges have lately raised their terms to £1 a year. The other small lodges pay 15s. a year. There is a dispensary here, started by the friendly societies, where they procure their medicines. There are country lodges at the villages around, which have various agreements, the usual one being 5s. a visit, in addition to a retaining fee of £1 a year.

HOSPITAL ABUSE.

By H. W. Bryant, L.R.C.P. and S., Edin., &c.

In the report of the Inspector of Charities of Victoria, ending June 30th, 1904, he states that "Greater care is now being exercised by some of the managing bodies to see that patients who are suitable cases for hospital treatment contribute according to their means."

Now there is no doubt that this is so, for I am emphatically informed that at the Melbourne Hospital wardsmen and nurses are specially instructed to see that both in-patients and out-patients should pay something towards the upkeep of the hospital; and the very people for whom the charity should be available are the very ones that this system presses upon most harshly.

The other type, who are only too willing to pay a small sum, flood the hospitals, and easily obtain admission and gratuitous medical attendance, to the great detriment of the poorer classes.

The inspector says "that from the declared particulars furnished by each applicant for treatment, the officials of the institution should be in a position to determine whether such applicant is a proper case for treatment, and too much care cannot be taken against imposition."

I could give you case after case of imposition on every hospital about Melbourne that has come to my knowledge during the last two years. Perhaps it would not be amiss to specify a few instances, for the benefit of the profession generally:—

A well-to-do tradesman, worth at least £5,000, sent his wife to the lodge doctor (A.N.A.) for a note to take his child to a hospital in order to have post-nasal growths removed. The doctor, on making inquiries, found that the lady had already been to the hospital, and the Resident, finding the people were on a lodge, refused to treat the child without a letter from the lodge doctor. The lodge doctor said to the lady, "Do you understand that this hospital is only intended for poor people?" and she replied, "Mrs. So-and-so, who is quite as well off as we are, had her child operated upon there, and I don't see why my child should not receive attention there also." These people were very indignant at being refused a letter, and finally got the child operated upon at that hospital by subterfuge and lying.

Dr. J. P. Ryan, in his presidential address to the Medical Society of Victoria, published January 20th, 1903, in the *Intercolonial Medical Journal*, drew attention to the number of hospitals, &c., in the State receiving Government and public aid, and I think it worthy to note that for the year ending June 30th, 1904, the Inspector of Charities for the State of Victoria gives the following list:—

Hospitals (special)	41
Convalescent homes	2
Hospitals and benevolent asylums	9
Benevolent asylums	6
Orphan asylums	7
Other asylums	3
Refuge and rescue homes	10
Ladies' benevolent asylums	70
Medical dispensaries	2
Philanthropic associations	14

All of these are receiving Government grants, besides public subscriptions and donations.

Dr. J. P. Ryan also drew attention to the number of benefit societies in every town in the State, whose members obtain medicine and medical attendance for their families and themselves for a very small sum.

Dr. Barrett gave the result of his researches into this question some time back, and he came to the conclusion, from carefully worked out statistics, that our population was being pauperised. Our hospitals were being used by patients who were quite capable of paying for their medical attendance, but who would not do so when they could get it for nothing. He considered that there was no necessity to increase the hospital accommodation, which was quite large enough in comparison to the population, but rather to look more closely into the position of the patients applying for charity.

There is a general feeling at the present time amongst the profession that something should be done to try and stop the impositions that are being made upon our hospitals.

Many of us have been consulted by well-to-do people with regard to some operation, and when told that it would cost, say, £10, or £15, which they could easily afford, they have gone to one of the large hospitals and been operated upon by one of our leading surgeons for nothing. The same surgeon would probably charge from £30 to £50 for the same operation outside.

We have all been told of cases by the patients themselves where they have gone to the paying ward of one of the hospitals, and have, by paying a small sum for their maintenance, obtained gratuitous nursing and medical attendance. They have afterwards expressed the opinion that, having given a small sum to the funds of the hospital, they were under no further compliment or obligation to that institution or its staff, and they feel they have highly honored both by their patronage.

The general public are thus becoming more pauperised every year, and it has been the feeling of many medical men for some time past that some great effort must be made to bring matters to a crisis, in order that they may obtain a little fairer treatment for the future. The general practitioner knows that small operations, such as the removal of post-nasal growths, curettes, &c., are performed in the hospitals for patients who are too mean to pay for them privately. There is no doubt that such cases should not be attended at hospitals, unless the subscriber sending them in, or the general practitioner. can positively state by letter that they are destitute. This would save a lot of the dissatisfaction at present felt by general practitioners, and would also be a great saving of the voluntary contributions which are given in the belief that only the poor are benefited.

It is quite time the committees of the hospitals took more active means than are at present adopted to curtail their expenditure and improve their positions and efficiency.

By properly investigating into the positions of people who are attending at the hospitals, a great saving might be made, and the legitimate poor get their proper share of attention ; but frequently we hear that the hospitals are overcrowded, and poor people have to be carted about from place to place before they are admitted. Then we hear that a member of Parliament is found by a policeman close to his home, suffering from a broken leg, &c., and instead of taking him to his home, and sending for a doctor and a nurse, or even taking him to a private hospital (of which there are a plentiful supply in all the suburbs of Melbourne), he is taken to a place where only the poor are supposed to be attended, and kept there until his leg has knitted.

I should not object so much if he were temporarily attended to, and then sent to his home or to a private hospital ; but no, he is kept there until he is quite fit to be moving about again.

Another common practice is much to be deplored, viz., the sending of crushed hands and other injuries occurring at big factories and Government workshops to the nearest hospital.

In these days of telephones, it is always easy to obtain a medical man at very short notice, and I consider that a first aid should be provided for at all big institutions, and paid for by a fund initiated for such purposes.

The medical man called in to such a case could then inquire into the circumstances of the individual, and, if poor, he could go to the hospital, but if in a position to pay for outside attendance, he ought to be made to do so.

This would save a lot of work at the hospitals, not to mention the expense in dressings, &c. All these people in Government employment receive a fair wage, and the most of them can afford to pay for medical attendance, and I consider if they are attended to at the time of the accident at the hospital, they can, after this, receive outside attention, and not, as at present, return for weeks for dressings, &c.

In the late President of the Medical Society of Victoria's address (G. F. Howard, B.A., M.D. & Ch.B., Melb.) he has given a very straight opinion, from a medical point of view, of the position of most of our hospitals. He has drawn attention to the abuse of gratuitous medical relief in the hospitals about Melbourne. He points out that the population has not increased of late years, but there has been a great increase in the number of hospital patients ; and he thinks the cause of this to some extent is due to the system of exacting contributions from patients who say they are unable to pay ordinary fees. He considers, as we all do, that hospitals should only be used by the destitute classes. He quotes the following extracts from the Police Offences Act, 1890, section 41, subsection 3 :—

" Any person who shall commit any of the following offences shall be deemed a rogue and a vagabond within the meaning of this part, and be liable to the punishment next hereinafter specified."

" Any person imposing or endeavoring to impose upon any charitable institution or private individual by any false or fraudulent representations, either verbally or in writing, with a view to obtain money or any other benefit or advantage, shall be liable to imprisonment in any gaol with hard labor, for any time not exceeding two years."

This ought to be printed in very large letters in and about every one of our hospitals, and also on the back of all forms of admittance either to the in or out patient departments, and a few prosecutions would clear the atmosphere wonderfully.

Dr. Howard says that practically the whole of the classes from whom contributions are now being exacted ought to be in friendly societies, or similar institutions.

He summarises the objections to the pay system as follows :—

(1) Exclusion of deserving cases from the hospitals by paying patients.

(2) Tendency to pauperise the section who really know they are being treated gratuitously.

(3) Patients paying fancy they are paying full value for services rendered, when they are paying nothing for the medical attendance.

(4) It leads to the underselling of the outside medical practitioners, whose lot is assuredly hard enough at present.

(5) The conversion of a charitable institution into a trading institution.

(6) Unsatisfactory examination into circumstances.

At the present time everything is being done by hospital committees to pander to the pauperisation of the public.

Paying wards in public hospitals must be done away with *in toto*.

Strict investigation into the circumstances of each patient attending the in or out patient department, and a fixed wage of the individual, or collectively of the family, should be settled upon.

In the *British Medical Journal* dated October 29th, 1904, a brief report of the proceedings of the hospitals committee contained references to a memorandum on the principles of hospital management and hospital letters that might be wisely applied to the management of our hospitals in Australia.

The first difference of importance was "that an investigating officer shall be appointed, whose duty shall be to inquire into the eligibility of all applicants."

The following certificate, which has to be signed by all subscribers to the Royal Victoria Hospital, Bournemouth, seems an admirable idea :—

"I certify, having satisfied myself as to the circumstances, that A.B., ..., aged ...., occupation .... residing at ...., is a fit and proper person to receive the benefits of the charity."

The front of the letter also contains the two following statements :—

"The out-patient department is only intended for those who are too poor to pay for medical relief."

"In any case where the subscriber is in doubt as to the circumstances of the patient, please write across the letter 'For inquiry'"

On the back of the letter is the statement that—"Persons earning more than £1 per week, or members of a family, the collective incomes of which family at home exceed 30s. per week, are not admissible to the benefits of the out-patient department."

At this hospital in all doubtful cases a specially trained inquiry agent is employed to visit the home of the applicant and ascertain the circumstances of the case. The out-patient staff are also required to mark "For inquiry" the letter of any applicant who appears not to be a fit case for charity.

The following recommendation was finally drafted and approved by the annual representative meeting on July 26th :—

"That some means of investigation into the circumstances of the applicants for relief shall be employed in all medical charities, and, where possible, a special officer shall be appointed for this work."

These suggestions are the result of careful investigations in the mother country ; but what about the results of investigations into the same subject in Australia ?

The number receiving charitable relief for the year ending June, 1904, was 139,771 ; being 4,830 over the number assisted in the previous year.

Every year more medical practitioners are getting their diplomas, and our population seems at present to be at a standstill. The lodges are using all kinds of endeavors to obtain members from the middle and wealthy classes instead of keeping to the poor, and the result of all this means absolute slave-driving for the general practitioner, for a miserable remuneration.

The question then arises, is our population in a state of poverty ? The answer is emphatically no. For Australia pays the highest wages in the world. What are we to do to stop this state of things ? For I fear we have no one to help us but ourselves. In fact we seem to have an enormous opposition to any attempt to ameliorate our positions, and the first thing we must do in order to make a start is to unite.

Let us lay aside all minor differences and come together in one vast union, and thus meet our opponents on equal terms to start with.

By union all our present abuses will be easily remedied, and numerous schemes might be adopted to benefit ourselves generally and the community as well.

Dr. J. P. Ryan has pointed out that half a century ago a public hospital was only used by those who were in much need of help, and it was avoided by all who were not more or less destitute; but—partly owing to the part-paying system adopted—this idea is passing away, and the profession is suffering very seriously for its good nature and foolishness in days gone by.

With reference to the discussion that has been raised lately about the representation of the medical staff of a hospital on its committee, it seems to have caused great anxiety in certain quarters of the community, and fears have been expressed that it would be a mistake.

This matter was thrashed out some time ago in the case of the Women's Hospital, with the result that the hospital has gained in efficiency, and an absence of the interminable quarrels that were once so characteristic of its management. It is well known that in almost all the hospitals of Great Britain the honorary staff is represented.

According to Mr. Butters, one of the committee of the Melbourne Hospital, it is impossible for a gentleman on the honorary staff to also occupy a position on the committee without making use of the latter position in order to gain advantages in the former. He drew attention, on the morning of the election, to Dr. Moore, who occupies this dual position, having used his beds for his own nominees. He speaks of ungenerous and uncalled-for remarks when they are applied against himself; but how does this statement of his compare with his action in stating certain so-called facts in a public paper on the morning of an election, when there is no chance of their being denied, justified, or replied to? Why men should be on a hospital committee who know absolutely nothing about the internal organisation of such a place, and who refuse to take the advice of those whose lives have been spent in studying such subjects, passeth all understanding. Nevertheless, all efforts at reform must be crushed with a stern hand; expert knowledge must not be allowed on hospital committees, and abuses must go on to the end of the chapter, keeping our profession down to its lowest point, using its services gratuitously, and even contemptuously, and curtailing its sources of income in the most barefaced manner.

Suggestions for consideration—

1. Union of the profession.
2. An investigating officer attached to each big institution, and one for, say, three or six of the smaller ones, with power to summon any people abusing the charities; their duties being to investigate into the circumstances of each case at the homes of such cases.
3. A wage limit to be fixed for a single individual and for families.
4. To do away with all part payments, and only use the hospitals for the destitute.
5. The adoption of dispensaries conducted on similar lines to those in Edinburgh, which would come in between the hospitals and the lodges.
6. The establishment of private hospitals on cheaper lines than at present, charging sums sufficient to pay for the up-keep of these hospitals, and within the range of expenditure of the working classes; any medical man who is desired by that patient attending on cheaper lines than ordinary. This would have to be guided by a wage limit also.

A FEW STATISTICS FOR THE INFORMATION OF THE PROFESSION.

Number receiving charitable relief in Victoria—

For year 1895	115,346
1900	118,412
1901	127,037
1902	127,860
1903	134,941
1904	139,771
1905	—

POPULATION, NUMBER OF REGISTERED MEDICAL MEN, AND MEMBERS OF FRIENDLY SOCIETIES IN VICTORIA.

Year.	Population at End of Year.	Number of Registered Medical Men.	Number of Members of Friendly Societies.
1884	944,564	621	58,859
1894	1,182,290	1,051	80,604
1904	*1,207,605	1,265	†102,040

* On September 30th. † Figures for 1903.

DISCUSSION.

DR. G. T. HOWARD (Melbourne) said that it was a waste of time to discuss the question whether there really was hospital abuse. About one form of it, by people well able to pay ordinary medical fees, there was practically no difference of opinion. Impostors of this class ought to be prosecuted; but owing to the inadequate inquiry into circumstances they escape at present unscathed. The real difficulty is with the class not destitute and not able to pay ordinary medical fees. One way, apparently in the line of least resistance, the hospital managers thought, was to admit them and make them pay something. This innovation, plausible enough in its way, threatens to revolutionise the relations existing between the profession and the poorer classes of the community. The more of these contributing patients (who fondly imagine that they are paying for what they get, while really being treated gratuitously) there are in the hospital the worse it is for the destitute sick, for whom such institutions are intended. It must inevitably happen that deserving destitute are turned away while beds are occupied by contributing patients. This is the point to which attention must be persistently drawn. Most of them ought to be in friendly societies, or in provident dispensaries, like those of Sydney or Broken Hill. These latter institutions were on the right lines, but needed a peculiar combination of enthusiasm, perseverance, and tact and patience, very rare, and as a rule badly appreciated. Those of them needing hospital treatment should be in hospitals instituted and kept up by the benefit societies. The points the profession should insist upon were much closer inquiry into patients' circumstances by some extra hospital agency—preferably charity organisation society—and that public hospitals were for those absolutely unable to pay even the most modest fees.

In addition they ought to use every opportunity to impress these views on hospital managers, and, above all, they ought to organise until their social and political influence was much greater than at present.

THE ETHICAL RELATIONS BETWEEN TOWN AND COUNTRY PRACTITIONERS.

By Frank Allwork, L.S.A., Lond., New Norfolk, Tasmania.

An experience of upwards of twenty years in "unopposed" country practice in Australasia, and of the many accompanying disadvantages arising therefrom, must be my excuse for calling the attention of my confreres to this subject.

I have purposely avoided the use of the term "consultant," because where my lot has been cast it must be admitted to be inapplicable, except when used in connection with one who is purely a specialist. It is well known that in the great majority of cases the medical men in colonial towns are, both of necessity and possibly also of choice, in every sense of the term, general practitioners. How few (comparatively) honorary surgeons to our public or charitable institutions will, in private practice, decline to attend midwifery cases, or honorary physicians refuse to operate on a surgical one. Now it is especially the relations between the country practitioner and these men that I wish to deal with; and let me make it quite clear that, with some important and disreputable exceptions, I am not charging them with, or even wishing to impute to them, dishonorable motives; but that there is a great want of thought, tact, and discretion often displayed by them when they are consulted by a patient from the country, to the detriment of the patient's own adviser, is a fact well known to, and sometimes bitterly experienced by, the lonely man in the country. Let us for a moment consider the position and surroundings of such an one, as compared with those of his more fortunate town brother. He is morally bound to attend all cases to which he may be summoned, under any and every condition as to distance, weather, hour, or financial means of the patient. His summons is often of the most vague description, especially as regards accidents. His opportunities for arriving at a diagnosis (which it is generally insisted shall be immediately given) are frequently limited to a single or perhaps two interviews, probably under the additional disadvantages of a candle or bad lamp in the middle of the night. For surgical or midwifery cases the nursing assistance is limited to the nearest neighbor, or, at most, a handy woman; the sanitary conditions, bedding accommodation, and culinary arrangements are generally deplorable. All his thinking has to be done under the most adverse circumstances, it may be at the termination of a long journey; nor has he the opportunity of discussing dubious points with a friendly neighbor. He must also be somewhat of a general specialist, and certainly possess a mind capable of adapting itself to peculiar circumstances and making shift with most primitive appliances. Added to all this, the very term "unopposed" means "unassisted"—even by a competent nurse. Finally, the patients of the town man employ him of their own free will—he is of their own choice. What an enormous preliminary advantage this is. On the other hand, the country man is employed *of necessity;* there is no one else to go to, and possibly some prejudice, personal or professional, is held against him; the friends, and it may be the patient himself, are prepared to differ from him in every possible way, even in some instances wilfully disregarding his instructions or advice; the case may present such features as to make a second opinion desirable, if not actually necessary. Out of consideration for the patient's slender means, or not wishing to entail a journey with a doubtful fee upon a professional neighbor, the practitioner hesitates to suggest this too long, when, if able to travel, he finds as his reward the patient has been taken to town to see Dr. So-and-so, who is well and favorably known to his first cousin. The diagnosis, line of treatment, and general opinion of the case may in every

respect be the same; but the unfortunate employment of different terms conveys a very different meaning to the lay mind, and apparently reflects upon the judgment and skill of the man who has borne the heat and burden of the day by attending a case a distance from home at great personal physical and mental expense. Such and many similar circumstances prevent the customary letter, introducing the case and giving the previous history, from being sent. Yet how great are the possibilities in the hands of the second practitioner for preventing misunderstanding and mischief were he, before pronouncing himself very decidedly, to communicate with the original attendant, to whom the patient will, under the circumstances, probably sooner or later return. Would he not thus be acting more in the interests of the patient also? If a similar sequence of events occurred in town, it is scarcely to be conceived that some communication, either personal or by telephone, would not take place. Why not extend this courtesy to a colleague in the country, even at the cost of a little more time? Such instances are, doubtless, due to thoughtlessness. Want of tact and discretion in giving a positive diagnosis, without a previous history, in a promiscuous case from the country, are mistakes which sometimes score to the credit of the country man, but not till misunderstanding or mischief, which generally could be avoided, has been provoked.

One cannot but regret, also, the disposition sometimes shown by young medical officers of our hospitals to unthinkingly disparage the work of the country practitioner, without having the least conception of the difficulties of private practice in general and unopposed country practice in particular.

In conclusion, I would like to testify to the fact that these violations of ethics of which I speak are rather the exceptions than the rule, but are, in my opinion, and I believe, in the opinion of others, of sufficient frequency to justify attention being drawn to them, with the result, I hope, that those men who have the greater opportunities may be more liberal-minded towards those who are less favorably circumstanced, and, by more courteous communication, help to diminish their many disabilities. This might, with advantage, even be extended into a friendly discussion of cases of mutual interest, though such be only of an every-day character, especially when we take into consideration the fact that rural men are generally debarred from the possibilities of attending the meetings of any medical society. The effect of such action would be to not only raise these men in the esteem and gratitude of the "unopposed" practitioner, but to elevate the general standard of professional ethics.

THE TAKING OF PAYMENT FROM HOSPITAL PATIENTS.

By Dr. W. Moore.

The subject that I have chosen for my paper is one of great importance, but it has only become obtrusive in quite recent years. As yet the medical profession has devoted but little attention to it; and, so far as I am aware, very little has been written upon it.

Though of only recent origin, this evil, for such it must be regarded, has already become very wide-spread. In Victoria the practice is, I believe, universal; indeed, a former Treasurer commanded it as a condition of receiving the Government grant. But even before this the evil was wide-spread. In New South Wales and Queensland the practice is very prevalent, probably just as prevalent as in Victoria; and I presume that here in South Australia, and

in the other States, it exists. It has grown to such an extent in England that recently the Hospitals Committee of the British Medical Association met a number of hospital managers, and discussed the matter with them.

I think at the Melbourne Hospital the practice of taking payment from patients is met with in about as bad a form as is possible; and I shall, therefore, devote a little time to describe its mode of origin, and its extent at the present time. Like many evil practices, it started in a very insidious way. Some years ago it was considered by the authorities that certain very undesirable people made use of the warmth and comfort of the out-patient department, and used it as a resting-place. It was thought that, by making all out-patients pay a shilling registration fee, these undesirables would be kept away, as they would prefer to keep their shillings to provide themselves with beer. It was soon found that even this shilling fee brought in a considerable sum of money, and so the principle of payment was extended, and all out-patients were asked to pay a certain sum per week for medicine and for treatment; and, of course, it is very easy to see how the principle was still further extended, and in-patients were next asked for payment.

Recently the Finance Committee of the Melbourne Hospital furnished a report on this matter. It stated—"In the Melbourne Hospital the compulsory contribution system has been found satisfactory. There is a special officer appointed to make the necessary inquiries into the circumstances of patients, and to collect their contributions with tact and humanity, and in no case to exact more than a reasonable amount in proportion to the patient's means."

During the year ending June 30th, 1904, 164 out-patients paid 5s. a week; also from out-patients contributions of 10s. to 20s. had been received, these sums amounting to £31 13s. Among in-patients, 168 paid from 10s. to 20s. a week, and as many as 95 paid over £1. It wound up with the following statement:—" In no case, apart from casualties, has a patient been able to contribute such sums as would have obtained suitable private medical attendance, or treatment in a private hospital; and the Finance Committee are of opinion that the system of requiring contributions from patients in proportion to their means is just and proper in order to provide for the large number of patients who, though not absolutely indigent, are unable to pay the ordinary medical fees."

For the year ending June 30th—	£	s.	d.
1901—In-patients' contributions amounted to ..	1,699	0	0
Out-patients' " " ..	1,216	4	4
1902—In-patients' " " ..	1,608	14	1
Out-patients' " " ..	1,144	6	3
1903—In-patients' " " ..	1,560	9	3
Out-patients' " " ..	1,074	4	6
1904—In-patients' " " ..	1,464	8	7
Out-patients' " " ..	1,198	11	0

It will thus be readily seen that not only are the lay managers of the Melbourne Hospital strongly in favor of taking payment from hospital patients, but that very considerable sums of money are obtained in this way.

In his report for the year ending June 30th, 1904, the Inspector of Charities in Victoria, who has been and is a strong and most persistent advocate of the practice of exacting payment from patients, says, " The amount obtained from patients under the form of admission ticket last year was £19,627, which was an increase of £1,211 over that received in the previous year.

" Greater care is now being exercised by some of the managing bodies to see that patients who are suitable cases for hospital treatment contribute according to their means.

"When the new form of ticket was introduced a few years ago, I stated that, even under a lax administration, at least £20,000 per annum should be obtained from this source towards the maintenance of our medical charities. Although that amount was practically reached last year, yet the administration in regard to the ticket still continues to be lax in many of the hospitals.

"If the provisions of the ticket were carried out in their entirety, the revenue to hospitals from this source should reach £30,000 per annum at the very lowest."

It is thus seen that not only is the practice prevalent, but that it is strongly supported by hospital managers and officials.

It may very reasonably be asked—and I have heard the question asked by medical men—why should hospital patients not pay what they can afford? And I must admit that it is a question that cannot be answered in an off-hand way. At the same time I am satisfied that very good reasons can be given why public hospital patients should not be asked to pay.

In the first place the medical profession cannot too strongly insist on the fact that public hospitals are maintained for the treatment of the sick and injured poor; for the treatment of those who are so poor that, under the added misfortune of sickness or injury, they are dependent for the treatment necessary to bring them back to health again on their better-off fellow citizens. It is for the relief of such cases that physicians and surgeons give their services, not for the relief of those who can afford to pay for treatment. Last year, among other recommendations to the Council of the British Medical Association by its Hospitals Committee were these:—

"That poverty and sickness be the considerations for the admission of all patients for hospital treatment.

"That no charge for the treatment of any patient shall be made, though voluntary contributions may be accepted."

If money is asked for and given, then the patient not unnaturally thinks that he is paying, and paying adequately, for what he gets—not only for his medicine and his food and nursing, but for his medical treatment: and seeing that he is paying for it, there is no reason why he should not get it at the hospital as well as anywhere else. He, too, recommends his friends to go to the hospital and get the same excellent, but cheap, attendance. Thus the constituency of the hospital is greatly increased, and the increase is of the very class that should not go to hospital. Of course the reply is made that inquiries are always made into the circumstances of every patient. The report of the Finance Committee of the Melbourne Hospital, above referred to, says, "There is a special officer to make the necessary inquiries into the circumstances of patients, and to collect their contributions. It admits that this important investigation and the collecting of the money is handed over to a subordinate, and I maintain that, under such circumstances, any subordinate will be certain, in order to show his importance and to please his employers, to raise as much money as he possibly can. He will naturally tend to do two things that are wrong—(1) to take money from those who can pay him the larger sums, such as 5s., 10s., or more a week for out-patients, and 15s., 20s., or more a week from in-patients, without inquiring too closely into their ability to pay ordinary fees for medical treatment; and (2) to demand some payment, even though it be small, from people who cannot afford to pay at all.

That these two errors are freely committed, I am quite certain. Here are some proofs. A woman under my own care I found possessed £1,000 or £1,200 worth of property of her own; her husband had a fair suburban business, and money in the bank. The hospital knew of most of these facts, and yet the patient was taken in. Again, I have a cutting from a newspaper saying that Mr. G., who died in the Melbourne Hospital, left £450 to be divided equally

between the Melbourne and Alfred Hospitals. Only the other day a distinguished member of this Congress told me of a case where a colleague, who has a private hospital of his own, sent a patient to a public hospital to be operated on. Among papers in the pocket of this man, who said he could not pay for treatment, were deposit receipts for £1,400. Another case occurred at still another hospital, where the surgeon, after he had done the operation, learned that the patient's husband was very well off. He told the patient that she should give a donation to the hospital. This she refused to do, saying that her husband had no money, and, as a reason for his having no money, made the statement that he had recently bought the farm adjoining his own. I know of numbers of such cases, and I am sure that most of you know of them too.

But it may be asked, did not such cases occur when payment was not sought, and would it not occur now if payment were not demanded? They certainly did occur when payment was not sought from patients, but I think to a far less extent than now; and in those days there was a little more excuse than now. There were then practically no private hospitals, and the public hospitals were the only places properly equipped for dealing with cases requiring serious operations, and no doubt a certain number of such patients, who could have paid medical fees, did go into public hospitals. But the conditions are very different now, when better treatment can be given outside than in the large hospitals. And I am perfectly certain that numbers of people who would never dream of trying to get their medical treatment for nothing are quite content to go and pay £1 a week, although they could easily pay for nursing outside and for medical attendance as well. It is my opinion that many patients who pay for their treatment in private hospitals are less able to do so than are many whom we treat as objects of charity in our public hospitals. It frequently happens that patients sent to me by other medical men, who state that they are fit subjects for treatment in a public hospital, begin to make inquiries about the cost of having their treatment carried out in a private hospital; and a very considerable number of them eventually decide to be treated privately. Thus I am sure that numbers of patients who could afford to be treated privately in a nursing home, or in their own homes, find their way to a public institution, where, in return for the payment of a small weekly charge, they receive board, nursing, and medical attendance.

Now in regard to the second error—the demanding of payment from people who cannot afford to pay anything—I know it will be said that this is not done; but my colleagues and I have met with too many cases to admit of any doubt on the matter. We have been told by patients that they have not been to hospital for some weeks, when they should have been, because they had not the shilling to pay. Then it is quite fair to assume that for every such case that comes under the notice of a member of the staff there will be several that are not seen. Again, it has happened that an out-patient has been told that she should have plenty of milk and nourishing food, and she has asked in astonishment where she is to get them; and yet it has been found that she was paying 2s. 6d. a week to the hospital. These, perhaps, are extreme cases, but I also maintain most strongly that many of those who can apparently pay 5s., 10s., 15s., or 20s., or even more, suffer very great hardships in consequence of that payment being exacted. For instance, in one case a mother was paying 30s. a week for her child who had typhoid. She was earning her living and that of her children by teaching and in other ways. To enable her to do this she kept a servant; but through having to pay this sum to the hospital she had to dismiss the servant and do her housework as well as her other work, and in consequence her own health

broke down. In another case a man's sick-pay from his lodge was taken whilst he was ill with typhoid. He went out very weak, lost his situation, all his money was gone, and he was stranded.

I am convinced that thus in the great majority of cases an injustice is done in demanding payment from patients at a public hospital, whether out or in patients. The great majority of those who pay cannot afford to pay. Even the smallest payment is a severe strain on their slender resources, and in great part defeats the ends sought to be attained by the charitable, and tends to frustrate the efforts of the members of the medical staff in their treatment. A small minority, but still in the aggregate a considerable number, could pay for medical treatment, and yet receives it as paupers by paying to the hospital a small sum to cover the expenses of medicine and nursing. These patients also in the case of in-patients exclude others who are fit subjects for charitable relief.

It is perhaps worth while briefly to inquire into the causes of the great development of the practice of exacting payment from hospital patients. No doubt hospital committees have found it increasingly difficult to obtain sufficient funds for the carrying on of their work. The great increase in the number of operations performed, the introduction of anti-toxins and other expensive drugs, &c., have tended to increase expenditure. At the same time the trend of legislation in Australia has undoubtedly had much to do with making it difficult for the hospitals to increase their revenues by private subscriptions. Legislation intended to benefit the laboring classes, who used the hospitals, at the expense of their employers, who for the most part maintain them, lessens the income of our hospitals. And the existence of a heavy income tax on larger incomes, whilst the smaller incomes are quite exempt, has the same effect. Then the straitened finances of our State Governments have caused them, instead of increasing the grant from Government to meet the increased expenditure, to actually in many cases lessen it. Mainly in consequence of this action of the Government the Brisbane Hospital is at the present moment in a serious difficulty, the committee has resigned, and the Government is considering the question of undertaking the management. At the same time, at any rate in the case of Victoria, the Government, through the Treasurer, has practically insisted on patients being made to pay what they can.

These seem to be the main causes, and this has to be borne in mind from the hospital managers' point of view. At the same time I am sure that the medical profession should strenuously maintain the principle that no payment whatever should in any case be asked for, and even the taking of voluntary contributions from patients should not be in any way encouraged. If on such a matter the medical profession were agreed, I believe they could get their views adopted. In all hospital matters the medical profession, if certain of its own ground and unanimous in its opinion, could have its own way.

If no fees were asked for, and if great care were taken to exclude all patients who can pay, the number of patients seeking admission to the public hospitals would be greatly reduced, and the very poor would have a much better chance of being promptly attended to. The greatest objection to treating all patients absolutely free is that there still would be a certain number of patients who could not afford to pay for treatment outside ; but who could, without great hardship, pay something for their maintenance. Now, I maintain that the number of these cases is very small, and that it is steadily diminishing, for the conveniences of private treatment are steadily increasing. It would be much better too that the few patients who could pay a little for their maintenance in hospital should be made a present of everything than that a pernicious principle should be allowed to prevail in the management of our hospitals.

The profession should devote more time and thought to the management of our hospitals, and endeavor to see that they are managed on lines that would prove more satisfactory to ourselves and to the general public.

Not long ago Dr. Thomson, of Brisbane, speaking to me on this matter, said that four different kinds of hospitals were needed—

(1) Hospitals for infectious diseases, into which patients were compelled to go more for the safety of the community than for their own. These should undoubtedly be maintained out of taxation, municipal or State.

(2) Private hospitals for those who can pay.

(3) Public hospitals maintained by the subscriptions of the charitable, and here, unfortunately, by subsidies from the Government. These should be solely for the poor, and no payment should be taken.

(4) There still remains an important section not provided for: those people who cannot pay the recognised fees for nursing and for medical attendance. These should not be provided for in the same institution as the very poor, for the reasons stated. I think that individual effort on the part of medical men and others will easily settle this question; indeed, it is doing so now. If any collective effort is to be made it should be by friendly societies or some such bodies.

MEDICINE AND SOCIOLOGY.

By T. F. Macdonald, M.B., C.M., &c.

Mr. President and Gentlemen—Students of Sociology, more especially those who are at the same time active members of our profession, must have long ago become convinced of the necessity for a reclassification of the main divisions of medicine. Hitherto medicine has been divided into two main classes—curative medicine and preventive medicine. It seems to me imperative that a division must be added, namely, that of philosophic, social, or evolutionary medicine, in keeping with the deep social changes which are everywhere to be discerned in society at the present time.

The question before the medical profession is, what shall be its position when the social changes shall have become complete, and the present system has been altered? The necessity for a thorough study of Sociology must be apparent to everyone, and I propose that a Chair of Sociology be established in every medical school throughout the world, but more particularly in Australia. Every other science has been duly honored by medicine, and Sociology, the latest—but not the least—of the sciences, is entitled to every consideration at the hands of the medical profession. Perhaps, indeed, it is the most important of studies, and although one is loth to add to the curriculum of the already too heavily weighted medical student, at the same time the necessities of the present trend of things make it imperative that some definite step should be taken for the general study of Sociology.

As examples of our ignorance on the subject, I may mention that in 1894 a congress of evolutionists was held in Chicago, and the following questions were submitted for discussion:—

(1) Can the doctrine of evolution, in its sociological aspects, offer wise suggestions for the solution of the grave social problems of our time?

(2) What are the next steps society should take in keeping with the suggestions which evolutionists may offer?

These questions were sent to all prominent evolutionists throughout the world, both in England and on the Continent. I had the privilege of reading some two hundred answers to those questions. In not one single instance was the great principle of mutual aid, so important to a complete conception of the true nature of social evolution, mentioned. So important did the matter appear to me that, at the International Medical Congress in Rome, that same year, I read a paper upon the subject, and drew the attention of the international medical world to the importance of a revision of the whole study of evolution. Professor Lombroso made a point of specially congratulating me upon the happy ideas in my paper, and agreed with me upon the necessity of the medical profession giving more attention to the study of the science of Sociology.

The Malthusian doctrine is responsible for the Darwinian conception of evolution, which, put in a nutshell, means the survival of the fittest according to the law of struggle for existence. Huxley emphasized this law in his famous Cambridge manifesto, and argued that society, in its process of evolution, was fighting nature all the way. Herbert Spencer has elaborated the theory of the struggle for existence in his famous Synthetic Philosophy, covering the whole range of human thought. The deductions from Spencer's philosophy and from the teachings of Huxley go to show that in society, according to this theory of evolution, there must be a perpetual bloodthirsty struggle among the units of society in the first instance, and between combinations of these units in the second, and the larger combinations as represented by societies, nations, and empires. According to this law, national war, competition, and struggle must for ever exist.

The fallacy in the Darwinian conception of evolution is that of half truth being asked to do duty for whole truth. While no one can deny the existence of a natural law of self-preservation among animals which entitles them to every consideration as individuals (and the principle of individuality is undoubtedly one of the first principles of social evolution), at the same time there is another principle of equal if not of greater importance to humanity. It has been established more particularly by Prince Kropotkin, in his views which were elaborated in a course of seven years' exposition in the nineteenth century, namely, the law of *mutual aid* among animals, primitive peoples, medieval nations, and amongst ourselves. Kropotkin has shown that the animals which have survived best in the struggle for existence are the associated animals. He gives excellent illustrations of the mutual aid principles expressed in the development of ants, termites, bees, and birds. He shows how the sparrow has survived the eagle, although the eagle is a more powerful individualistic animal; how cockatoos, by a process of mutual aid, rob orchards and cornfields. They post sentinels who give warning of any danger. The flock stand by each other, and by this means they strengthen the individual chances of survival, and so far as being able to survive as a species, they can all the better survive as individuals. Kropotkin further pointed out that Darwin's statement that the struggle for life is more severe between units of the same species is a fallacy. Even Darwin himself never brought one single instance to prove the statement that the struggle was most severe among members of the species. In all other departments of his work Darwin is famed for his elaborate illustrations of any statements he makes, and this is the one notable instance of his failing to do so.

The mutual-aid principle, however, is well exemplified in the quadrupeds. Beavers exist in societies on terms of complete autonomy. It is simply marvellous how the combinations among the beavers fulfil the function of abstract government in ruling bodies. The beavers by mutual aid and com-

mon action build great dams for common benefit, and mathematicians have been puzzled to account for the accuracy of their engineering. These dams are built of logs, clay, &c., which are obtained by the beavers swimming up streams, gnawing through some tree which might be overhanging the stream, floating it down, and placing it by common effort. Initiative is a great principle among the beavers, as among all the associated animals. Should an ant find a tit-bit too large for itself to carry, it is not long in calling its mates to its assistance. In the same way, when the beaver finds a tree in a good position for falling into the stream, he calls his brethren to the common work of felling the tree, gnawing off its branches, floating it down stream, and putting it in place. Monkeys are well known to live in great tribes, and it is by the principle of mutual aid which they have practised that they derive their intelligence and survival against strong individualistic heavily armed animals. Against combinations of monkeys, the lion, the tiger, the bear, and the eagle, or any other strong individualistic animal, has no chance whatever. It has been described how monkeys in desiring to cross a small river have formed themselves into a living chain by clasping each other and swinging until the last unit of the chain can catch a grip of a tree on the opposite side of the stream, thus forming a natural living suspension bridge over which the whole tribe of monkeys may pass. The reverse action of the chain then takes place, the first unit letting go his hold and swinging towards the other side of the river. A more beautiful illustration of the principle of mutual aid it would be hard to find in the whole range of animal knowledge. The mutual-aid principle manifests itself in its most powerful form among primitive peoples. Man may indeed be said to have attained his position at the head of the animate world because of his social nature and his deep knowledge of the principle of mutual aid and social affairs.

The two principles of self-preservation and mutual aid, then, play their part as a dual number in the development of humanity. They might be called the centripetal and centrifugal forces of society. In its evolution society swings either along the principle of individuality to its extremes, or along the principle of association or mutual aid. By experience society finds its mistakes, when the pendulum swings backwards and forwards, according to what might be called the law of social oscillation. A true conception of the nature of social evolution, then, is impossible without taking into account the full bearing of the mutual-aid factor in social development; but, with the exception of a few ardent students of Sociology, the mutual-aid factor is never taken into account, and we are brought face to face with the impossible problem of attempting to solve social questions upon the struggle-for-existence theory, and we find our efforts in social reform at variance with natural laws.

The mutual-aid factor supplies a long-felt want to humanitarian thinkers, and society has nothing to fear in its evolution if the full force of the mutual-aid theory is taken into account.

Another question which is not understood by thinkers of the present time is the great question of the *unemployed*, and it behoves medical men who look to the future of their profession, as well as to the future of society, to take into account this great question. It seems to me that the unemployed must be looked upon as a permanent feature of the present system of society, comparable in this respect to the criminal, the sick, the indigent aged, and the criminal child. Whenever society makes the discovery of any permanent feature of this nature, it is not long in setting to work to build a permanent institution around it. Hence we find that gaols were invented to meet the criminal question, hospitals were evolved to deal with the sick, reformatories for naughty children, and benevolent asylums for old people in poverty.

At present, society perhaps does not consider that the unemployed question forms a permanent feature in the present social system. I do not propose to enter into an argument on the subject here, but will content myself with appealing to the general intelligence of medical men that such is the case, and that it behoves society to set to work, and as quickly as possible, to build up permanent institutions for the relief, if not solution, of the unemployed question.

I have thought it possible to devise a scheme of social settlements which would embrace workshops of every description, and great tracts of land for agricultural purposes : these settlements to be controlled by enormous associations, which would elect executive committees. The unemployed would be received freely to these settlements. They would be given freedom of choice as to which trade or work they would preferably take up—agricultural or manual trades, such as carpentry, weaving, paper-making, and pursuits of that nature. By this means the unemployed would be quickly regenerated. The profits arising from any work which they would perform would be divided on the co-operative principle, so as to avoid all difficulties about trade union rates of wages. A permanent reservoir, then, of healthy normal labor would thus be built up, and numbers of really useful citizens could always be on hand as occasion arose for their absorption into society. Should there be no immediate demand for such labor, one thing is certain : these regenerated units of society can always be converted into soldiers and sailors, and no one can deny that at the present time Australia is in very great need of such assistance. Drill would be enforced upon all inmates of these settlements, and one of the finest armies in the world could gradually be built up, if required, by means of institutions of this sort.

I instance these two important questions more as an argument in favor of the necessity for establishing Chairs of Sociology at our universities and schools than to ask the medical profession to take up any special or definite action in regard to them.

I would like to submit the following resolution to the Congress, if my colleagues are in agreement with me on this matter, viz.:—That this Conference expresses its belief that it would be to the advantage of the medical profession if Chairs of Sociology were established in all the teaching schools of Australia.

APPENDIX TO STATE MEDICINE, ETC.

THE ORGANISATION OF THE AUSTRALIAN ARMY MEDICAL CORPS ON PEACE FOOTING, WITH ITS EXPANSION TO WAR REQUIREMENTS.

By Surgeon-General W. D. C. Williams, C.B.

Mr. President, Ladies and Gentlemen—A proposition was submitted to me by Lieutenant-Colonel Eames, C.B., as to the desirability of requesting that a Naval and Military Section might be included in the Medical Congress we are now assisting at, and this officer voiced the opinion of a number of the officers of the Australian Army Medical Corps in New South Wales.

The President of the Congress was approached on the matter, and although the movement was not taken in hand early enough to form a distinct section, permission was graciously given to ventilate the question at a separate meeting.

This is the first time in the history of the Naval and Military Medical Services of Australia when distinct attention has been given to this most important branch of the services, and I venture to express a hope that in future Congress gatherings it will form a section of its own, and engage that attention which those conversant with the many and various phases it presents will most readily admit it deserves.

Gentlemen, the honor has fallen on myself to lead the way with a paper, at what I may hope to be really the inauguration of a Naval and Military Medical Service Section, and deeply proud am I of that honorable distinction.

With the brief time at disposal every endeavor will be made to clearly and concisely lay before you the present organisation of the Australian Army Medical Services.

The Australian Army Medical Corps organisation provides for three main conditions, viz :—

1. Departmental—at Headquarters.
2. Field Force Troops in each State on peace and war establishments.
3. Garrison Troops in each State on peace and war establishments.

1. The departmental administration is vested in the Director-General Medical Services, who is responsible to the Military Board for all matters connected with the general efficiency, training, medical and surgical field equipment, transport, &c., of the Medical Services.

2. The composition of the Australian Army Medical Services is as follows:—

   1. Permanent Army Medical Corps, Nucleus of.
   2. Militia Army Medical Corps—Officers' active list and unattached.
   3. Volunteer Army Medical Corps—Officers only.
   4. Army Nursing Service.

3. The Permanent Army Medical Corps do not at present exist in all States. They carry out the medical duties in connection with permanent troops, and act as an instructional cadre in medical duties generally for all arms.

4. In each military district a principal medical officer is appointed, and in the State of New South Wales there is a staff officer medical service in addition.

5. The Militia and Volunteer Army Medical Corps is organised as follows:—

   1. Officers attached to regiments and corps.
   2. Mounted bearer companies.
   3. Infantry bearer companies.
   4. Field hospitals.
   5. Garrison companies.

Certain officers of the Australian Army Medical Corps, Militia, and Volunteer are attached for duty with special regiments and corps. These officers, together with the regimental stretcher-bearers, &c., constitute the regimental medical service.

6. The allotment and designation of mounted bearer companies, infantry bearer companies, and field hospitals for the field force and garrison troops respectively, are as follows :—

1.—FIELD FORCE.

No. 1. Mounted bearer company, New South Wales.
No. 2. Mounted bearer company, New South Wales.
No. 3. Mounted bearer company, Victoria.
No. 4. Mounted bearer company, Victoria.
No. 5. Mounted bearer company, Queensland.
No. 6. Mounted bearer company, South Australia.
No. 1. Infantry bearer company, New South Wales.
No. 2. Infantry bearer company, Victoria.
No. 3. Infantry bearer company, half Queensland and half South Australia.
No. 1. Field hospital, New South Wales.
No. 2. Field hospital, New South Wales.
No. 3. Field hospital, Victoria.
No. 4. Field hospital, Victoria.
No. 5. Field hospital, Queensland.
No. 6. Field hospital, South Australia.

2.—GARRISON TROOPS.

New South Wales garrison company.
Victorian garrison company.
Queensland garrison company.
South Australian garrison company.
Western Australian garrison company.
Tasmanian garrison company.

The medical units of the field force are found by the States of New South Wales, Victoria, Queensland, and South Australia.

The units have a similar allotment in New South Wales and Victoria, and those of Queensland and South Australia also correspond as regards distribution and numbers.

The garrison companies of the Army Medical Corps, organised for duty with garrison troops at the fortifications and with district reserves, vary in strength in each State, in accordance with the number of forts and the distribution of garrison troops.

7. The Australian Army Nursing Service is a voluntary body, formed to supply a nursing service under an organised military system, for duty at base hospitals and stationary field hospitals in times of war.

8. The establishment of the Australian Army Nursing Service will be, until further orders :—New South Wales and Victoria, one lady superintendent, one matron, and twenty-four nursing sisters in each State ; Queensland and South Australia, one lady superintendent, one matron, and fourteen nursing sisters in each State ; Western Australia and Tasmania, one lady superintendent, one matron, and ten nursing sisters ; total, six lady superintendents, six matrons, and ninety-six nursing sisters.

Provision has not, as yet, been made on the Estimates for the nursing service of Western Australia and Tasmania.

9. It is a nucleus of a military nursing service. The nursing sisters undergo annually a course of instruction in the special organisation and duties which are absolutely essential for the conduct of military nursing.

This Australian Army Nursing Service, I am proud to say, is extremely popular, and the keenest interest in the work is shown by all.

10. Reserve of officers. A scheme has been formulated, but has not yet received official sanction, whereby the creation of a reserve of officers could be at once proceeded with.

Briefly, the reserve would provide staffs of consultant physicians and surgeons for duty at base and stationary field hospitals, and, from the junior members, additional *personnel* for bearer companies or field hospitals, or even hospital trains if required.

The scheme would not cause any dislocation of the working of the Government or civil hospitals in military districts. Much additional accommodation would inevitably be needed, to meet the requirements of sick and wounded sent down from the front, in whatever State or States that might be, entailing occupation of suitable buildings in hospital vicinity, or the erection of tents in hospital or adjacent grounds. This would naturally fall on the military authorities to carry out, and if in time of peace not only were the *personnel* for hospital duty duly allotted, the routine of official duty drawn up and requisitions prepared for all medical stores and equipment, there would not be any hitch when the pinch came; many valuable lives would be saved; and that useless expenditure of money avoided which hurried and hysterical organisation invariably brings about.

I feel certain, gentlemen, when the reserve system receives official approval that it will commend itself to a great number of the profession, who, although not now holding commissions in the Australian Army Medical Corps, would only be too glad to render patriotic service in time of need, and give their services on the lines indicated. The want of some such reserve, properly organised in peace time, was severely realised by the Imperial authorities when the strain on the *personnel* of the then existing medical services began to be felt.

11. The question of bringing into line the voluntary aid societies of the several States as organised bodies, to leaven in with the medical services in time of emergency, is one that requires early and intent consideration; allusion will be made to this again later on.

12. The establishments of units of the Australian Army Medical Services, both for peace and war, viz., mounted bearer companies, infantry bearer companies, or field hospitals, are organised for service with the brigades to which they are attached—whether light horse or infantry—and form part and parcel of those brigades, and would take the field with them in the first line of transport.

These medical units are capable of immediate sub-division, both as regards *personnel*, equipment, and transport, into two half units, should it be necessary from tactical considerations to subdivide the brigade to which any medical unit may belong.

The establishments, peace and war, of the several units of the Australian Army Medical Corps for the field force are given in Appendix No. 1.

13. There are several differences in the organisation of the medical services of the Imperial Forces and those of the Commonwealth. Prior to the new organisation of the Royal Army Medical Corps, which came into effect in the early part of this year, the main points of difference were :—

1. That the Imperial Service did not provide for any peace establishments of units—only a war establishment—with certain small additions of *personnel* when serving abroad. I have always held that a peace unit, complete in itself, gives every opportunity for

a high-class and efficient training, as it is possible to carry out exercises, duties, and routine on a reduced scale, precisely as they would be on a war footing.

2. The next departure in the Commonwealth service is that whereas, in the Imperial Forces, the Royal Army Medical Corps are dependent on the Army Service Corps for their ambulance wagons, cart transport, horses, harness, and drivers, in the Australian Medical Services everything connected with its transport is part of the corps, and each unit has its own distinct allotment. It is thus self-contained, a dual command is avoided, higher efficiency is attained—as even all drivers, in addition to their special qualification, are trained stretcher-bearers and certified in "first aid," instead of being drivers pure and simple, and belonging to another corps, devoid of medical traditions. When the drivers form part of the medical unit, they are on the spot when wanted, and officers and non-commissioned officers know them, and they know their superiors; the *esprit* of comradeship exists, and a unit with a high standard ensues.

Whatever success may have attended the units of the New South Wales Army Medical Corps in the late South African war, I attribute in no small measure to the fact that its transport was self-contained, and a part of the corps.

Another point in which the Commonwealth service has struck out in a new line is the establishment of ambulance depots in the various States for the accommodation of the whole of the ordnance stores, medical and surgical field equipment, ambulance wagons, transport wagons and carts, harness and saddlery, required for regimental services, bearer companies—mounted and infantry—and field hospitals both for the field and garrison forces, in accordance with the allotment of troops in the general scheme of organisation per States.

By this means every officer throughout the medical services of the Commonwealth, whether attached to a regiment or medical unit, will be able—as soon as the remaining depots are constructed, and the supply of equipment and stores now on indent or for local contract is to hand—to draw his material from the compartment in which it is stored. available at a few minutes' notice, simply by signing a requisition, which, in printed form, tabulates his stores, and then join the regiment or brigade to which he may be allotted, fully equipped.

This system applies as equally to bearer companies and field hospitals as it does to regimental equipment.

Another departure is that in every fort throughout our defences casualty-rooms have been fitted up, replete with stretchers, surgical dressings, and a few drugs; and each casualty-room is equipped on similar lines. All the medical officer attached for duty at any fort has to do is to walk in and find all to hand, either by day or night.

14. The organisation which has so lately come into force in the Imperial Service does away with the separate units of bearer company and field hospital, and a combined unit has resulted, known as a field ambulance for use with infantry, and a cavalry field ambulance for mounted troops. This is the first attempt in the Imperial Service to provide a special unit for cavalry: up to now the bearer companies and field hospitals—as used for infantry—have had to do their best with cavalry troops. and a want of mobility has always been sorely felt.

These field ambulances are divided into sections, each capable of acting separately—the field ambulance into three sections, and the cavalry field ambulance into two sections.

The ambulances are made up of a bearer division, corresponding to the old bearer company, a tent division for 150 patients to the previous field hospital, and transport details—which are still drawn from the Army Service Corps.

The *personnel* in these combined units has been increased. The details of the establishment of a field ambulance and a cavalry field ambulance are given in Appendix No. 2.

It will be noticed that in completing the *personnel* that a proportion of the officers required may be civilian surgeons specially employed, and a number of the men to be specially enlisted.

The most important factor in medical services is that of training, which, to be of any value, must be thorough. No detail can be spared, no point missed.

Our officers come into the service with high professional attainments, and many of them in special line of practice, and are given every opportunity to make themselves proficient in the military side of their service, both by courses of instruction and camps of continuous training.

Other ranks have similar facilities afforded them.

The training of men who are required to render first aid in the field, often under heavy fire, the conduct of the transport of wounded by the stretcher-bearers and ambulances, the multitude of duties which are part of the every-day work of hospital orderlies and attendants in field and other hospitals, is, in my opinion, the one point we should ever have in view. Nothing can be more lamentable to conceive—worse than lamentable, almost criminal—than that sick or wounded should be handed over to the care of attendants who, by want of training and experience, do infinite mischief. In the words of the dying King Arthur, " A little thing may hurt a wounded man."

The Defence Act provided that all officers, irrespective of what branch of the service they may belong to, must qualify for promotion up to and including the rank of lieutenant-colonel, and it is therefore incumbent for medical officers to submit themselves for examination in those subjects connected with the military conduct of a medical service as have been duly defined in the syllabus of examination for each rank.

Officers joining the Australian Army Medical Corps did so with the rank of lieutenant, and served three years in that position ; but the Military Board have lately decided that they shall in future enter the service with the rank of captain on their first commission, to be provisional, and confirmed on passing the requisite examination of the rank within a specified time.

It is maintained that this will do much to popularise the medical service amongst the profession, and hold out an inducement for applications for commissions in the several States.

15. This brings us now to that most vital point—how can we expand our medical services from a peace to a war footing ? In this we have to deal not only with conditions which do not exist in peace—the formation of hospitals at the base, on lines of communication, and in fortress areas, hospital trains, advanced depots of medical and surgical stores, and may be hospital ships, also with items of equipment and transport—but with the very difficult question of *personnel*, both officers and men. Officers have much to learn in the military conduct of their duty, and men cannot be trained in a week or a month. The training of a first-class hospital orderly cannot be attained in a year.

It would be beyond the power of any country to keep a highly trained and completely equipped medical service up to war requirements on its military estimates, but it is within any nation's power to have its medical units so organised that necessary expansion from peace to war can rapidly and efficiently be carried out. But this must entail previous organisation, which, when set in motion, will proceed without friction, delay, or useless expenditure.

A highly-trained nucleus is essential; given this, expansion through means of a reserve of officers, and the welding into the service the *personnel* and equipment of an efficient Red Cross Society would be easily effected, and should give the most splendid results.

Let us look forward to the establishment of the Red Cross Society of Australia at no distant date.

In July last the new Red Cross Society of the United Kingdom was inaugurated at Buckingham Palace by their Majesties the King and Queen. This new Red Cross Society is not limited to the United Kingdom, but is to be an Empire movement. The object of the society will be to provide volunteer aid in time of war, on the lines laid down in the Geneva Convention. In times of peace the services of such a society could be used towards alleviating the suffering brought about by floods, fire, or any national calamity.

In time of war the society would come under naval and military control, and would provide equipment and a trained *personnel* to work side by side with army medical services.

Her Majesty in the opening proposed that—" This new organisation should be based upon membership association, and the members and associates of the society shall be recruited from all classes throughout the Empire.

" The society shall be entirely voluntary, and while in touch with the War Office and Admiralty, the society shall be organised and act wholly independently of those departments in time of peace, but naturally, in time of war, it must be under naval and military control.

" I therefore now appeal to all the women of the Empire to assist me in carrying out this great scheme, which is essentially a woman's work, and which is the only one and only way in which we can assist our brave and gallant army and navy to perform their arduous duties in time of war."

That a Red Cross Society, as indicated by the speech of Her Majesty, is not only possible, but from every aspect most desirable for our Commonwealth, seems to go without comment, and I trust in the near future that opportunity will be seized to place this magnificent movement of philanthropy on a pedestal to which all Australia will be able to look with pride and admiration.

The first step would be the formation of a central organisation: centres to be established in each State on similar lines, and a harmonious bringing into the fold of the various civil aid societies now existing amongst us.

The magnificent work carried out by the Red Cross Society of Japan during the war which has just been concluded by the signing of peace, has been an object lesson to the world, providing, as it has done, a thoroughly organised system, both as regards *personnel*, equipment, general and medical stores for a Red Cross hospital, relief detachments, rest stations, depots of supply, and transport columns for the vast armies of Japan which have been operating in the East for the past eighteen months.

It has a reserve of 3,000 trained nurses ready for immediate service.

The Japanese society was only incorporated in 1902, so that within a couple of years it has had to submit its organisation to the most severe strain that it could be called upon to perform, and it has stood every test in the most splendid manner. At the present time the society's income stands at £250,000, and the members number over a million.

I quote the above facts from that most valuable and interesting work " Under the Care of the Japanese War Office," by Miss Ethel McCaul, R.R.C., who received the sanction of Her Majesty the Queen and the permission of the Japanese Government to inspect and report on the work of the Red Cross Society of Japan.

With such an example before us, does it not behove Australia to make some move in a similar direction, and establish a society which would be of the highest national benefit to our navy and army in time of war, and which would at once allow of that expansion of the medical services without which disaster would be inevitable, and untold sufferings the dire result.

16. Having already exceeded my time limit, I will now, Mr. Chairman and gentlemen, bring this paper to a close, with much regret that it has been so sketchy, but with the hope that it may afford some insight into what our Medical Service is, and what we should look forward to for the attainment of the high efficiency it demands, and its expansion, which would be such an urgent necessity in time of any national emergency.

Appendix No. 1.

AUSTRALIAN ARMY MEDICAL CORPS.

The Australian Army Medical Corps will be organised for administrative purposes into companies as follows :—

I. Mounted companies consisting of—

One mounted bearer company and half field hospital for service with Light Horse Brigades (Tables XIX. and XXIII.).

II. Infantry companies consisting of—

One infantry bearer company and one field hospital for service with Infantry Brigades (Tables XXI. and XXIII.).

III. Garrison companies of varying establishments to suit local requirements.

Table XIX.—Peace Establishment of a Mounted Bearer Company for Service with Brigade of Light Horse.

Ranks.	Personnel.						Horses.			Vehicles.		
	Officers.	Warrant Officers.	Staff Sergeants and Sergeants.	Buglers.	Rank and File.	Total.	Riding.	Draught.	Total.	Carts, Transport.	Wagons, Ambulance.	Total.
Medical Details.												
Captain or Lieutenant	1	—	—	—	—	1	1	—	1	—	—	—
Staff Sergeant	—	—	1	—	—	2	1	—	1	—	—	—
Sergeant	—	—	1	—	—		1	—	1	—	—	—
Bugler	—	—	—	1	—	1	1	—	1	—	—	—
Corporal	—	—	—	—	1	16	16*	—	—	—	—	—
Privates	—	—	—	—	13			—	—	—	—	—
Private as cook	—	—	—	—	1			—	16	—	—	—
Private as batman	—	—	—	—	1			—	—	—	—	—
Total medical details	1	—	2	1	16	20	20	—	20	—	—	—
Transport Details.												
Drivers, ambulance wagons	—	—	—	—	4	5	—	8	8	—	2	2
Driver, transport cart	—	—	—	—	1		—	2	2	1	—	1
Total transport details.	—	—	—	—	5	5	—	10	10	1	2	3
Total mounted bearer company	1	—	2	1	21	25	20	10	30	1	2	3

* Includes 1 pack horse.

APPENDIX No. 1—*continued.*

PEACE ESTABLISHMENT OF A MOUNTED BEARER COMPANY, &c.—*continued.*

NOTE.—To bring up to war strength there will be required :—

Ranks.	Medical Details.	Transport Details.	Total.
Captain or lieutenant	1	—	1
Warrant officer	1	—	1
Sergeant	1	—	1
Rank and file	16	6	22
Total	19	—	25

WAR ESTABLISHMENT OF A MOUNTED BEARER COMPANY FOR SERVICE WITH BRIGADE OF LIGHT HORSE.

Ranks.	Personnel.						Horses.			Vehicles.			
	Officers.	Warrant Officers.	Staff Sergeants and Sergeants.	Buglers.	Rank and File.	Total.	Riding.	Draught.	Total.	Carts, Transport.	Carts, Water.	Wagons, Ambulance.	Total.
Medical Details.													
Captain	1	—	—	—	—	2	1	—	1	—	—	—	—
Lieutenant	1	—	—	—	—		1	—	1	—	—	—	—
Warrant officer ...	—	1	—	—	—	1	1	—	1	—	—	—	—
Staff sergeant..	—	—	1	—	—	3	1	—	1	—	—	—	—
Sergeants	—	—	2	—	—		2	—	2	—	—	—	—
Buglers	—	—	—	1	—	1	1	—	1	—	—	—	—
Corporals	—	—	—	—	2			—	—	—	—	—	—
Corporal as cook	—	—	—	—	1			—	—	—	—	—	—
Privates	—	—	—	—	26	32	32*	—	32	—	—	—	—
Private as cook.	—	—	—	—	1			—	—	—	—	—	—
Privates as batmen	—	—	—	—	2			—	—	—	—	—	—
Total medical details	2	1	3	1	32	39	39	—	39	—	—	—	—
Transport Details.													
Drivers, ambulance wagons .	—	—	—	—	8		—	16	16	—	—	4	4
Drivers, transport carts	—	—	—	—	2	11	—	4	4	2	—	—	2
Driver, water cart	—	—	—	—	1		—	1	1	—	1	—	1
Total transport details..	—	—	—	—	11	11	—	21	21	2	1	4	7
Total mounted bearer company	2	1	3	1	43	50	39	21	60	2	1	4	7

* Includes 2 pack horses.

APPENDIX No. 1—*continued.*

PEACE ESTABLISHMENT OF A BEARER COMPANY FOR SERVICE WITH INFANTRY BRIGADE.

Rank.	Personnel.							Horses.			Vehicles.			
											Wagons.		Carts.	
	Officers.	Warrant Officers.	Staff Sergeants and Sergeants.	Artificers.	Buglers.	Rank and File.	Total.	Riding.	Draught.	Total.	Ambulance.	Transport.	Water.	Total.
Medical Details.														
Major	1	—	—	—	—	—		1	—	1	—	—	—	—
Captain	1	—	—	—	—	—	3	1	—	1	—	—	—	—
Lieutenant .	1	—	—	—	—	—		1	—	1	—	—	—	—
Warrant Officer	—	1	—	—	—	—	1	1	—	1	—	—	—	—
Quartermaster-sergeant	—	—	1	—	—	—		—	—	—	—	—	—	—
Compounder	—	—	1	—	—	—	3	—	—	—	—	—	—	—
Sergeant ...	—	—	1	—	—	—		—	—	—	—	—	—	—
Bugler	—	—	—	—	1	—	1	—	—	—	—	—	—	—
Corporals ..	—	—	—	—	—	4	1	—	—	—	—	—	—	—
Privates ...	—	—	—	—	—	16		—	—	—	—	—	—	—
Private as cook	—	—	—	—	—	1		—	—	—	—	—	—	—
Privates as wagon orderlies .	—	—	—	—	—	2	26	—	—	—	—	—	—	—
Privates as batmen ..	—	—	—	—	—	3		—	—	—	—	—	—	—
Total medical details ...	3	1	3	—	1	26	34	4	—	4	—	—	—	—
Transport Details.														
Sergeants ..	—	—	1	—	—	—	1	1	—	1	—	—	—	—
Farrier-sergeant	—	—	—	1	—	—	2	—	—	—	—	—	—	—
Collarmaker-sergeant .	—	—	—	1	—	—		—	—	—	—	—	—	—
Drivers, ambulance wagons ..	—	—	—	—	—	8		—	16	16	4	—	—	4
Drivers, transport wagons	—	—	—	—	—	4	13	—	8	8	—	4	—	4
Driver, water cart	—	—	—	—	—	1		—	2	2	—	—	1	1
Total transport details	—	—	1	2	—	13	16	1	26	27	4	4	1	9
Total bearer company	3	1	4	2	1	39	50	5	26	31	4	4	1	9

NOTE.—To bring up to war strength there will be required :—

Ranks.	Medical Details.	Transport Details.	Total.
Officer	1	—	1
Staff sergeants and sergeants	2	1	3
Rank and file	29	17	46
Total	32	18	50

APPENDIX No. 1—*continued.*

War Establishment of a Bearer Company for Service with Infantry Brigade.

Ranks.	Personnel.							Horses.			Vehicles.			
											Wagons.		Carts.	
	Officers.	Warrant Officers.	Staff Sergeants and Sergeants.	Artificers.	Buglers.	Rank and File.	Total.	Riding.	Draught.	Total.	Ambulance.	Transport.	Water.	Total.
Medical Details.														
Major	1	—	—	—	—	—		1	—	1	—	—	—	—
Captains ..	2	—	—	—	—	—	4	2	—	2	—	—	—	—
Lieutenant .	1	—	—	—	—	—		1	—	1	—	—	—	—
Warrant officeer	—	1	—	—	—	—	1	1	—	1	—	—	—	—
Quartermaster-sergeant	—	—	1	—	—	—		—	—	—	—	—	—	—
Compounder	—	—	1	—	—	—	5	—	—	—	—	—	—	—
Sergeants ..	—	—	3	—	—	—		—	—	—	—	—	—	—
Bugler	—	—	—	—	1	—	1	—	—	—	—	—	—	—
Corporals ..	—	—	—	—	—	2		—	—	—	—	—	—	—
Corporals as wagon orderlies ..	—	—	—	—	—	5		—	—	—	—	—	—	—
Corporal as cook.....	—	—	—	—	—	1		—	—	—	—	—	—	—
Privates ...	—	—	—	—	—	35		—	—	—	—	—	—	—
Private as cook	—	—	—	—	—	1	55	—	—	—	—	—	—	—
Privates as wagon orderlies .	—	—	—	—	—	5		—	—	—	—	—	—	—
Privates as batmen ..	—	—	—	—	—	4		—	—	—	—	—	—	—
Privates as supernumeraries..	—	—	—	—	—	2		—	—	—	—	—	—	—
Total medical details ...	4	1	5	—	1	55	66	5	—	5	—	—	—	—
Transport Details.														
Sergeants ..	—	—	2	—	—	—	2	2	—	2	—	—	—	—
Farrier-sergeant	—	—	—	1	—	—	2	—	—	—	—	—	—	—
Collarmaker-sergeant .	—	—	—	1	—	—		—	—	—	—	—	—	—
Corporal ...	—	—	—	—	—	1		—	—	—	—	—	—	—
Drivers, ambulance wagons ..	—	—	—	—	—	20		—	40	40	10	—	—	10
Drivers, transport wagons ..	—	—	—	—	—	6	30	—	12	12	—	6	—	6
Driver, water cart	—	—	—	—	—	1		—	2	2	—	—	1	1
Drivers as supernumeraries	—	—	—	—	—	2		—	—	—	—	—	—	—
Total transport details	—	—	2	2	—	30	34	2	54	56	10	6	1	17
Total bearer company ..	4	1	7	2	1	85	100	7	54	61	10	6	1	17

APPENDIX No. 1—*continued.*

Peace Establishment of a Field Hospital of 100 Beds.

Ranks.	Personnel.							Horses.			Vehicles.		
	Officers.	Warrant Officers.	Staff Sergeants and Sergeants.	Artificers.	Buglers.	Rank and File.	Total.	Riding.	Draught.	Total.	Wagons, Transport.	Carts, Water.	Total.
Medical Details.													
Major	1	—	—	—	—	—	3	1	—	1	—	—	—
Captain	1	—	—	—	—	—		1	—	1	—	—	—
Lieutenant	1	—	—	—	—	—		1	—	1	—	—	—
Chief wardmaster	—	1	—	—	—	—	1	1	—	1	—	—	—
Steward*	—	—	1	—	—	—	2	—	—	—	—	—	—
Compounder	—	—	1	—	—	—		—	—	—	—	—	—
Corporal as clerk	—	—	—	—	—	1	15	—	—	—	—	—	—
Corporal as cook	—	—	—	—	—	1		—	—	—	—	—	—
Private as cook	—	—	—	—	—	1		—	—	—	—	—	—
Privates as ward orderlies	—	—	—	—	—	8		—	—	—	—	—	—
Private as washerman	—	—	—	—	—	1		—	—	—	—	—	—
Privates as batmen	—	—	—	—	—	3		—	—	—	—	—	—
Total medical details	3 —	1	2	—	—	15	21	4	—	4	—	—	—
Transport Details.													
Sergeant	—	—	1	—	—	—	1	1	—	1	—	—	—
Corporal	—	—	—	—	—	1	8	1	—	1	—	—	—
Drivers, transport wagons	—	—	—	—	—	6		—	12	12	6	—	6
Driver, water cart	—	—	—	—	—	1		—	1	1	—	1	1
Total transport details	—	—	1	—	—	8	9	2	13	15	6	1	7
Total field hospital	3	1	3	—	—	23	30	6	13	19	6	1	7

Note.—To bring up to war strength there will be required :—

Ranks.	Medical Details.	Transport Details.	Total.
Officers	2	—	2
Staff sergeants and sergeants	5	1	6
Artificer	—	1	1
Rank and file	15	6	21
Total	22	8	30

* Also perform duties of quartermaster-sergeant.

APPENDIX No. 1—*continued.*

WAR ESTABLISHMENT OF A FIELD HOSPITAL OF 100 BEDS.

Ranks.	Personnel.							Horses.			Vehicles.		
	Officers.	Warrant Officers.	Staff Sergeants and Sergeants.	Artificers.	Buglers.	Rank and File.	Total	Riding.	Draught.	Total.	Wagons, Transport.	Carts, Water.	Total.
Medical Details.													
Major	1	—	—	—	—	—		1	—	1	—	—	—
Captains	2	—	—	—	—	—		2	—	2	—	—	—
Lieutenant	1	—	—	—	—	—	5	1	—	1	—	—	—
Quartermaster ..	1	—	—	—	—	—		1	—	1	—	—	—
Chief wardmaster	—	1	—	—	—	—	1	1	—	1	—	—	—
Assistant ward-master	—	—	1	—	—	—		—	—	—	—	—	—
Steward	—	—	1	—	—	—		—	—	—	—	—	—
Compounders ...	—	—	2	—	—	—		—	—	—	—	—	—
Cook	—	—	1	—	—	—	7	—	—	—	—	—	—
Pack storekeeper.	—	—	1	—	—	—		—	—	—	—	—	—
Supernumerary .	—	—	1	—	—	—		—	—	—	—	—	—
Corporal as steward	—	—	—	—	—	1		—	—	—	—	—	—
Corporal as cook .	—	—	—	—	—	1		—	—	—	—	—	—
Corporal as clerk.	—	—	—	—	—	1		—	—	—	—	—	—
Corporal as supernumerary	—	—	—	—	—	1		—	—	—	—	—	—
Privates as war orderlies	—	—	—	—	—	14		—	—	—	—	—	—
Private as cook ..	—	—	—	—	—	1	30	—	—	—	—	—	—
Private as pack storekeeper ...	—	—	—	—	—	1		—	—	—	—	—	—
Private as messenger	—	—	—	—	—	1		—	—	—	—	—	—
Privates as washermen	—	—	—	—	—	2		—	—	—	—	—	—
Privates as supernumeraries ...	—	—	—	—	—	2		—	—	—	—	—	—
Privates as batmen	—	—	—	—	—	5		—	—	—	—	—	—
Total medical details	5	1	7	—	—	30	43	6	—	6	—	—	—
Transport Details.													
Staff sergeant ...	—	—	1	—	—	—	2	1	—	1	—	—	—
Sergeant	—	—	1	—	—	—		1	—	1	—	—	—
Shoeing smith ...	—	—	—	1	—	—	1	—	—	—	—	—	—
Corporals	—	—	—	—	—	2		—	—	—	—	—	—
Drivers, transport wagons	—	—	—	—	—	10		—	20	20	10	—	10
Driver, water cart	—	—	—	—	—	1	14	—	2	2	—	1	1
Driver as supernumerary	—	—	—	—	—	1		—	—	—	—	—	—
Total transport details	—	—	2	1	—	14	17	2	22	24	10	1	11
Total field hospital	5	1	9	1	—	44	60	8	22	30	10	1	11

APPENDIX No. 2.

(A)—A Field Ambulance (Three Sections)—(1) Personnel and Horses.

Detail.	Personnel.						Horses.		
	Officers.	Warrant Officers.	Staff Sergeant and Sergeants.	Buglers.	Rank and File.	Total.	Riding.	Draught.	Total.
Detail of Field Ambulance—Bearer Division.									
Captains or subalterns	3	—	—	—	—	—	3	—	3
Sergeants	—	—	3	—	—	—	—	—	—
Buglers	—	—	—	3	—	129	—	—	—
Corporals	—	—	—	—	117*a*	—	—	—	—
Tent Division (150 *Patients*).									
Lieutenant-Colonel	*b*1	—	—	—	—	—	1	—	1
Majors	2	—	—	—	—	—	2	—	2
Captains or subalterns	3	—	—	—	—	—	3	—	3
Quartermaster	1	—	—	—	—	—	1	—	1
Sergeant-Major	—	1	—	—	—	—	—	—	—
Sergeants: Nursing duties	—	—	2	—	—	—	—	—	—
Sergeants: Stewards	—	—	2	—	—	—	—	—	—
Sergeants: Compounders	—	—	3	—	—	63	—	—	—
Sergeants: Clerks	—	—	3	—	—	—	—	—	—
Corporals: Cooks	—	—	—	—	3	—	—	—	—
Corporals: Pack storekeepers	—	—	—	—	3	—	—	—	—
Privates: Nursing duties	—	—	—	—	21	—	—	—	—
Privates: Clerks	—	—	—	—	3	—	—	—	—
Privates: Cooks	—	—	—	—	3	—	—	—	—
Privates: Washermen	—	—	—	—	3	—	—	—	—
Privates: Supernumeraries	—	—	—	—	9	—	—	—	—
Total	10*c*	1	13	3	165	192	11	—	11
Attached—Transport Details (*A.S.C.*).									
Sergeants	—	—	3	—	—	—	3	—	3
Drivers: For vehicles	—	—	—	—	41	60	—	82	82
Drivers: For spare draught horses	—	—	—	—	3	—	—	6	6
Drivers: Spare (10 per cent.)	—	—	—	—	3	—	—	—	—
Batmen	—	—	—	—	10	—	—	—	—
Total field ambulance (including attached)	10	—	16	3	222	252	14	88	102
Details left at the base (not included in above tables).									
Private (storeman) *d*	—	—	—	—	1	—	—	—	—
First re-inforcement *e*	—	—	—	—	16	17	—	—	—

a Of these 90 may be specially enlisted men. *b* Commands the unit. *c* Of these 3 may be civilian surgeons specially enlisted. *d* Will mobilise with the unit. *e* Will mobilise with the unit for service abroad.

APPENDIX No. 2.—*continued.*

(2)—TRANSPORT.

Detail.	Vehicles.	Drivers (A.S.C.).	Draught Horses.
Carts, water (1 per section)	3	3	6
Wagons, ambulance	10	20	40
Wagons, G.S. for baggage, stores, and supplies (3 per section)	9	18	36
Drivers, for spare draught horses	—	3	6
Drivers, spare	—	3	—
Total	22	47	88

NOTE.—Two field ambulances will be included in an infantry division, and one ambulance will be included in the corps troops of an army corps.

(B)—A CAVALRY FIELD AMBULANCE (TWO SECTIONS)—(1) PERSONNEL AND HORSES.

Detail.	Personnel.						Horses.		
	Officers.	Warrant Officers.	Staff Sergeants and Sergeants.	Buglers.	Rank and File.	Total.	Riding.	Draught.	Total.
Details of Field Ambulance—Bearer Division.									
Captains or subalterns	2	—	—	—	—		2	—	2
Buglers	—	—	—	2	—	42	—	—	—
Corporals	—	—	—	—	4		—	—	—
Privates	—	—	—	—	34*a*		—	—	—
Tent Division (50 *Patients*).									
Lieutenant-Colonel or majors	2*b*	—	—	—	—		2	—	2
Captains or subalterns	2*c*	—	—	—	—		2	—	2
Sergeant-Major	—	1	—	—	—		1	—	1
Sergeants: Nursing duties	—	—	2	—	—		—	—	—
Sergeants: Stewards	—	—	2	—	—		—	—	—
Sergeants: Compounders	—	—	2	—	—		—	—	—
Sergeants: Supernumerary	—	—	1	—	—	34	—	—	—
Corporals: Cooks	—	—	—	—	2		—	—	—
Corporals: Pack storekeepers	—	—	—	—	2		—	—	—
Privates: Clerks	—	—	—	—	2		—	—	—
Privates: Nursing duties	—	—	—	—	10		—	—	—
Privates: Cooks	—	—	—	—	2		—	—	—
Privates: Washermen	—	—	—	—	2		—	—	—
Privates: Supernumeraries	—	—	—	—	2		—	—	—
Total	6	1	7	2	60	76	7	—	7
Attached—Transport Details (*A.S.C.*).									
Sergeants	—	—	2	—	—		2	—	2
Drivers: For vehicles	—	—	—	—	32		—	64	64
Drivers: For spare draught horses	—	—	—	—	2	44	—	4	4
Drivers: Spare, 10 per cent.	—	—	—	—	2		—	—	—
Batmen	—	—	—	—	6		—	—	—
Total field ambulance, including attached	6	1	9	2	102	120	9	68	77
Details left at the base (not included in above totals).									
Private (storeman) *d*	—	—	—	—	1	—	—	—	—
First re-inforcement *e*	—	—	—	—	6	7	—	—	—

a Of these 18 may be specially enlisted men. *b* The senior commands. *c* May be civil surgeons specially enlisted. *d* Will mobilise with the unit. *e* Will mobilise with the unit for service abroad.

APPENDIX No. 2—*continued.*

(2)—TRANSPORT.

Detail.	Vehicles.	Drivers, A.S.C.	Draught Horses.
Carts, water (1 per section)	2	2	4
Wagons — Ambulance — six-horsed (3 per section)	6	18	36
Wagons — Ambulance — two-horsed, light (2 per section)	4	4	8
Wagons — G. S. for baggage, stores, and supplies (2 per sec.)	4	8	16
Drivers, for spare horses (draught)	—	2	4
Drivers, spare	—	2	—
Total	16	36	68

NOTE.—A cavalry field ambulance will be included in a cavalry brigade.

INDEX.